59

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL

REVIEW

OR

QUARTERLY JOURNAL

OF

PRACTICAL MEDICINE AND SURGERY.

VOL. XVIII.

JULY—OCTOBER, 1856.

LONDON:

JOHN CHURCHILL, NEW BURLINGTON STREET.

MDCCCLVI.

LONDON:
SAVILL AND EDWARDS, PRINTERS, CHANDOS STREET,
COVENT GARDEN.

CONTENTS OF No. XXXV.

OF THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

JULY, 1856.

Analytical and Critical Reviews.

§

Bibliographical Record.

Original Communications.

Chronicle of Medical Science.

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

JULY, 1856.

PART FIRST.

Analytical and Critical Reviews.

REVIEW I.

1. *Lectures on the Comparative Anatomy and Physiology of the Invertebrate Animals.* Delivered at the Royal College of Surgeons. By RICHARD OWEN, F.R.S., Hunterian Professor to the College. Second Edition. Illustrated by numerous Woodcuts.—*London*, 1855. 8vo. pp. 689.
2. *General Outline of the Organization of the Animal Kingdom, and Manual of Comparative Anatomy.* By THOMAS RYMER JONES, F.R.S., Professor of Comparative Anatomy in King's College, London, &c. Second Edition. Illustrated by Four Hundred Engravings.—*London*, 1855. 8vo. pp. 842.

IT is a somewhat singular coincidence, that the only two British Treatises on Comparative Anatomy which can be pointed-to as in any degree representing the present state of that science, and which made their first appearance almost contemporaneously, should have reached a second edition at the same time. The 'Lectures' of Professor Owen originally appeared as the notes of Mr. White Cooper, revised by the Professor himself; and were published in numbers as they were delivered, the completed volume bearing the date 1843. The 'General Outline' of Professor Rymer Jones also originally appeared in numbers, and the complete volume bears date 1841. Having published in the interval the first volume of his 'Comparative Anatomy of the Vertebrata'—the 'Anatomy of Fishes,'—and intending, as we trust, to complete that great work in a manner worthy of his unrivalled knowledge of Vertebrated Animals, Professor Owen has thought it desirable to re-issue his 'Lectures on the Invertebrata' in an enlarged form; adding a considerable quantity of new matter, and inserting numerous references to the original authorities for the facts and opinions advanced; and taking, of course, the entire responsibility upon him-

self. We are led to suppose that the author considers himself, in these additions, to have brought his work up to the existing state of knowledge on this subject; and he advances no reason why it should not be fairly tried by the standard which he has himself raised.—Professor Rymer Jones, on his part, after referring to some of the most important advances which have been made by Continental and British labourers in this field since the publication of his previous edition, tells us that he "has endeavoured, to the best of his ability, to keep pace with their diligence and onward progress, so as adequately to record and acknowledge their contributions to the general stock of scientific lore."

It will be our duty, and, we are sorry to say, our by no means pleasing duty, to inquire how far the additions and alterations made in these Treatises can be considered as placing them upon the level of our existing knowledge of the subject of which they treat. In doing this, however, we must limit ourselves to one particular department,—the group of animals constituting the Radiata of Cuvier;—since it will be necessary for us, in order to show our readers what are our grounds of complaint, to enter into some preliminary detail as to the present aspect of the border-ground between the Animal and Vegetable kingdoms, which will not, we hope, be unacceptable to them. It is, of course, to the first of the works before us that we shall more particularly address ourselves. Professor Owen occupies a position second to none in that department of Comparative Anatomy which he has made more particularly his own—viz., the Osteology of the Vertebrata; and considering the vast advances which the Comparative Anatomy and Physiology of the Invertebrata have made of late years, and the large number of labourers whose contributions must be individually studied in order to embody them in any general system, we opened the work with a misgiving that his acquaintance with these would be found to be far from complete, and that many old errors would be retained, many new truths passed by. It is quite enough, indeed, for one man's work, to keep pace with the rapid progress of any single department of this science; and Siebold, one of the most laborious and conscientious of continental systematic writers, as well as himself one of the most accurate and zealous of original inquirers, exercised a wise discretion in restricting himself, in the production of his admirable 'Vergleichende Anatomie,' 1848,* to the Invertebrated classes; the anatomy of the Vertebrata being undertaken by his colleague Stannius. It would be well if men of great ambition and comprehensive grasp of mind would act more upon the time-honoured adage, "*Non omnia possumus ommes.*" The authority which they acquire by their labours in one department, gives them a *prestige* in regard to any others that they may undertake to elucidate, which becomes mischievous, when, as too often happens, they do their work imperfectly by grasping at too much. Professor Owen's authority is deservedly so high on the subjects which he has made his own, that we are most desirous for his sake that he should not lower it by striving for what it is impossible in the nature of things that he can thoroughly attain; and we feel called upon to watch with a careful eye

* An American translation of this masterly work, with numerous Notes and additional References, by the late Dr. W. J. Burnett, bringing it down to 1854, is, in our opinion, by far the best Treatise on Invertebrate Anatomy, that the English student can have recourse to.

that it be not employed to the positive detriment of science, in perpetuating error and retarding the progress of truth.

The aim of Professor Rymer Jones's treatise is less high, and his authority is of less weight; but from its popular style, the generally-good selection of its subjects, and the beauty of its illustrations, it has acquired a reputation amongst students, of which we should gladly see it rendered more worthy.

A good-natured criticism of such works, in which their excellences alone should be dilated-on, and their defects altogether passed by or scarcely noticed, would be much more agreeable to ourselves, as well as more palatable to the subjects of it. But we must speak out as the interests of truth and justice seem to us imperatively to demand, whatever be the cost to ourselves.—With these preliminary remarks, we enter upon that general survey of the lowest forms of Animal life, which will enable us to inquire how far their nature and meaning have been understood by our authors.

The association of a large assemblage of these forms under the designation *Protozoa*, as first proposed (we believe) by Siebold, has come to be very generally accepted among the Zoologists and Physiologists of Germany, although there is not yet a complete accordance as to the definition of the group, and the range of forms which it should include. The definition given by Siebold stands as follows:—"Animals in which the different systems of organs are not distinctly separated, and whose irregular form and simple organization are reducible to the type of a cell." The fundamental idea contained in this definition was more fully expanded by him in a valuable essay 'On Unicellular Plants and Animals,' published in the first volume of Siebold and Kölliker's 'Zeitschrift;' wherein he discusses the relations which his Protozoa bear to the *Protophyta* that constitute the parallel group in the Vegetable kingdom, and inquires into the validity of the characters which have been assigned as the basis of their separation. Although the correctness of many of the details contained in that essay has been disproved by subsequent research, and although the general doctrine of *cells* in vogue at the time of its production has been conclusively shown to require revision,* yet the fundamental idea still remains unshaken—viz., that there is a division of the Animal kingdom, among the members of which there is no more differentiation of organs than there is in the simplest Plants, and which in this respect correspond to the earliest embryonic states of the higher animals. Whether this division may be fairly considered to have been permanently established, or whether it must be regarded for the present as provisional only, is a question which we shall be in a better position to discuss, when we shall have examined some of the principal facts that bear upon it.

The general result of recent microscopic investigation, in regard to the lowest forms of Vegetable and Animal life, seems to us to lead to this conclusion—that organisms may possess an independent existence, may go through all the phenomena of growth, multiplication, and reproduction, and may even possess considerable power of spontaneous motion, without having advanced even so far in the differentiation of their parts as to

* See Mr. Huxley's paper in the British and Foreign Medico-Chirurgical Review, vol. xii. p. 285.

possess those attributes which are involved in the ordinary idea of a "cell." By way of explaining our meaning, we shall select an illustration from each kingdom; and the comparison of the two will enable us to inquire in what lies the essential difference between them.

One of the humblest of known Protophytes—the *Palmoglæa macrococca* (Kützing), whose multiplication gives origin to the green slime that is found on damp stones and walls,—consists of isolated particles of a spheroidal shape and greenish colour, commonly imbedded in a stratum of gelatinous matter, which an ordinary observer would at once pronounce to be vegetable cells. But a careful examination shows that there is here no definite distinction between "cell-wall" and "cell-contents;" the whole particle being composed of a nearly-homogeneous mass of "protoplasm," through which chlorophyll-granules are dispersed. In the midst of these, however, a nucleus may be sometimes discerned; and this is usually brought into clear view by the action of tincture of iodine, which turns the nucleus dark-brown. These particles, increasing in size, undergo duplicative subdivision by the usual process of elongation and constriction; and it is observable that the nucleus gives indications of the commencement of this subdivision earlier than the particle which encloses it. Each new cell (if such it may be called) then begins to secrete from its surface a gelatinous envelope of its own; so that, by its intervention, the two are usually soon separated from one another. Sometimes, however, this is not the case; the process of subdivision being so quickly repeated, that there is not time for the production of a separate gelatinous envelope to each particle; so that a series of spheroids, hanging-on one to another, is produced. There appears to be no definite limit to this kind of multiplication; and extensive areas may be quickly covered, in circumstances favourable to the nutrition of the plant, by the products of the duplicative subdivision of one primordial cell. This, however, is simply an act of *growth*, precisely analogous to the multiplication of cells in the earliest embryonic condition of the higher Plants and Animals, before any differentiation of organs begins to show itself. And as every cell thus produced is similar to every other, and may live independently of it, such plants may still be appropriately designated as "unicellular," notwithstanding that they may be composed of large aggregations of cells connected by a gelatinous matrix, instead of being mere agglomerations of cells completely isolated from each other. The Palmoglæa not only thus grows and multiplies, but it also performs what is now coming to be generally recognised as a true *generative* process; which takes place, as might be expected, on the simplest of all types. For this process consists in the *conjugation* of any pair of cells, the substance of the two undergoing a complete mutual fusion, which is not obstructed by the intervention of any limiting membrane; the communication is usually made at first by a narrow neck or bridge, and gradually extends through a large part of the contiguous boundaries, until at last the whole of each particle is involved in it. A "spore" is thus formed, which is the "primordial cell" of a new generation, and which gradually evolves itself into an aggregation resembling that out of which it arose, by a renewal of the process of duplicative subdivision. This spore is something very different, both in aspect and in composition, from the body

that would be produced by the mere coalescence of two particles; for the green granular matter disappears, its place being taken by oil-particles, which are at first small and distant, but gradually become larger and coalesce so as to form oil-drops; and the colour of the body changes, with the advance of this process, from green to a yellowish-brown. When the spore begins to vegetate, on the other hand, producing a pair of new cells by binary subdivision, a converse change occurs: for the oil-globules disappear, and green granular matter takes their place, whereby the ordinary colour of the plant is restored. This is precisely analogous to what occurs in the maturation and germination of the seed among higher plants; and the analogy is rendered yet more complete by the fact, that the spore, like the seed, is capable of remaining dormant for an unlimited period, when deprived of moisture.

Now for such a mass of protoplasm to become converted into what is ordinarily regarded as the type of the Vegetable cell, a series of changes must take place in it, involving a differentiation between the cell-wall and the cell-contents; and this involves, on the one hand, a greater consolidation of the external layer of the protoplasm, and a more complete liquefaction of its internal portion. The membrane that is first formed, which has been termed by Mohl the "primordial utricle," is identical in composition with the albuminous protoplasma, as is shown by the effects of re-agents; and it does not always seem distinctly separable from the layer of protoplasm by which it is lined. Some recent Vegetable Physiologists, indeed, question its proper existence, affirming that it is merely the superficial layer of protoplasm, more tenacious than the rest. But to us it appears that, looking to the origin and nature of this membrane, the question is simply one of *degree* of differentiation. When the external layer, call it what we may, has such a tenacity that the substance of two cells brought-together cannot coalesce without a rupture of this integument, we must call it a membrane, even though it may differ but very little from the viscid matter it surrounds.—The typical cell, if isolated, subsequently acquires a complete envelope of cellulose, secreted from the surface of the primordial utricle; but this, which is commonly known as the cell-wall in Vegetable cells, seems to have no other than a protective function; and where the cells are packed closely into a parenchyma, their cellulose-walls coalesce, like the gelatinous envelopes of the particles of Palmoglæa which are homologous with them, so that the boundaries of those proper to individual cells cannot be distinguished. Now whilst this process of consolidation is taking-place externally, a reverse change, that of liquefaction, is in progress within. This commences by the formation of *vacuoles* in the substance of the protoplasm; these, however, not being empty spaces, but cavities filled with a fluid more watery than the protoplasm. These "vacuoles" increase in number and in size, the smaller ones coalescing to form larger; and at last they come to occupy nearly the whole interior of the cell, the primordial utricle being still lined by a layer of viscid protoplasm, to which the colouring matter is usually in great degree restricted, although this is sometimes diffused through the whole cell-contents. And thus the typical Vegetable cell comes to consist of—1. The cellulose wall; 2. The primordial utricle; 3. A layer of protoplasm in contact with it; 4. The

watery cell-sap of the interior; and 5. The nucleus, usually imbedded in the protoplasm-layer;—these parts being developed, by a process of gradual "differentiation," out of a minute mass of protoplasm, in which the nucleus was the only part to be separately distinguished.

The successive stages of this formation may be best traced-out by careful observation of the process of cell-growth in the higher Algæ; but the study of the development of new organs in Phanerogamic Plants leads to the same conclusions; and the results at which Mr. Wenham* has lately arrived, from observations chiefly made on the newly-imported aquatic weed, *Anacharis alsinastrum*, are so instructive that we shall subjoin a brief summary of them. He finds that when a new leaflet is being formed from the main stem, it commences, not (as is commonly supposed) in a single cell, but in the simultaneous development of some hundred at once, which make their first appearance in the midst of a mass of protoplasm which is enclosed in a membrane that subsequently seems to become the epidermis of the leaf. This mass is at first homogeneous; but it is soon seen to contain a multitude of cavities of irregular size and shape, filled with liquid, whilst the protoplasm between these becomes more viscid. The number of these is often increased, and their size rendered more uniform, subsequently to their first formation, through the division of the larger cavities into two by the interposition of a narrow bridge of protoplasm, or into three by the interposition of a broader bridge, in the substance of which another cavity developes itself; whilst new cavities appear wherever there is any considerable accumulation of protoplasm not already hollowed-out. These cavities are next observed to be lined with a definite membrane; and within this, protoplasm, chlorophyll, and cyclosis-currents subsequently become distinguishable.

On the importance of the independent support afforded by these observations to the doctrine of Mr. Huxley already referred-to, it is quite unnecessary for us to enlarge; and we shall only remark that not only are we fully satisfied of the competence and fidelity of Mr. Wenham as an observer, but his view harmonizes with a number of facts which have fallen under our own cognizance, and which the ordinary doctrines of cell-development have not served to explain.

Turning now to the Protozoa, we find in the *Amœba* and in the *Actinophrys*, types of animal existence, which, in so far as we are yet acquainted with them, may be legitimately ranked on the same level as the Palmoglæa, although placed on the other side of the boundary line, for reasons which will presently be apparent. The body of each of these creatures is a minute mass of a substance which long since received from Dujardin the appropriate name of "sarcode," and which seems to be the equivalent of the protoplasma of the Protophyta; resembling it very closely in chemical composition and in general attributes, but being endowed in addition with a high degree of contractility. The body is not enclosed, in either of these beings, by a distinct limitary membrane, although the outer stratum of the sarcode obviously possesses more consistence than its inner part, the latter being semifluid. Vacuoles or clear spaces are seen in various parts of the sarcode-body; and in these are very commonly observable alimentary particles, introduced in the way to be presently

* Transactions of the Microscopical Society, 1856, p. 1 et seq.

described. Besides these vacuoles, a "contractile vesicle," which pulsates at tolerably-regular intervals, is always to be distinguished; sometimes in the interior of the body, sometimes near its surface, and sometimes projecting above its surface. The chief difference between Amœba and Actinophrys, which agree in the foregoing particulars, lies in the nature of the changes of form which both of them exhibit, and in the mode in which food is received through their means. In the Amœba, the contour of the whole body is continually undergoing change; for the shapeless mass puts-forth one or more finger-like prolongations, which are simply extensions of its sarcode-substance in those particular directions; and a continuation of the same action, first distending the prolongation, and then (as it were) carrying the whole body into it, causes the entire mass to change its place. After a short time, another prolongation is put forth, either in the same or in some different direction; and the body is again absorbed into this. When the creature, in the course of its progress, meets with a particle capable of affording it nutriment, its sarcode-body spreads itself over this, so as to receive it, through any part of its parietes, into some of the vacuoles in its interior; a sort of stomach being thus extemporized, within which the alimentary particle undergoes a sort of digestion, the nutrient material being extracted by the enveloping sarcode, and any indigestible part making its way to the surface, and finally escaping through any part of it with which it happens to be in proximity. The form of the Actinophrys, on the other hand, never seems to depart widely from the globular; but its sarcode usually extends itself into a great number of contractile filaments, termed *pseudopodia*. The number and length of these, however, are continually varying; and sometimes they entirely disappear, in which case the animal cannot be certainly distinguished from an Amœba, until it begins again to put them forth. It is by the agency of these filaments, that the food of the creature is obtained; for whilst the body remains at rest, the pseudopodia act the part of the tentacula of the Hydra, being so many fishing-lines which are ready to entrap any suitable particles that may come in their way; and not merely comparatively inert and lowly-organized beings, but various small animals of great activity as well as high organization, are thus laid-hold of. When any such body happens to come into contact with one of the pseudopodial filaments, this usually retains it by adhesion, and forthwith begins to retract itself; as it shortens, surrounding filaments also apply themselves to the captive particle, bending their points together so as gradually to enclose it, and then themselves shortening progressively until the prey is brought to the surface of the body. That the threads of sarcode of which the pseudopodia are composed, are not furnished (any more than the body itself) with an investing or limitary membrane, is shown by their complete coalescence or fusion with each other, when they happen to come into mutual contact. The food thus drawn to the surface of the body by the contractility of its pseudopodial extensions, is introduced into its substance by the continuance of the same kind of operation, and gradually passes from its peripheral to its central part, where its digestible portion undergoes solution, the indigestible part (such as the shell of a minute Crustacean, or the hard case of a Rotifer) finding its way out, as in the Amœba, through any part of the surface of the body.

Thus in these creatures, although they have neither digestive cavity, mouth, nor anus,—although they are to all appearance nothing else than particles of animated jelly not even confined within a definite membrane, —the prehension and ingestion of food, the extraction of its nutritive portion by a digestive process, and the rejection of what cannot be thus reduced, by an act of defecation, are performed as characteristically, and in reality as perfectly, as in the highest animals. They multiply, however, after the manner of Protophytes, by self-division; and it has been found, in the Amœba, that portions separated from the sarcode-body, either by cutting or tearing, can develope themselves into independent beings. It has been thought, too, that, as in the Protophyta, their generative function consists in an act of "conjugation;" but recent observations have shown that it is by no means unfrequent for two, three, or even more individuals to coalesce together for a time, without the formation of any product at all analogous to the vegetable spore; the compound body afterwards separating again into detached individuals, which do not, however, always present the relative sizes they had before the occurrence of this curious fusion. Hence we must confess ourselves ignorant at present of this essential part of the life-history of these Protozoa; and neither can it be predicated in what their Generative operation is likely to consist, nor have we any idea of the nature of its product. It is quite possible, from the analogy of other low forms of animal organization, that after multiplying almost indefinitely in the Amœba- or the Actinophrys-form, some entirely different form may evolve itself, with which we may be already acquainted, though without entertaining the least suspicion of its relationship to this group; by this the generative operation may be performed, and its first products may be Protozoa of the one or of the other kind respectively. Or it would also be consistent with what we see elsewhere, that this generative act should be performed in the Amœba- or the Actinophrys-condition, and that its product should be an animal of some very different organization, which in its turn reproduces the Amœba- or Actinophrys-type by an act of gemmation. We dwell upon the deficiency of our knowledge on this point, and on the possible contingencies of the solution, to show how little we yet know about these curious creatures; and thus, on the one hand, to prevent their place in the scale from being considered as definitely fixed, and, on the other, to stimulate and direct further observation.

If, now, we compare an *Amœba* or an *Actinophrys* in its quiescent state, with a *Palmoglœa*, or any equally simple Protophyte, we can scarcely assign any *structural* characters by which one could be differentiated from the other. But when we look at their *physiological* actions, how wide is the distinction. The Protophyte, like the Phanerogamic plant, obtains the materials of its nutrition from the air and water that surround it, and possesses the marvellous power of detaching oxygen, hydrogen, carbon, and nitrogen from their previous binary combinations, and of uniting them into chlorophyll, starch, albumen, and other ternary and quaternary combinations: but the Protozoon, in common with the highest members of the Animal kingdom, is (to all appearance) destitute of any such combining power; and is consequently dependent for its support upon organic substances previously elaborated by other beings; so

that it must in the end derive its sustenance, directly or indirectly from the Vegetable kingdom. Again, the Protophyte obtains its nutriment by the absorption of liquid and gaseous molecules, which penetrate its body by simple imbibition: whilst the Protozoon, though destitute of any permanent mouth, stomach, intestine, or anus, extemporizes (so to speak) all these organs for itself whenever there is occasion, ingests solid particles into the interior of its body, and there subjects them to a regular digestive process. But further, the Protophyte in its ordinary condition is motionless; and although many of the aquatic forms pass through a motile stage, this seems to have reference simply to their dispersion, and depends merely upon the rhythmical vibrations of ciliary filaments with which they are endowed in that phase of their lives: whereas the movements of the Protozoa which we have described, bear a much closer resemblance to those of the higher Animals, being executed by changes of shape in the general contractile substance of the body, and are subservient to the acquisition of food.

Thus, then, by attending to the nature of their food, the mode of its introduction, and the character of their respective movements, a line of distinction may be drawn between the Protophyte and the Protozoon, scarcely less definite than that which separates the insect from the plant whose leaves it devours, or the elephant from the tree on whose tender shoots it browses.

But although our fundamental idea of a Protozoon should be based on such examples as the preceding, yet it must be capable of extension, so as to comprehend a much wider range of forms and conditions than are displayed in the Amœba and the Actinophrys. These are, in fact, the types of a group to which the name of *Rhizopoda* was first assigned by Dujardin, and which has been proved, by recent discoveries, to have been of no mean importance in former ages of the earth's history, though now comparatively insignificant. For, as was long since asserted by Dujardin, we are not only to rank under this head the comparatively few and minute forms which inhabit fresh water, but the vast class of *Foraminifera;* whose beautifully-regular chambered shells had not unnaturally suggested the idea of their Nautiloid affinities to such as were unacquainted with the organization of their soft parts; which had been pulled-down by Ehrenberg from the rank of cuttle-fish (assigned them by D'Orbigny) to that of polypes; but which have now been finally demonstrated, by the concurrence of microscopic observations made upon the living animals, and more especially by the admirable researches of Professor Schultze,* to be true Rhizopods. They are distinguished from ordinary Rhizopods, however, by their possession of the power of forming calcareous envelopes, of which the successive segments produced by gemmation remain for the most part attached to one another, and thus give origin to shells, whose forms will vary according to the plan on which the segments increase, but are nearly always characterized by a symmetry and beauty that become most marvellous when it is remembered that they originate from minute particles of animated jelly. That the vast multiplication of the minuter forms of Foraminifera, in the seas of the Cretaceous epoch, contributed largely towards that accumulation of white mud at their

* Ueber den Organismus der Polythalamien (Foraminiferen). Leipzig, 1854.

bottom, which constitutes what we now know as Chalk, there can be no doubt whatever; although it would probably be too much to affirm (as some have done) that Chalk is entirely or even chiefly formed of their remains. But this group appears to have attained its greatest development early in the Tertiary period, to which are restricted by far the greater part of its larger forms, and in which its structural types seem the most complete and most strongly marked. The Nummulites, Orbitolites, and Orbitoides, which have now almost entirely disappeared from our ocean-waters, must then have been among the most numerous and the most widely-diffused of all forms of marine life; for a vast band of "Nummulitic limestone"—through nearly the whole of which these three types present themselves, blended in various proportions,—may be traced from the Atlantic shores of Europe and Africa, through Western Asia to Northern India, and thence to the Pacific shore of China, often 1800 miles in breadth, and frequently of from 1500 to 2000 feet in thickness; and a similar formation prevails likewise over vast areas of North America. This Nummulitic limestone does not merely *contain* Nummulites, &c., but is in general almost entirely (if not completely) made-up of them; the matrix or rock-substance in which the recognisable specimens are imbedded, being usually composed (as microscopic examination of their sections demonstrates) of the comminuted particles of similar organisms, with which smaller Foraminifera are intermingled.

Another development of the Rhizopod type seems to be presented to us in the *Polycystina;* a group of minute animals distinguished for the most part by the remarkable forms and elaborately-worked aspect of their siliceous casings. These, although occasionally met-with in the existing seas, are chiefly known to Microscopists (for whom they furnish a set of marvellously-beautiful objects) by that vast aggregation of their fossilized exuviæ, which was discovered a few years ago in Barbadoes, by Sir Robert Schomburgk. The recent observations of Professor J. Müller upon living specimens of Polycystina, seem to leave no doubt as to the close relation of the animals which form them to those of the Foraminifera.

But the most remarkable modification of this type is presented in the *Sponge*-tribe; which seems to us to have been clearly proved by recent investigations into its minute structure and developmental history, not only to be unquestionably animal in its nature, but also to form the connecting link between the Protozoa and the Polypifera. For the soft flesh with which the skeleton of the Sponge is clothed, has been found to consist of an aggregation of Amœba-like bodies; some of which are furnished with long cilia, by whose agency those currents are kept-up, which were long since observed by Dr. Grant to be continually traversing the passages and canals of the entire mass. And from the recent observations of Mr. Carter and others upon the early development of Sponges, it appears that they begin life as solitary Amœbæ; and that it is only in the midst of aggregations formed by the multiplication of these, that the characteristic sponge-structure makes its appearance. The formation of spicules is the first indication of that organization which makes the Sponge-body one whole; and these appear to originate in the calcification or silicification (as the case may be) of particular cells, or rather segments of sarcode, a distinct animal basis being found to remain when the mineral

matter of the calcareous spicules has been dissolved away by an acid. The transition between Foraminifera and Sponges is much less abrupt than at first sight appears; for among the lower forms of the first of these groups, there are some which may be described as discoidal masses of sarcode traversed by a reticulated calcareous skeleton, and only wanting a system of pores and canals to be true Sponges; whilst in certain Sponges the ordinary fibrous skeleton strengthened with spicules, is replaced by a continuous mineral reticulation. And a remarkable connecting link between the two seems to be presented in the curious *Thalassicola*, first discovered by Mr. Huxley,* and since observed by Professor Müller, which is considered by the latter as also having relations with the Polycystina. On the other hand, the passage between Sponges and the Alcyonian Zoophytes has always appeared to us to be clearly established by those intermediate forms, in which the existence of polype-mouths seems quite subordinate to that of the general spongioid body; and especially by the fact long since announced by Professor Milne-Edwards, that in the new offshoots of certain Alcyonians, the spongioid body is developed, with its system of ramifying canals, before any polypes make their appearance at their orifices.

We have not yet done with this Rhizopod type of life. For there now appears to be no doubt, that we are to associate with it the curious *Gregarina*, whose place in the scale has of late been a subject of no little controversy in Germany. Considered in reference to its habitat, this creature is an Entozoon; for it is found exclusively in the intestinal canals of other animals, being almost invariably present in the Earth-worm, very common in Insects, and occurring also in Mollusks and Fishes. Each individual essentially consists of a single cell, more or less ovate in form, and sometimes considerably elongated; a sort of beak or proboscis frequently projects from one extremity; and in some instances this is furnished with a circular crown of hooklets closely resembling that which is seen on the head of Tænia. The Gregarina exhibits a decided advance in grade of development as compared with Amœba; for the cell-wall is quite distinct from the cell-contents, the former being a pellucid membrane, whilst the latter consist of a milk-white fluid, usually minutely-granular, in the midst of which a pellucid nucleus is commonly to be seen. The membrane with its contents, except the nucleus, are soluble in acetic acid. This animal does not put forth digitate extensions like the Amœba, nor radiating pseudopodia like the Actinophrys; but it possesses contractility enough to be the subject of considerable changes of form, by which it executes movements of progression. In regard to its reception of food, it is conformable to the type of the Cestoid Entozoa; for these have no proper digestive cavity, and obtain their nourishment by the absorption of the juices in the midst of which they live, through the whole of their permeable surface; and it appears to be for the purpose of renewing the stratum of fluid in contact with that surface, that it is clothed with cilia. Thus, whilst not less dependent than the true Rhizopoda, upon nutrient material previously elaborated by other living organisms, the Gregarina does not perform the ingestive and digestive process which is so remarkable a feature in their life-history; this being

* Annals of Natural History, second series, vol. viii. p. 433.

rendered unnecessary in them, as it is in the Cestoid Entozoa, by the state of preparedness of the fluids they imbibe, which have been digested for them (so to speak) by the animal whose intestinal canal they infest. The multiplication of the Gregarina is sometimes effected by the simple act of self-division; but sometimes by a process which seems analogous to the formation of "zoospores" among the Protophyta. The granules dispersed within the cell aggregate into corpuscles, which, at first spherical, afterwards become boat-shaped, a large number being thus formed within each Gregarina-body. These corpuscles, at first designated "pseudo-naviculæ" from their shape, but now more commonly known as "psorosperms," are set free by the rupture of their parent-cyst; and they have been found, by the recent researches of Dr. Nathaniel Lieberkühn, to develope themselves first into Amœba-like bodies, from which Gregarinæ are subsequently evolved.* A sort of "conjugation" has been seen to take place between two individuals, whose bodies, coming in contact with each other by corresponding points, first become more globular in shape, and are then encysted by the formation of a capsule around them both; the partition-walls between their cavities disappear; and the substance of the two bodies becomes completely fused together. This conjugation, however, can scarcely be regarded as having any more significance as a true generative process, than the fusion of two or more bodies of Amœba or Actinophrys; since its products do not seem to differ in any respect from those which may be formed without conjugation in the interior of a single Gregarina-cell. Hence it seems clear that we do not yet know the whole of the life-history of this curious creature; and it is quite possible that, in common with Amœba and Actinophrys, it may give origin to some very different form.

In respect to its distinct cell-wall, and to the more complete limitation of the body which it affords, Gregarina may be considered as in some sort establishing a passage towards the group of *Infusoria* proper; the distinguishing character of which is, that the sarcode-body is included within a well-defined membrane, and that this membrane has a definite oral aperture, through which alimentary particles are introduced, with a separate anal orifice in many instances, through which the rejectamenta can be got rid of. By most of the German Microscopists, the Infusoria are considered as single cells, chiefly (as it would seem) on the ground of a "nucleus," or what appears to be such, being present in each,—notwithstanding the existence of the oral and anal openings into their interior, and the frequent presence of organs with which it is difficult to conceive of single cells being endowed. The group of Infusoria, as thus characterized (we adopt Siebold's limitation of it, excluding his order *Astoma*, of whose vegetable nature subsequent research leaves scarcely a doubt), is far less comprehensive than that of the so-called Polygastrica of Professor Ehrenberg. For it is now quite certain that among these were ranked a large number of forms belonging to the Vegetable kingdom; to which the progress of inquiry is continually adding. Thus of the vegetable nature of the group of *Desmidiaceæ* we believe that no unprejudiced observer would now entertain a doubt; for all their characters are such

* See his account of the Evolution of Gregarinæ, in Mém. de l'Acad. Roy. de Belgique, tome xvi., and subsequent notices in the Bulletin of the same Academy, tome xxi. Nos. 3 & 7.

as would lead us to associate them with Protophytes, no single attribute of animality unequivocally existing among them. And although opinions are less unanimous with respect to the *Diatomaceæ*, the preponderance is now decidedly in favour of their affinity to Desmidiaceæ. That almost everything which Professor Ehrenberg has affirmed of their organization is untrue, is the unanimous verdict of the many observers who have within recent years devoted themselves to their study. They seem, in fact, to be nothing else than isolated cells, growing and multiplying under the same conditions as those of ordinary Protophytes, and being chiefly peculiar in the consolidation of their external coat by silex. The contents of these cells have all the essential characters of a vegetable endochrome; and there is strong reason to believe that their siliceous envelope has an organic basis of cellulose. The vegetable nature of the *Volvocineæ*, advanced as probable by Siebold, has been placed beyond a question by the researches of Williamson and Busk. And to the preceding we may now add almost with certainty all the genera included by Siebold in his order Astoma, save one or two which appear to be larval forms of some higher animals. As a striking instance of the extent, to which the careful study of the life-history of the simplest Protophytes tends to modify the doctrines to which the authority of Ehrenberg has given a temporary currency, we may advert to the case of *Protococcus pluvialis;* out of the different phases of which one Plant, according to the careful observations of Dr. Cohn, Professor Ehrenberg has constructed about forty *species* belonging to fifteen *genera* of *Animalcules.*

The limits of our space forbid us from going into any details upon the varieties of form and structure presented by the true *Infusoria;* but we shall place before our readers what we believe to be the essential facts determined by recent research, with regard to their organization and life-history. The sarcode-body, which is enclosed in a distinct membrane, has usually a tolerably-definite form, and seems itself but little endowed with contractility (save in a few exceptional cases), its movements being chiefly executed by the instrumentality of its ciliary appendages. These, whether few or many, are always so disposed as not only to be subservient to the general locomotion of the body, but also to create a current towards the oral orifice. The internal substance of the body is composed of soft sarcode, in the midst of which are seen numerous "vacuoles," and also "contractile vesicles" (two to sixteen in number) which execute rhythmical movements of contraction and dilatation at tolerably regular intervals. The alimentary particles introduced through the mouth, are commonly moulded into little aggregations of a rounded form, which are received into the vacuoles; and they are often seen to execute a sort of circulation through the cavity of the body, which seems, however, to be merely maintained by the successive introduction of new alimentary particles, each aggregation of which pushes-on its predecessors. Thus the pellets that first entered, gradually make their way to the anal orifice, yielding-up in their course their nutritive materials; or, if no such orifice exist, they either find their way back to the mouth, or (it is believed) force their way out by an extempore anus through the limitary membrane. The multiplication of Infusoria by the process of duplicative self-division, is a process that has long been familiar to Microscopists; but various other

modes of propagation have become known of late years. Thus it has been shown by Stein, Jules Haime, and others, that many Infusoria at certain times undergo an *encysting* process; the phenomena of which were completely misapprehended by Professor Ehrenberg. The Animalcule loses its activity, its form becomes more rounded, and its cilia or other filamentous prolongations are either lost or retracted. The body then secretes from its surface a sort of gelatinous case, which hardens so as completely to enclose it; the Animalcule, however, still remaining free in the midst of its coffin-like investment. This condition was not unknown to Professor Ehrenberg, who considered the encysting process as the expiring effort of life; but if the cysts and their contents be attentively watched, it will be seen to be preliminary to the production of new individuals. This production may take place in different modes. For sometimes the substance of the body appears to break up into numerous "gemmules," analogous to the "psorosperms" of Gregarina, and to the "zoospores" of Protophytes; and these, when set free by the bursting of the cyst, swim forth to develope themselves into a new brood of Animalcules of the same type with that from which they sprang, though at first perhaps bearing little resemblance to it. But in other instances, only a single offspring is developed from the nucleus of the original cell-body; which offspring may have a very dissimilar form. Thus the *Vorticella* gives origin, through this encysting process, to an *Acineta*, which is very like an Actinophrys; and this Acineta, acquiring a stalk but still retaining its general characters, assumes the form which has been distinguished as *Podophrya*. From the nucleus of this is evolved an internal bud, which gradually comes to present the form of a young Vorticella; and this, escaping from the Acineta-body by a gap formed in some part of its wall, goes forth to originate a new colony of Vorticellæ; whilst the Acineta, the gap in whose wall soon closes-up again, goes-on stretching out and retracting its radiating filaments, and after a time produces in its interior a new nucleus for a second Vorticella bud.

Neither of these processes, however, can be fairly looked-on in any other light, than as modifications of the general plan of multiplication by gemmation, which corresponds, in its essential features, with the *growth* of the higher animals. With anything that can be truly accounted their *generation*, we are yet entirely unacquainted; and it is, therefore, quite possible, that the complete life-history of Infusoria may include some phases of which we have not at present any idea. By Professor Agassiz, indeed, it has been asserted that *Paramecium* and *Bursaria*, two genera which are usually considered as among the most typical of Infusoria, are nothing else than the larvæ of Planaria; and if this were proved in regard to them, we should be disposed to regard the entire class as merely consisting of embryonic forms of higher animals. But we cannot help believing that Professor Agassiz has been misled on this point by imperfect observation; more especially since, as we have lately learned from Dr. Wagener, the Cercaria-like embryos which come forth from the ova of some Trematode worms, although so like Infusoria in their general aspect as to be readily mistaken for them, differ from them in this essential particular,—that they possess the water-vascular system characteristic of the adults, in the same rudimentary form in which it presents itself among the Rotifera.

Having thus endeavoured to place our readers *au courant* with the general state of knowledge on this subject,—which we have entered into thus fully, because it involves considerations of the highest interest and importance, alike in Physiology and in Zoology,—we have to inquire how far the mode in which it is treated in our two recent British treatises on Comparative Anatomy can be regarded as satisfactory.

Ignoring altogether the term "Protozoa," which seems to us singularly appropriate, Professor Owen ranks as a sub-province of Cuvier's Radiata, under the common designation "Infusoria," the *Rotifera*, the *Rhizopoda*, and the *Polygastria;* the Sponges being altogether left out. Now the Rotifera, since their complex organization was first (however imperfectly) made known by Professor Ehrenberg, have been ranked by all who have attentively studied them, in a far higher part of the animal series, namely, in some part of the Articulated sub-kingdom. Thus Professor Leydig, who has published a most important monograph upon this group, considers it most allied to the Crustacea, and designates it *Cilio-Crustacea.* By Mr. Huxley, again, the resemblance of certain Wheel-Animalcules to the larval forms of certain Marine Worms, and the presence of a water-vascular system in the one group as in the other, is considered (and we think with justice) as indicating that the special affinity of the Rotifera is with the Annelida. And in a recent communication to the Royal Society, Mr. Gosse has given strong confirmation to the doctrine of their Articulated nature, by showing that the parts of their curious masticating apparatus may be fairly considered as homologous with the buccal apparatus of Mandibulate Insects. We looked with some interest, therefore, to Professor Owen's account of this group; expecting to find him assigning some reasons for still keeping it under the Radiated sub-kingdom, and for degrading it to the lowest of the provinces into which he divides this; but have found none whatever. For anything that he tells us, the student would be left in utter ignorance of the general doctrine of the best-informed Naturalists on this point, and would be not a little surprised and perplexed at finding the Rotifera so differently placed in almost every other modern treatise on Invertebrate Anatomy or Zoology.

The retention of the term Polygastria, as the designation of the group to which Siebold and those who follow him limit the term Infusoria, seems to us extremely undesirable, as tending to keep before the mind the *polygastric* hypothesis of Professor Ehrenberg, which has now been fully proved to have been founded upon an entirely erroneous conception of the real nature of these animalcules. Even Professor Owen speaks of this hypothesis as borne down by the weight of opposing evidence; yet he repeats some of Professor Ehrenberg's descriptions, and allows his large figures of the "Monad of Volvox," "Vorticella," and "Leucophrys," still to stare his readers in the face, as if for the very purpose of impressing his erroneous views on their minds. Further, although he adopts the term Rhizopoda, he makes no distinct separation between them and the Polygastria; the Foraminifera are not so much as mentioned; while Sponges are left, together with a number of undoubted Plants, in that limbo between the Animal and Vegetable kingdoms, "in which the character of the organized fundamental nucleated cell is retained, with comparatively little change or superaddition;"—all those recent additions to

our knowledge of them which seem to have conclusively established their title to rank as Animals, being entirely ignored.

But this is by no means all. Professor Owen has attended so little to the recent progress of inquiry upon the border-groups of the Animal and Vegetable kingdoms, that he seems utterly unaware that the place of many creatures which he continues to describe and delineate among Animalcules, has been definitely shown to be on the Vegetable side of the boundary-line; and his chapter on the Polygastria is consequently made-up of a most heterogeneous collection of "facts and figures," in which Diatomaceæ and Desmidiaceæ, Volvocineæ and Palmelleæ, are made to do duty as Animalcules, in opposition to the conclusions of the most competent among the recent investigators into their nature and history. And it is from not having applied himself to the impartial consideration of the evidence, that he raises objections to the physiological distinction which has been drawn between the two kingdoms—as we believe, upon the most satisfactory grounds. For he deems it a sufficient invalidation of the doctrine that true Plants make their own organic compounds, whilst true Animals derive theirs from bodies previously organized, to say that masses of Animalcules have been known to decompose carbonic acid and to give off oxygen, like Plants, under the influence of sun light: the fact being, that the supposed Animalcules (*Frustuliæ*, *Chlamydomonas*, *Euglena*) are the very creatures which have now been proved by *other evidence* to be really Plants; so that here, as in many other cases, *exceptio probat regulam.* —Let us compare this with another somewhat analogous instance.

Suppose that the distinguished Professor by whom the importance of the characters furnished by the minute structure of the *teeth* was first demonstrated, had been led to distrust the value of any deduction respecting the nature of a *bone* that might be drawn from its microscopic appearances, because a bone which he believed to be that of a Bird was pronounced on that authority to be that of a Reptile,—and suppose that this very bone was afterwards proved, even to the satisfaction of the Professor himself, to be reptilian,—would not the value of the microscopic test, instead of being invalidated by the supposed disproof of its reliability, be immensely raised by this evidence of its essential superiority to characters furnished by imperfectly-preserved external configuration? This, as Professor Owen well knows, is no hypothetical case; and its parallelism is obvious.

Until reliable evidence shall be offered to the contrary, therefore, we think it may be held as certain, that the peculiar attribute of the higher Plants is equally characteristic of the lower; and that whenever any aquatic organism is found to decompose carbonic acid under the influence of sun light, and to set free oxygen, that organism may be ranked as a vegetable, however active may be its movements. It may be said that this is "begging the question;" but we reply that in all the cases hitherto cited of this kind, the proof of the vegetable nature of these organisms has been drawn from independent sources. We may mention the following case in illustration, as having occurred to ourselves about ten years since. The water of a rain-water cistern, which had been thoroughly cleaned out, and which had been filled a few days afterwards by a heavy thunder-shower, was observed to present a greenish tinge; and it was

noticed that a green froth, full of minute bubbles, came to the surface whenever the sun shone on it. On examining a portion of this froth under the microscope, we found that the water was crowded with green cells in active motion; and although the only bodies at all resembling them of which we could find any description, were the so-called Animalcules constituting the genus *Chlamydomonas* of Ehrenberg, and very little was known at that time of the motile conditions of Plants of this description, yet of the vegetable nature of these organisms we could not entertain the smallest doubt. For in all their essential microscopic characters, and in their mode of multiplication, they corresponded with undoubted Protophytes; they appeared in freshly-collected rain-water, and could not, therefore, be deriving their support from organic matter; and under the influence of light they were obviously decomposing carbonic acid and setting free oxygen. Our attention was soon attracted from these little beings to an enormous swarm of Wheel-Animalcules, which soon made their appearance, and which greedily devoured their predecessors. But had we followed out their complete history, as Dr. Cohn has since done, we should have found that our Chlamydomonas was nothing else than the *motile* form of *Protococcus pluvialis*, that alternates under certain conditions with a *still* form, in which its Vegetable nature is manifested beyond a doubt.

In his general treatment of this part of his subject, Professor Rymer Jones seems to us to have a much truer appreciation of the present aspect of our knowledge of it, than is displayed by Professor Owen; but when we come to particulars, we find some very unaccountable blemishes. He adopts the designation Protozoa, and gives to the group precisely the same range as we should ourselves assign to it. But strange to say, the first order of beings described by him under this head, is that of *Spermatozoa;* which, as the history of their development and actions fully demonstrates, have no more title to be regarded as independent organisms than have blood-corpuscles or ciliated epithelium-cells. From these he proceeds to an account of Amœba and Actinophrys, and thence to the Rhizopods, Foraminifera, and Sponges; as to his description of all of which we have only to speak in terms of commendation. In his description of the so-called polygastric Infusoria, he returns to the views which he had the merit of being one of the first to promulgate, in opposition to the authority of Professor Ehrenberg, as to the non-existence of an alimentary canal and multiple pedunculated stomachs; having, in the interval between his first and second editions, avowed his conversion to the polygastric doctrine.* The general account of the group remains essentially the same as in the previous edition, the chief additions being derived from the treatise of M. Dujardin, with just enough reference to Siebold to show that he was acquainted with his views. Of everything that has been done by Stein, Cohn, Haime, and others, during the last few years, he seems to be in profound ignorance.

The next of Professor Owen's "sub-provinces" is that of *Entozoa;* which he divides, as in his former writings on that subject, into Cœlelmintha and Sterelmintha; nominally adding thereto the division Turbellaria (founded by Ehrenberg, and more precisely and completely established

* See Cyclopædia of Anatomy, vol. iv. p. 14.

by Oersted and Schultze), but in his more detailed account of it continuing to rank it with the Trematode Worms. To continue to keep the Intestinal Worms out of the Articulated sub-kingdom, with many undoubted members of which they have the closest affinity, seems to us to imply an unaccountable want of appreciation of the essential features of their organisation; and as all the most eminent Continental Naturalists agree, we believe, with Siebold and Quatrefages, in ranking the *Helminthes* (Entozoa), *Turbellaria, Rotifera,* and *Annelida,* as the Vermiform subdivision of the Articulate series, we should have been glad to learn Professor Owen's reasons for leaving them where he does. We can find no other than the filamentous character of the nervous system; a part of the organism whose completely subordinate rank in these creatures, entirely forbids, as it seems to us, any especial value being attached to such a condition as a basis of classification. Both our authors, in treating of this group, notice some of the important Continental researches, by which it has been conclusively proved that the so-called Cystic Entozoa are nothing else than abnormally-developed Cestoid Worms; but neither of them seems to be at all acquainted with the completeness which has been given to these researches during the last four or five years; for none of the more recent memoirs on this point are cited (even the important work of Van Beneden, reviewed in our tenth volume, being altogether unnoticed by Professor Owen), and the Cestoidea are still ranked in each work as a separate group, although we are told in both that they are all *probably* tænioid larvæ. We are sorry to be obliged to add, moreover, that both authors continue to repeat the erroneous statement, that the longitudinal canals of the Tænia and other Cestoid worms, represent a digestive apparatus; the fact having now been most fully substantiated (especially in the recent beautiful monograph of Dr. Guido Wagener), that these canals constitute a "water-vascular" system, as was affirmed by Siebold in 1850. And we must here add, that this system, whose proper interpretation by Siebold constitutes one of the most remarkable of all modern advances in invertebrate anatomy, and whose true import and relations are an object of special attention with every real student of Helminthology, is scarcely mentioned by name in either of our British systematic treatises.

The sub-province *Radiaria* seems intended by Professor Owen to include those of Cuvier's Radiata, in which radial symmetry is a predominating characteristic; since he associates in it the Echinodermata, Acalephæ, and true Zoophytes, still retaining in immediate connexion with the latter the Bryozoa (more properly Polyzoa), notwithstanding what he himself admits to be their strong Molluscan affinities. We are sorry to be again called-upon to remark upon certain notable omissions and errors, which detract very much from the general excellence of Professor Owen's two lectures on Polypi. Thus in treating of the sexual generation of the Compound Hydrozoa, a subject which is rendered difficult by the apparent variety of the organs by which the function is accomplished, he omits to notice Professor Allman's valuable memoir on *Cordylophora,** in which the essential conformity existing amidst all these varieties is pointed-out, in accordance with the interpretation suggested by one of the most

* Philosophical Transactions, 1853.

remarkable of their intermediate forms. Whether the reason of this silence lies in the fact, that Professor Allman takes occasion to state his accordance with us (vols. i. and iv.) in our interpretation of the so-called "Alternation of Generations," and thus implies his dissent from Professor Owen's hypothesis of "Parthenogenesis," we can of course only surmise. Again, in describing the anatomy of Actinia, he repeats the now antiquated error, that the convoluted tubes which lie in the chambers that surround the stomach are testes, and that these animals are consequently androgynous; the fact having been clearly proved, that these tubes contain "thread-cells," or "filiferous capsules," exactly resembling those of the integument, and that the so-called ovaria contain sperm-cells and spermatozoa in some individuals, and ova in others, the sexes being separate, as was shown fifteen years since by the independent researches of Kölliker and Erdl.—Like errors of omission and commission are found in Professor Rymer Jones's treatment of this part of the subject; and he seems to have made no attempt to extricate himself from that strange confusion between the different subdivisions of the Anthozoa, arising from his adherence to a principle of classification now entirely exploded, under which he laboured at the period of his first edition, and which his article Polypifera, in the 'Cyclopædia of Anatomy and Physiology,' showed to have been not cleared-up by the detailed study of the group. He separates the Bryozoa from the true Zoophytes by the interposition of the Entozoa; but he says not a word of their Molluscan relations; indeed, by ranking them between Entozoa and Rotifera, he would seem rather to regard them as having vermiform affinities. Professor Owen, however, seems fully sensible of the close approximation made by the Bryozoa to the Compound Ascidians which form the lowest step in the Molluscous series; and he justifies his wide separation of the two on the ground of what he affirms to be an essential difference in their developmental history:—"No compound Ascidian," he remarks, "quits the ovum as a gemmule swimming by means of cilia either generally diffused, or aggregated on special lobes after the type of the Rotifer," as is the case with the embryos of cértain Bryozoa; "and no Bryozoon quits the ovum in the guise of a Cercarian, to swim abroad by the alternate inflections of a caudal appendage." But does he forget that such an embryonic condition as that of the Bryozoa is characteristic of some of the most typical Mollusca; whilst the tadpole-like embryo of the Ascidians is unlike every other Molluscan embryo, its peculiar endowments being limited to that one group, and being consequently a special Ascidian, and not a general Molluscan character?

Let us see how this case stands. That Bryozoa should have been formerly ranked as Zoophytes, is not surprising, when we consider their similarity to that group in habit of life and in general aspect; but in proportion as their true structure has been disclosed by microscopic research, have their points of difference become more and more apparent, and their points of approximation to Mollusca (first pointed out by Milne-Edwards and Andouin) more clearly discernible. Thus, in the first place, all true polypes use their tentacula to grasp their food and convey it to the mouth; and if their surfaces possess cilia, these take no share in the ingestion of aliment. On the other hand, in Bryozoa, as in all Acephalous Mollusca, the nutritive particles are drawn in by a ciliary current, which also serves

to aërate the fluids. In no true polype is there a separate intestine and anal orifice, nor does the digestive apparatus hang freely in the visceral cavity; in the Bryozoa, as in the Mollusca, we find both these characters of elevation, together with (in certain species) a gizzard-like organ, and a cluster of biliary follicles in and around the stomach, closely resembling those of the Compound Tunicata. The relative position of the oral and anal orifices, again, and the position of the single nervous ganglion between them, are essentially Molluscan characters. The absence of a heart and circulating system in Bryozoa is, it is true, a character of degradation; but this apparatus is already so extremely degraded in the Tunicated Mollusks, as to be only removed by one step from that provision for the movement of fluid in the general cavity of the body, which represents in Bryozoa the blood-circulation of higher animals. The propagation by gemmation, although formerly supposed to be a character exclusively Zoophytic, is common also to the greater part of the Tunicata. And although many of the composite fabrics of Bryozoa have a stony density, and closely resemble the solid polypidoms of certain Anthozoa, yet in others, especially among the fresh-water species, we find a very close resemblance to the gelatinous bed or leathery crust in which the Compound Ascidians are lodged. To us, therefore, it seems clear that the Bryozoa and Tunicata ought to be placed in close approximation to each other, their general plan of conformation being no more different than that of any other two classes of the Molluscous series; and that, taking the same rank in that series with the Vermiform group among the Articulata, they present various relations of *analogy* to members of that group, though none of *affinity*. The term *Molluscoida* has been proposed by Milne-Edwards as a distinctive designation for this group; but such a separation seems scarcely required by any essential difference in *plan* from that of the true Mollusca, to whose "archetype" it has been shown to approach very closely,* the chief differences lying in grade of development. Professor Owen again adverts to this question, when treating of the Tunicata in a later part of the volume; and the following sentence will, we think, afford pretty satisfactory evidence, in the obscurity of its ideas and the involution of its style, that he has by no means "thought himself clear" upon the subject:

"If these significant indications of the fundamental affinity of the Polypes, with the retention of the polype form, and the absence of a respiratory organ in the highest of the class should beget a doubt as to the propriety of calling a Bryozoon a Mollusk, and thereby losing the advantage of the latter term as a definite and intelligible sign of a certain advance of organization, the comparative anatomist, whilst admitting the full amount of the affinity of the Bryozoa with the Tunicata, and thereby illustrating his view of the Molluscous series as constituting a great parallel branch of the Animal kingdom with the articulate series, may anticipate a verdict in favour of his judgment, in the necessarily artificial mode of successively treating of the different types and grades of organization, if he should select the compound Ascidians as the point at which, for his needs of description and generalization, he severs an unequivocally natural series of animals from the wide-spread root or base from which it springs." (p. 473.)

If our readers comprehend this, it is more than we can do.

* See the article Mollusca in the English Cyclopædia, vol. iii.

At the commencement of Professor Owen's ninth Lecture, On the *Acalephæ*, we are startled by the following statement:

"In the preceding lecture we saw that, whilst the new individuals propagated by gemmation were for the most part like the parent, those that came from the ova were in very few instances like the parent, but underwent a considerable metamorphosis. They quitted the egg-state either as a ciliated planula under the guise of a leucophrys, or were partially ciliated on special lobes, like a rotifer; or, what was more extraordinary, they came forth under the form of an animal which is usually ranked as a member of the higher class of Radiata, viz., a free-swimming, bell-shaped, or discoid medusa." (p. 157.)

On turning to the preceding lecture for the justification of this last statement, which seems to us to manifest a complete want of comprehension of the facts of the case, we find it stated (p. 155) that in the marine *Hydrozoa*—

"The offspring developed in the ovi-capsules are, as a general rule, the ciliated larvæ called 'planulæ;' the *Plumularia coronata* offering an exception analogous to the *Alcyonella* in the highest, and the *Hydra* in the lowest, class of polypes; whilst other *Plumulariæ*, the *Corynidæ*, and certain species of *Campanularia*, deviate in a still more remarkable manner by the development and liberation of the locomotive offspring in the guise of a minute *Medusa*."

Still more astonished at the assertion that a Zoophyte ever produces a medusan *embryo*,—the fact having been established beyond all question that the Medusa is a *bud* from the Zoophyte, containing its sexual apparatus,—we turn back to the lecture on Hydrozoa, where we find it stated correctly enough (p. 131) that the Coryne originally developes a many-armed nutritive polype or individual; but that a set of *buds* developed around the base of the first polype, instead of repeating the form and condition of that animal, take-on a higher form, resembling that of a bell-shaped Medusa, become detached, and swim off to a distance, forming and discharging the ova, which in their turn develope the fixed polype-shaped Coryne. This history, in its essential features, is true of all the Compound Hydroida and of the Medusan Acalephæ; which thus are but two states, or rather parts, of one and the same kind of organisms. As we long ago maintained, in opposition to the "Parthenogenesis" doctrine of Professor Owen, the Medusan buds are the sexual organs of the Hydroid Zoophyte; and neither can be regarded in itself as a complete organism, any more than a Plant can be said to be a complete organism without its floral apparatus, or its floral apparatus without its stem and leaves. Whilst, in the ordinary *Corynidæ*, the Medusa-buds detach themselves, and swim freely away, before maturing their ova or spermatozoa, in the fresh-water *Cordylophora*, the generative bud does not assume the characteristic Medusan form, although presenting (as Professor Allman has shown) the essential features of the Medusan structure, and developes its ova or its spermatozoa whilst still in connexion with its stock; and its ova, when fertilized by the spermatozoa, evolve themselves first into the form of ciliated gemmules, and then into that of Hydroid polypes. So among the *Campanularidæ*, in which the generative buds (like the ordinary polypes) are produced in clusters within horny capsules, these buds evolve themselves in some species into the form of independent Medusæ; whilst in others they do not detach themselves, but expand one after another into the Medusan form at the mouth of the capsule, withering

and dropping-off after they have matured their generative products; and in other cases, again, in which the medusan conformation of the sexual gemmæ is obscured by want of development, the generative act is performed whilst they are still enclosed within their capsules. This last is the only mode of generation that has yet been witnessed among the *Sertularidæ;* for no free Medusoids have been observed to make their way out of the (so called) ovigerous capsules of this family, the bodies developed within which, although commonly reputed to be eggs, are really sexual gemmæ, containing sperm-cells or ova as the case may be, though never attaining the condition of Medusæ. It is a complete misconception to affirm, as Professor Owen does (p. 161), that "the bell-shaped medusoid which Dalyell saw struggling to escape from the ovicapsule of the *Campanularia,* is the equivalent, or homologue, of the ciliated planula, which in like manner escapes from the ovi-capsule of the *Sertularia;*" for the homologue of the "bell-shaped medusoid" is the ovigerous or spermigerous gemma, which, in the Sertularidæ as in some Campanularidæ, remains within the capsule, unexpanded into a Medusoid; while the equivalent of the "ciliated planula" is the gemmule, which, in the Campanularidæ as in the Sertularidæ, is the first product of the true generative operation, whether this be performed by free medusoids or by gemmæ whose development into the medusan form has been arrested.

Now among the Hydroida, the zoophytic form is that which attracts most attention, and by which, therefore, the organisms belonging to it have hitherto always been designated; the fact that the true generative apparatus of many of them was developed in the form of free-swimming medusans, having until lately escaped observation. On the other hand, the Pulmograde Acalephæ have until recently been known only in the Medusan stage of their existence; their origination as gemmæ from Hydroid Polypes having likewise been a discovery of the present era. But now that the absolute identity of the two processes has been substantiated, can any sufficient ground be assigned for keeping apart the two groups of which they are severally characteristic? The analogy of the Vegetable kingdom will supply us with a useful basis of comparison. In many Phanerogamia, especially the groups that furnish our most valuable timber-trees, the vegetative portion of the organism,—namely, the stem and roots, the branches and leaves,—is so predominant, and the generative apparatus is so imperfectly developed, that our "idea" of an oak, an elm, a beech or a fir, is almost entirely based on the characteristic aspect of their aggregate. But there are other Plants of which the flower or generative apparatus is the only ostensible part, the vegetative being altogether subordinate, and perhaps so completely concealed as to attract no attention; thus, for example, many persons only know the Colchicum by its autumnal blossom, and are not at all aware that it sends-up a leaf-stalk in early spring, which dies-down some months before the flower appears; the remarkable parasitic Rafflesia seems to be "all flower," its nutriment being drawn already elaborated from the plant upon which it sprouts; whilst, again, the *Vallisneria spiralis* only makes its existence known to us in its native streams, by sending its unisexual flowers to their surface, of which the female still remains in connexion with the plant at the bottom through the intermediation of an elastic

spiral stem, but the male detaches itself altogether whilst still quite immature, floats to the surface, expands there, and performs the act of fecundation long after its separation. Now would any one dream of classifying Plants according to whether their vegetative or their reproductive apparatus, their foliage or their flowers, happened to make the strongest impression upon our senses; or to separate the Colchicum and the Vallisneria from other plants, because we may never happen to see any part of them but their blossoms? The *scientific* "idea" of a Plant involves the entire organism, its apparatus of nutrition with its apparatus of generation; and this is common to all the cases we have cited, notwithstanding the extreme difference which is presented by different tribes in the relative proportions which these two apparatuses bear to one another. And just on the same principle, the scientific idea of a Hydroid Zoophyte ought to be made to include its medusoid as well as its polypoid buds, notwithstanding that the former may be so imperfectly developed as to constitute no obvious feature in their organisation; whilst the scientific idea of a Medusa must include the antecedent polype-stock from which it has been budded-off. Professor Owen, however, justifies his retention of the class by an analogy of a very different kind, that of those Insects which pass the greater part of their lives under ground or in water in the larval state (in which they are on a level with the vermiform articulata), and which only present themselves for a brief period in the perfect or imago state; for he remarks, "we do not class the cockchafer and the May-fly with the Vermes, as we ought to do according to the analogy of the Campanularia and the Coryne." The two cases, however, are far from having the parallelism which he assigns to them. The larval insect is *not* a worm; for however much it may resemble a worm in its general grade of organisation, it has no generative apparatus, and is therefore not a complete animal, to be classed among the Vermes: whilst, again, the whole of its progress towards the higher form is marked by the progressive development of parts which are added or substituted, for the completion of the organism; and it is quite as just, therefore, to take this perfect form as the type of its class, as to base the typical characters of the human species rather upon the entirely-developed organism, than upon any of the earlier phases of it. But the Medusa does not stand to the Hydroid Zoophyte in this relation; for the former cannot be regarded as the perfected type of the latter, any more than the latter can be regarded as a complete organism without the former. The polype-stock alone is like a worm without sexual organs; the medusa alone is like the sexual apparatus detached (as certain worms do detach it) from the body that developed it.

The view which we advocate is supported by all those recent additions to our knowledge of the *Cirrhigrade* and *Physograde* Acalephæ, of which the recent researches of Huxley, Kölliker, Vogt, Leuckart, and other eminent Anatomists have been so productive. Here again we have to notice the unaccountable omission, on the part of both our Authors, of all reference to these masterly investigations, which have contributed to a very general accordance among all those who have studied this curious group of animals, as to their real nature and relations, which are concisely expressed by Kölliker's term "Schwimmpolypen." For the *Velella* and

Physalia, the *Diphyes* and the *Siphonophora*, whose nature has been a source of perplexity to all Zoologists who have sought to understand them as *simple* animals, are readily comprehended when their general plan is compared with that of any one of the composite Hydroida, and allowance is made for the speciality of organisation by which it is adapted for free locomotion. Thus, the well-known *Physalia*, or "Portuguese man-of-war," has been anatomised as if its great air-vesicle or float were the essential part of its body; and all sorts of speculations have been put forth with regard to its nature, the cirrhi dependent from its under side being looked-upon as quite subordinate organs. But of these cirrhi it is now known that the shorter ones, like those of the little Velella, are so many hydroid polypes, in mutual communication with each other, so as to form a polygastric digestive apparatus, to which the air-vesicle of the Physalia stands in the same relation, as the delicate horizontal plate of the Velella, with its vertical crest or sail, does to the polypoid cirrhi dependent from its under side; whilst, as these polype-mouths are not themselves furnished with tentacula, additional appendages of this kind are developed for supplying them with food. In each case, the generative function is provided-for by the development of medusoid-buds, which (as in the ordinary Hydroida) sometimes become detached, sometimes remain in continuity with the stock, evolving spermatozoa or ova; and the generative product appears to be a polypoid animal, from which the entire composite body is gradually evolved by gemmation.—We cannot but think it singular that Professor Owen should not have been aware, that Mr. Huxley communicated to the Linnæan Society, as long ago as 1849, a memoir on these Composite Acalephæ, containing the results of observations which he had made upon them during his four years' voyage as assistant-surgeon in the surveying-ship *Rattlesnake;* that the funds of that Society not enabling it to publish Mr. Huxley's memoir, he applied to the Government for the necessary means, and was kept by it in a state of suspense for several years; and that at last, the Government Grant Committee of the Royal Society appropriated a large sum to this purpose, so that the publication of Mr. Huxley's researches (which has been in great part anticipated by that of the observations of Kölliker, Leuckart, and Vogt, though these were not made until after Mr. Huxley's memoir had been communicated to the Linnæan Society) may now be speedily looked-for. And yet, as if he were perfectly unaware that either Mr. Huxley or any one else had already attained the solution of the complicated problem which this group of animals presents, he concludes his Lecture on the Acalephæ with this passage:

> "With regard to the development of the ciliograde and physograde species, scarcely anything connected or precise is at present known. The medical officer who may be destined for foreign service, and to whom the study of Nature offers any charm, could hardly contribute observations more valuable to natural history, than such as he might be able to make on the generation and development of the Pelagic Acalephæ."

Here, again, we are tempted to inquire whether Mr. Huxley's very pointed repudiation of the whole doctrine of Alternation of Generations, and of Professor Owen's parthenogenetic modification of it, can have anything to do with the Hunterian Professor's very marked abstinence

from all allusion to his labours on this subject. That he should take no notice of what Kölliker, Vogt, and Leuckart have published, is certainly surprising, but scarcely *so* surprising.

Although Professor Rymer Jones designates the Acalephæ of Cuvier by the term Hydrozoa, yet he appears to have made this change solely on the ground of the polypoid origin of the Medusan forms of the class; and his notice of the Physograda, Cirrhigrada, and Diphyda, the very forms which recent researches have shown to preserve the polypoid character through the whole of life, stands almost exactly as it did in the first edition, the only addition made to it being one that is by no means accordant with the existing state of our knowledge.

Both authors, as might be expected, give a tolerably-full account, with illustrative figures, of the developmental history of the *Cyanæa aurita*, as established by the observations of Sars, Siebold, Dalyell, and others; but neither makes any mention of the fact, which we hold to be of fundamental importance in the interpretation of the process, that the original polype-stock (the *Hydra tuba* of Dalyell), instead of *dividing* itself into medusa-disks, buds-off a pile of medusa-disks, and may even recommence its polypoid mode of gemmation after doing so. It is this fact, which, as we have shown on a former occasion,* establishes the essential homology between the polype-stock of the Campanularia and that of the Cyanæa; the difference between the two lying only in the fact, that the polype-buds of the former remain in continuity with each other, so as to form that composite structure by which the species is best known; whilst those of the latter detach themselves when mature from the parent-stock (like those of the common Hydra), and their medusa-buds, instead of being so minute as to escape notice, unless searched for by a Microscopist, evolve themselves into those massive forms which force themselves upon the attention of every observer. The life-history of the one organism, however, is so completely the same in all its essential particulars, with that of the other, that it is difficult to see on what ground the two could be ranked in distinct classes, if we were now to become acquainted with them for the first time, instead of having our view of them prejudiced by long usage.

Professor Owen's account of the class *Echinodermata*, although not in every respect what could be wished, is on the whole satisfactory. He gives a pretty full account of Professor Müller's recent researches on the curious larval states of this group; but this account is not furnished with the illustrations requisite to enable the descriptions to be comprehended. We will defy any one to form the least idea of their marvellous shapes without the aid of figures; and even the best delineations can give but a very imperfect notion of them. The chief point on which Professor Owen's account of the developmental history of these Echinoderms requires amendment, is his account, on the authority of Sars, of the development of *Echinaster sanguinolentus*. This history is so inconsistent with that of the development of the Star-fish larvæ afterwards observed by Müller, that it obviously needed revision; and this revision was made by Busch (a pupil of Professor Müller) in 1851. He found that the body regarded by Sars as a mere pedicle or organ of attachment for the

* Vol. i. p. 183.

young Star-fish, is really a larva-zooid, having a stomach and probably a mouth of its own, and is thus analogous to the larvæ of other Star-fishes, though of smaller relative size and less complex structure; the difference being apparently related to the peculiar circumstances under which the development takes-place in this type, the larva being retained within a sort of marsupial chamber, formed by the drawing-together of the rays of the parent around its mouth, instead of being sent to sea to take care of itself, as is the case with the Star-fish larvæ generally.—The want ot illustrations cannot be charged against Professor Rymer Jones's Chapter on this subject; for in addition to the copions and beautifully-executed figures which illustrated this part of the previous edition, numerous wood-engravings of the same high class, copied from the admirable representations of Professor Müller, have been introduced in elucidation of the descriptions. We cannot, however, speak in terms of commendation ot the chapter as a whole; since it contains many glaring errors. The mode, for example, in which Professor Rymer Jones would turn an Alcyonian Polype into an Encrinite (p. 203), shows an ignorance or forgetfulness ot some of the most essential features of the structure of the latter; and the assertion (p. 204) that the *Comatula* may be considered "one of the lowest of the Asteroid Echinodermata," must excite a lively emotion of wonder in the mind of every one who is acquainted with its true relations. For, as was long since pointed-out by Professor E. Forbes, the Comatula is essentially *a free Crinoid;* the earlier part of its life being passed in the true crinoidal state; and its entire organization being conformable to that type. One of the most striking features of that type is the presence of an intestinal tube and a distinct anal orifice, which places the Comatula as much above all ordinary Star-fish in this respect, as it is in the activity of its locomotion. Professor Rymer Jones does not give the slightest indication of being acquainted with Mr. J. V. Thomson's twenty-years'-old discovery, that the little body which he had previously considered as a Pentacrinus, is really the larval condition of the Comatula; and like Professor Owen, Professor Rymer Jones repeats Sars's account of the development of Echinaster, without the correction which alters the entire interpretation of the facts accurately recorded by Sars.

We have now arrived at the conclusion of the task which we have imposed on ourselves; and we lay down our pen with the feeling of great regret, that we have been obliged to execute it in a spirit of such constant depreciation. That much labour and ability have been brought to bear by both our Authors in the preparation of these new editions, none can be more fully aware than those who, like ourselves, have had occasion to go over the very same ground. And we can only lament that the want of determination to bring their works *thoroughly* up to the present state of knowledge, has led their Authors too often to rest satisfied with additions, when alterations, suppressions, and entire recastings were quite as much needed. We could easily show, if it were necessary, that this statement is no less true of the portions of both volumes which we have left unnoticed, than it is of those to the critical examination of which the preceding pages have been devoted. And we could also point, in the notes to Professor Owen's lectures, to many allusions to the writings of

contemporary authors, that betray a disposition to exalt himself at their expense, which will assuredly not increase the estimation in which he is held. In more than one instance, moreover, he makes these allusions to past editions of their writings, and cavils at statements which he can scarcely help knowing them to have since qualified or withdrawn. Would Professor Owen like some of his own earlier memoirs to be quoted against himself? Would he take his stand on his first 'Anatomy of the Terebratula,' or on the ornithic character of the wing-bones from the Maidstone chalk?

We cannot charge Professor Owen with any neglect of his predecessors in the way of citation; and the very copious list of works referred to, which concludes his volume, not only bears testimony to the extent of his research, but will be very useful as a guide to those who desire to ascertain the authority for his descriptions. The example of one who has confessedly the greatest opportunities of personal study of Comparative Anatomy at his command, and who never omits to draw the attention of his readers to any point on which he thinks he may lay claim to originality, should be a sufficient assurance that the materials of any such comprehensive systematic treatise *must* be largely drawn from the labours of others; and should make critics hesitate in stigmatizing as mere compilers, writers who honestly avow this necessity. We feel called upon also to remark, that Professor Owen seldom gives the least hint of the source of his illustrations (the number of which in this edition is not increased in by any means due proportion to the text), so that for anything that appears to the contrary, the reader might suppose them to be original. So far is this from being the case, however, that the greater number of them are borrowed without the least acknowledgment; a proceeding of which we cannot see any justification, and which appears to us manifestly unfair towards the original delineators. With the Hunterian Museum at his command, we should have thought that Professor Owen would have considered it more creditable to avail himself of the ample store of subjects it contains for the draughtsman's pencil, than to have recourse to a wholesale appropriation of the delineations of others, which can only be thought excusable when fully acknowledged.

Professor Rymer Jones contents himself with a few references at the foot of his pages; and of these not a few are worthless, his authorities having been often superseded by others more modern and more trustworthy. No fewer than sixty-two new illustrations have been introduced; and these are of the same high character with those which constituted so remarkable a feature of the previous edition. We find ourselves obliged to repeat our animadversion, however, with respect to the unacknowledged appropriation of the delineations of others; since very few, if any, of Professor Rymer Jones's illustrations are original, and their sources are very seldom indicated. We by no means object to a repetition of really good figures, especially when they are taken from Monographs whose authors have made a special study of the organisms they represent, and are equal or perhaps superior to any that could be produced *de novo* from less satisfactory materials; but justice as well as courtesy to the originators of them, seems to us to demand that these be not deprived, even by implication, of their rightful title.

Review II.

1. *On the Organic Diseases and Functional Disorders of the Stomach.* By George Budd, M.D., F.R.S., Professor of Medicine in King's College, London, late Fellow of Caius College. — *London*, 1855. pp. 357.
2. *Digestion and its Derangements. The Principles of Rational Medicine applied to Disorders of the Alimentary Canal.* By Thomas K. Chambers, M.D., Fellow of the Royal College of Physicians, Physician to St. Mary's Hospital, and Lecturer on the Practice of Medicine at St. Mary's Medical School, London.—*London*, 1856. pp. 552.
3. *Stomach and Intestine.* By Dr. Brinton. In the 'Cyclopædia of Anatomy and Physiology,' parts 46 and 47.—*London*, 1855.

Every one who has paid any attention to the progress of medical science in this country, must be aware that Dr. Budd has for many years made the chylopoietic viscera a subject of special study. The present volume, 'On the Organic Diseases and Functional Disorders of the Stomach,' is almost entirely made up of lectures delivered either at the College of Physicians, or to the students of King's College, and published shortly after delivery in the 'Medical Gazette,' and the 'Medical Times,' with such additions and corrections as the author's subsequent experience has suggested. It is a work of a purely practical character.

Dr. T. K. Chambers has produced a book of a very different stamp. The first half of his volume is composed of a sketch of the microscopic anatomy and the physiology of the parts concerned in the process of digestion, concluding with a very interesting chapter on 'The Physiological Action of Substances submitted to Absorption in the Alimentary Canal,' from which we should have quoted freely, if we had not speedily traced the origin of its most important sections to articles originally published by the author in this Review, on 'The Use of Alcohol, Tea, Coffee, and other Accessory Foods.' He has freely availed himself of the scientific labours of Bidder and Schmidt (the eminent Dorpat physiologists), and of the investigations of their numerous pupils; and shows himself perfectly conversant with the works of Kölliker, Ecker, Bernard, Frerichs, Lehmann, &c.; he has thus succeeded in presenting to the English reader a correct history of the process of digestion, in so far as it has been elucidated by the most eminent physiologists and some of the most celebrated chemists of the day. The last half of the volume, 'On the Derangements of Digestion,' is composed of ten chapters, in which the following subjects find prominent places:—Changes in Parts common to the whole Alimentary Canal; Morbid Affections of the Mouth and Gullet affecting Digestion; Morbid Affections of the Stomach; Morbid States of the Small Intestines interfering with Digestion; Morbid States of the Pancreas; Morbid States of the Liver affecting Digestion; Morbid States of the Colon affecting Digestion; Flatulence; and Regimen.

In the following pages we shall follow the arrangement adopted by Dr. Budd, giving, as far as our limits will allow us, an analytical sketch

of his work; while, at the same time, we shall not unfrequently advert, by way of illustration, to the parallel investigations of Dr. Chambers.

After a brief notice of the special difficulties attending the study of disorders of the stomach, Dr. Budd enters somewhat fully into the consideration of the changes which that organ undergoes after death from the action of its own proper (gastric) juice. Our readers are doubtless aware that it was John Hunter who first announced that the stomach may be dissolved or digested after death by the secretion poured forth from its own walls. The first two occasions on which he observed this phenomenon were on men who were killed from fracture of the skull (one man dying outright, and the other in a few hours); and he afterwards met with it in a man who had been hanged. He found softening of the stomach of the same kind in some of the animals on which he was making experiments with regard to digestion, and he frequently observed a similar appearance in different varieties of fishes, especially such as were killed when the stomach was more or less filled. From these and similar cases, he concluded that digestion of the stomach is most common, and takes place to the greatest extent, in persons who die violent deaths; but (he observes) there are few dead bodies in which the stomach, at its great end, is not in some degree digested. The next important step was made by Spallanzani, who, in repeating and varying Hunter's experiments, discovered the important fact, that a certain amount of heat is requisite to develop the solvent action of the gastric juice. Lastly, the experiments of Wilson Philip, and Carswell tend to show that this change only takes place when death happens soon after a meal—that is to say, when the process of gastric digestion is actively going on. Hence the conditions on which this peculiar process depends are—1. That the stomach at the time of death should contain a certain quantity of acid gastric juice; and 2. That the body should be exposed for some hours after death to the temperature required for artificial digestion. The first of these conditions is generally fulfilled in healthy persons meeting with fatal accidents soon after a meal; and it has hitherto been commonly supposed that intense post-mortem digestion of the stomach only occurs under such circumstances. Dr. Budd, however, quotes a very interesting case, in which the great end of the stomach and the adjacent portion of the diaphragm were completely dissolved in the body of a gentleman who met with a violent death from fracture of the skull, at a period when the stomach was probably quite empty.* It is difficult in this case to understand how, without the ordinary stimulus of food, so much gastric juice should have been secreted as to have occasioned this destructive process of solution. Was it (he asks) determined by the shock of the accident, or by the subsequent irritation of the brain which the accident occasioned? Dr. Budd seems almost inclined to attach some weight to the view which Hunter early entertained but subsequently abandoned, that digestion of the stomach is especially apt to occur after death from fracture of the skull.

In certain diseases, gastric juice seems to be secreted when the stomach is empty, and consequently exists in the stomach, unmixed with food: and, moreover, there are certain catarrhal states of that organ in which lactic acid is freely generated from the saccharine principles of the food, and

* See pp. 16–17.

forms with the mucous membrane an efficient digesting mixture. In persons who die from (or with) these diseased conditions, digestion of the stomach may occur in as high a degree as in healthy persons killed by accident soon after a meal; and Dr. Budd even goes so far as to maintain that—"Not unfrequently the softening of the stomach may be predicted with tolerable certainty by a peculiar train of symptoms which result from the presence of free gastric juice or of a digesting acid in the otherwise empty stomach."

This occasionally happens in cases of simple ulcer of the stomach. It is often found, sometimes in a high degree, in persons, and especially in women, who die from phthisis: these patients generally having had, for some time before death, much disorder of the stomach, such as "pain and tenderness at the epigastrium, loss of appetite, thirst, frequent vomiting (the matters vomited being slightly acid), or frequent nausea." Dr. Budd suggests the probability that, in these cases, the flow of gastric juice in the empty stomach is excited by irritation of the lung. It frequently takes place in persons who die from inflammatory diseases of the brain—diseases which give rise to the same kind of secondary gastric disorder as tuberculous diseases of the lungs—as vomiting, nausea, pain in gastric region, &c. It is likewise of common occurrence in persons who die from typhoid fever (especially when there is much cerebral disturbance), and is occasionally met with in persons who die from cancer of the uterus, from peritonitis, or from other abdominal diseases leading to secondary functional disorder of the stomach; but it is in infants who die from the age of three months to two years, especially in cases of hydrocephalus and phthisis, and occasionally in deaths from exhaustion, consequent on the eruptive fevers or on improper diet after weaning, that softening of the stomach, in a high degree, occurs most frequently.

These facts have been observed by Louis, Cruveilhier, and others, but they supposed the softening to be the result of some disease which gave rise to no appreciable symptoms, and to occur during life. Dr. Budd, on the other hand, regards the softening as occurring in all cases after death, and holds that its degree depends, *cæteris paribus*, solely on the quantity of gastric juice in the stomach at the time of death; and the preceding remarks show that it is especially common in those diseases which have been long known to lead to secondary functional disorder of the stomach, accompanied either with an augmented secretion of gastric juice, or with secretion of gastric juice when that organ does not contain food, or with undue retention of the solvent fluid.

The next question discussed by our author is, How is this functional disorder brought about in these several diseases, and what is its real nature? We must content ourselves with stating his conclusions, that the secondary disorder of the stomach in these diseases is produced through the intervention of the nervous system; and that it affects the secreting apparatus, and not merely the muscular coat of the viscus in question. Nor can we follow him through his lucid description of the rarer forms of softening of the stomach by the gastric juice; we must restrict ourselves to his summing-up, which is as follows:

"The result, then, at which I arrive is, that the softening, with thinness, of the coats of the alimentary canal, described by Louis; the pasty or pulpy, and the

gelatiniform softening, of Cruveilhier; and the other varieties described by other authors, distinguished by the colour of the softened tissues, are essentially the same change; and that this change, whether it exist in the lower end of the œsophagus, or in the great end of the stomach, or in the fore part of the stomach only, or in any part of the small or the large intestine, is the result of *digestion* after death; like the softening of the great end of the stomach, remarked by Hunter, that occurs after sudden and violent death in the midst of health, and soon after a meal." (p. 46.)

After a few remarks on the importance of the subject, both in reference to medico-legal inquiries and to practice (because the study of it leads us to the knowledge of a peculiar and not uncommon form of indigestion, dependent mainly on the presence of free acid in the stomach and bowels), we proceed, in the third lecture, to Congestion of the Stomach.

The most simple kind of congestion is that which results from some mechanical impediment to the return of blood from the stomach to the heart. Dr. Yellowly's [Yelloly's?] well-known cases, showing the effect of death by strangulation in the induction of this kind of gastric congestion, are referred to, and one of them (that of Nicholson) quoted; and Dr. Budd proceeds to illustrate by a very forcible case, the close similarity in this respect between the effect of severe epileptic fits and the sudden strangulation inflicted by the hangman. Congestion of the stomach, in a less marked degree, always exists in persons with the hob-nail or gin-drinker's liver, in consequence of the impeded passage of the gastric venous blood through the portal vein. Dr. Budd states that he has met with several instances of convulsions in which death was very much hastened by hæmorrhage from this source, and in which the sound state of the stomach showed that the blood had exhaled from the unbroken surface of its mucous membrane. The same thing may happen in organic diseases of the heart, and in such diseases of the lung as interfere with the passage of the blood through the chest, the hæmorrhage being usually very slight, but occasionally profuse and violent, as in two cases of rheumatic pericarditis described by our author.

He then shows, from a review of analogous cases, that "it is in accordance with a general law, that when the stomach is kept in a state of passive congestion from a bar to the current of blood through the liver or chest, the secretion of the gastric juice is diminished, the stomach can digest less food, and requires longer intervals of rest." If, under these circumstances, indigestible food, or an excess of ordinary food, be taken, some of it remains undigested in the stomach, and irritates, and may even inflame, the mucous membrane. Hence in the treatment of the diseases which occasion this congestion of the stomach, and consequent feebleness of digestion, we should prescribe a sparing and easily-digestible diet, and, when the system can bear it, total abstinence from fermented drinks; if, however, alcohol seems necessary, it should be given sufficiently diluted; and (adds Dr. Budd) when mercury and diuretics are deemed expedient for the object of relieving the embarrassed circulation, their action on the stomach should be carefully watched.

We have hitherto spoken only of the simplest kind of congestion, namely, the vascular turgescence arising from a mechanical impediment to the return of the blood from the stomach to the heart. It may, however, result from very different conditions. To say nothing at present of

the congestion that may arise from inflammation of the stomach, or from the growth of a cancer, or from other structural changes, a not unfrequent origin of this affection must be sought in an unnatural condition of the tissues which the blood nourishes, and likewise in an unnatural condition of the blood itself. Under this last head, Dr. Budd places the congestion of the stomach, and the consequent hæmorrhage which sometimes occurs in women from a stoppage of their monthly courses, and which are not unfrequently observed in malignant cholera and in yellow fever, and now and then, though very rarely, in the course of typhoid fever. Lastly, amongst the causes of congestion of the stomach, and consequent hæmorrhage, we must notice arrest of the biliary secretion, and enlarged spleen; in the latter case we find that the hæmorrhage is preceded or attended by ascites, by enlargement of the superficial veins of the belly, and by other symptoms, which show that the passage of the blood through the liver is greatly impeded. Whether, however, the seat of obstruction lie in the liver or in the spleen itself, is not yet fully determined.

The next two lectures are devoted to inflammation of the stomach. We shall briefly notice the most important forms of this affection, beginning with the simpler and proceeding to the more severe varieties. (1) One of the simplest and most frequent forms of inflammation of the stomach, is that which is brought on by excess in eating or drinking, and especially by the ingestion of substances hard of digestion and of alcoholic drinks. Dr. Budd quotes freely from Dr. Beaumont's experiments on Alexis St. Martin, which throw much light on this form of inflammation:

> "Many instances," says our author, "are on record, in which a person previously in good health has died very speedily and unexpectedly after a gluttonous meal of some indigestible substance, and apparently in consequence of the pain which the over-distension and irritation of the stomach occasioned. In such instances, the immediate cause of death is faintness, or stoppage of the heart's action, under the influence of the pain; and I believe that in most of them the heart was previously unsound, or the power to bear up under pain or other distressing sensations had been weakened by intemperate habits." (p. 92.)

(2) A higher degree of inflammation, brought on in the same way—namely, by direct irritation of the stomach, is seen in persons who have swallowed hard and insoluble substances, or irritant or corrosive poisons. Under this head we find a notice of the case recorded by Dr. Marcet, of a sailor who, in imitation of a juggler, swallowed fourteen clasp knives, and lived between three and four years afterwards, although at first, and at intervals subsequently, he suffered very severely from pain in the stomach, vomiting, &c.; and of the still more remarkable case recorded by Dr. Spry, of the man who swallowed molten lead during the fire which consumed the Eddystone Lighthouse in 1755. In attempting to throw water upon the fire above him, and in looking up to watch the result, a quantity of molten lead fell upon his head and face, severely burning those parts, as well as his neck and shoulders. From that moment he had a violent internal sensation, and imagined that a quantity of the lead had passed down his throat into his body. He and his companions were rescued and taken to Plymouth, where he was attended by Dr. Spry, who, however, would not believe his statement regarding the lead he had swal-

lowed. He constantly took his medicine, and swallowed many things, both liquid and solid, till the tenth or eleventh day, after which he suddenly grew worse; and on the twelfth day, being seized with cold sweats and spasms, he soon afterwards expired. On examining the body, Dr. Spry "found the diaphragmatic upper mouth of the stomach greatly inflamed and ulcerated, and the tunica in the lower part of the stomach burnt; and from the great cavity of it took out a great piece of lead, weighing 7 oz. 5 drs. 8 grs." He transmitted an account of the case to the Royal Society, "but that learned body, thinking the circumstance very unlikely and extraordinary, and doubting the truth of it, the reading of the paper was deferred until a further elucidation was received." Dr. Spry, finding his word thus doubted, took the wisest course that was open to him, and performed a series of equivalent experiments on dogs and fowls by pouring molten lead down their throats. These experiments clearly showed that there was no impossibility in the case he had recorded being perfectly true; and on forwarding them to the Royal Society, his original paper was read, and afterwards published in the 'Transactions.' Similar experiments were made a few years ago in France, by Bretonneau, who injected boiling water into the stomachs of dogs. Four of these dogs were killed on the third day, and one on the seventh day, after the operation; the stomachs were found to be more or less in a state of gangrene, but the animals were cheerful and played together after the first two days.

Similar to these are the numerous cases recorded in our medical literature, in which strong mineral acids have been taken, and in which the patients have often survived for a considerable period.

3. Another condition giving rise to inflammation of the stomach, is long-continued abstinence. Although this fact has been indistinctly perceived since the time of Hunter, we are indebted to MM. Andral and Gavarret for distinctly establishing it in a series of observations which they instituted, some years ago, on the influence of various conditions on the blood. In one of their experiments (quoted by Dr. Budd), three dogs were employed: the first was entirely deprived of food and drink, and lived twenty-one days; the second was deprived of food, but allowed to drink water, and lived twenty-five days; and the third was allowed a small quantity of soup every morning, and lived thirty-three days. The third dog was the only one in which the stomach was not ulcerated; and the abnormal redness of the mucous membrane of its stomach was less general and less vivid than in the others. Its case seems to show that even a small quantity of food, if it contain all the necessary elements of nutrition, may hinder those destructive changes which are caused by total abstinence. Although "evident signs of inflammation" were found by Andral in the stomachs of the animals on which he experimented, it does not appear, from the cases quoted by Dr. Alfred Taylor, in his 'Manual of Medical Jurisprudence,' that inflammatory redness of the stomach is by any means invariably present in deaths from starvation in the human subject.

Somewhat similar are the effects produced by a long persistence in food which, whatever be its quantity, is not sufficiently varied for healthy nutrition. This is well shown by the experiments of Magendie, and of

the committee of the French Institute, which showed that dogs kept exclusively on water, with the addition of oil, sugar, fat, or even of albumen, fibrin, or gelatine, soon die of starvation, just as when kept on water alone; and is painfully confirmed by some of the earlier Reports of the Inspectors of Prisons, especially in reference to a purely bread-and-water diet. Dr. Budd is inclined to think that a rigid diet, too long persisted in, in the early stage of continued fever, has often been productive of serious gastric disorder.

4. Lastly, inflammation of the stomach may be excited by the presence of some noxious matter in the blood,—as, for instance, arsenic or other irritant substances introduced from without, or morbid matters that are, under special circumstances, generated in the body. In the latter category we may place the poisonous matters which seem to be generated in the system in yellow fever and in cholera, and very probably in certain gouty states of the system.

Dr. Budd's remarks on the treatment of inflammation of the mucous membrane of the stomach are sound and judicious, but present no novelty. The fundamental point is to give the stomach sufficient intervals of rest, and to avoid irritating it by physic or food. In slight cases, all that is usually necessary is restriction for a few days to a sparing diet of farinaceous substances and milk, and the free use of cooling drinks; in more severe cases we must apply leeches to the epigastrium, and allow the patient to sip iced water, or to suck small pieces of ice, swallowing the water as the ice dissolves.

Towards the conclusion of this chapter we find a notice of that comparatively rare form of inflammation of the stomach in which coagulable lymph is effused into the sub-mucous cellular coat, and, hardening and contracting, forms a dense gristly tissue, binding the mucous membrane to the coats beneath. Lymph effused in this way often forms a gristly hard ring around the pyloric extremity, which acts as a permanent stricture. This form of disease seldom occurs till about the age of forty, and is almost invariably the result of spirit drinking. The symptoms of obstruction thus produced are the same as those occurring in cancer of the pylorus; it may, however, often be distinguished from the latter disease, (1) by its slower progress; (2) by the absence of hæmorrhage, which frequently occurs in cancer; (3) by the absence of any palpable tumour, which can often be detected in the latter disease; and (4) by the fact that it almost always occurs in spirit drinkers.

The treatment consists in the application of leeches and blisters, and the prescription of a rigid diet, as long as the inflammatory process is going on: subsequently the treatment can be only palliative; a most important point is, make the patient altogether give up the use of spirits.

The two next chapters treat of Ulceration of the Stomach—an affection which has received the names of *simple*, *chronic*, and *perforating* ulcer.

The stomach in most of these cases appears healthy, except for the existence of a single deep ulcer on its inner surface; this ulcer is seldom larger than a shilling, is generally circular or oval, and its edges are as sharply cut as if a portion of the mucous membrane had been punched out. Sometimes only the mucous membrane is destroyed; in other cases

the ulcerative process gradually eats through the other coats in succession, so as finally to give rise to perforation, in which case the contents of the stomach escape into the sac of the peritonæum; in these cases the external is much smaller than the internal aperture. The ulcer is generally situated along or near the lesser curvature of the stomach; usually nearer the pyloric than the cardiac orifice; and much more frequently on the posterior wall of the stomach than on the anterior. It is very seldom that more than one ulcer exists: of 79 cases noticed by Rokitansky, the ulcer was solitary in 62; of the remaining 17 cases there were 12 in which two ulcers existed, 4 in which there were three ulcers, and 1 in which there were five.

These ulcers are sometimes met with cicatrized; and sometimes, if the ulcer has been a large one, the process of healing, by the contraction that attends it, permanently alters the form of the stomach. Dr. Budd refers to two preparations in the King's College Museum, "in each of which the stomach is divided into two pouches, as if by a string passed transversely round it, looping up the greater curvature towards the lesser."

Our knowledge regarding the circumstances and special causes giving rise to ulceration of the stomach, is very defective; and none of the authors whose works we are now reviewing have added much to our information on these points.* That this simple or chronic ulcer of the stomach is not a rare affection, is shown by the data given by Jaksch, Dittrich, Willigk, and Dahlerup, in Germany; and by T. K. Chambers, Gairdner, Habershon, and Handfield Jones, in our own country, and which are quoted by Dr. Brinton, in page 160 of the seventeenth volume. It is more common in women than in men.† It hardly ever occurs before the age of puberty. (Dr. Budd once met with it in a girl aged fourteen and a half.) It is most common in the earlier portion of middle life; but has been met with up to the seventieth year. It seems to be relatively more frequent amongst the poor than amongst the rich, although it is confined to no class of society; and it is especially often found in maid-servants, between the ages of eighteen and twenty-five. Dr. Budd may possibly be correct in stating that "the ulcer has not been found in conjunction with, or in sequel to, any other disease, with such frequency as to lead us to conclude that it has any intimate connexion with it;" but he might with propriety have noticed the special frequency with which it occurs in pulmonary tuberculosis, and have alluded to the remark made by Rokitansky, that several of the patients whose bodies he examined, traced the origin of their gastric disease to intermittent fever. Moreover, Jaksch believes that childbed predisposes towards it.‡ Dr. Chambers, whose 'Decennium Pathologicum' testifies how fully he has availed himself of the information yielded by the St. George's Case Books, agrees, however, with Dr. Budd, in considering ulceration of the stomach as always an independent disorder, and not a symptom or a consequence of other affections. In short, it would appear that we have as yet no clue to the real cause of the disease, although the facts, that it is commoner amongst the poor than amongst the rich, and

* When these pages were written, Dr. Brinton's Essay On Ulcer of the Stomach, which appeared in the January number of this Review, was not published. See especially pp. 160–62.

† See Dr. Brinton's Essay, p. 161.

‡ In relation to this point we may refer to Dr. Brinton's Essay, p. 180.

that it is more frequent amongst young unmarried maid-servants than in other classes, would favour the inference that a state of anæmia predisposes to it. Why it hardly ever occurs before the age of puberty—why the ulcer is almost always single—and why it is always in the pyloric division of the stomach, and most commonly in or near the lesser curvature,—are questions which at present we have no means of answering.

Ulcers in the stomach are very difficult to heal, and in this respect they contrast strongly with corresponding lesions in the small and large intestine, which especially occur in fever and dysentery. The reasons for this difficulty of healing are—(1) the great change of volume to which the stomach is liable two or three times a-day, according as it is full or empty; (2) the constant churning motion which takes place in the stomach during digestion; (3) the mechanical and other irritation of the sore caused by the various substances taken as food and drink; and probably (4) the irritating action of the gastric juice, which, although it exerts no injurious effect on healthy mucous membrane, may dissolve and remove the plastic lymph which is effused at the bottom of the ulcer for the purpose of repairing the lost substance. It is thus (says Dr. Budd) "that a small ulcer, which causes no constitutional disturbance, which may not even much impair the nutrition of the body, and which, if situated in a lower portion of the same canal, might soon heal, becomes so serious a disease;—how it leads so continually to long-continued suffering and death."

Before turning to the symptoms of ulceration of the stomach, we may notice the natural end of this disease, if it runs its full course. There are three distinct ways in which it may prove fatal. (1) By perforation. "In the decennial period at St. George's Hospital, ending Dec. 31st, 1850, there died 24 cases of malignant disease of the stomach, and in these perforation was found thrice; there were 19 cases of simple ulcer, and perforation occurred in 9 of these. The difference is sufficiently marked to make the smallness of the numbers of no importance."* (2) By hæmorrhage, in consequence of the ulcer eating into one of the arteries of the sub-mucous cellular tissue. And (3) in rare cases, by mere exhaustion.

Sometimes ulceration may exist without any marked local symptoms. In 3 of 15 cases referred to by Dr. Chambers, in which ulceration of the stomach was present, but where death occurred from other causes, there were no local symptoms referable to that organ; and in the others, none were prominent enough to attract attention, excepting that "one patient had a pain in the left hypochondrium (which, by the way, was *not* the situation of the ulcer), and a fanciful appetite, but no vomiting, pyrosis, or any other mark of aggravated dyspepsia."†

Several explanations may be given of the absence of local phenomena. Dr. Chambers probably suggests the correct one—namely, that the want of common sensibility in the stomach makes it irresponsive to our modes of examination; and this view is confirmed by the fact, that when pain does exists, it seldom points out the exact locality of the injury.

In the great majority of cases, especially when the disease has lasted some months, the symptoms are generally well marked and significant

* Chambers, Digestion, &c., p. 402. † Ibid., p. 404.

enough. We will notice them in the order in which they are discussed by Dr. Budd. "The most constant symptom is pain in the stomach, which is generally referred to a small spot, and is more severe after meals, when the stomach is distended, and when its vermicular movements are going on." We regret that our author says nothing regarding the interval of time that generally elapses between the ingestion of the meal and the commencement of the pain, as definite information on this point would be valuable. According to Dr. Chambers, it generally begins within a quarter of an hour, and sometimes even attains its maximum within five minutes after the termination of the meal. The pain is most marked when the food or drink is of a higher temperature than the body; it is likewise much increased by moving about after meals. Dr. Chambers attaches great weight to the fact that the pain of an ulcer in the stomach is always increased by pressure, especially the local pressure of one or two fingers. The pain, moreover, in this disease is variable, sometimes being very severe for days or weeks, and then suddenly ceasing; while in malignant tumours, the pain, though not always severe, is almost always constant after it has once begun. There is also occasional eructation of a sour fluid, and sometimes vomiting. Dr. Chambers (in a memoir published in the 'London Journal of Medicine' for 1852) gives two cases in which the vomiting was so severe as to cause death; and he points out the importance of watching it closely wherever it occurs; for when ulceration is the cause of it, streaks of blood will seldom fail to be found some time or other, and they are "of the highest value in a diagnostic point of view, because they are extremely rare in other diseases which may be mistaken for simple ulcer." Often, however, the hæmorrhage is more abundant, as we have already mentioned, and in these cases it is not unfrequently preceded for a day or two by an increase of pain. Two or three pints of black clotted blood are sometimes vomited at once, while an additional quantity is carried off by the bowels, but the hæmorrhage seldom lasts more than a day or two. When it has once occurred, it is very apt to occur again, generally after the lapse of some months, but sometimes not for two or three years. Dr. Budd lays great stress on the fact, that "in persons under thirty, the only organic disease of the stomach that gives rise to profuse hæmorrhage, with very few exceptions, is ulcer." In persons above that age, vomiting of blood, preceded by disordered and painful digestion, may likewise arise from cancer; but Dr. Budd believes that the source of the hæmorrhage may be determined from the following considerations:

"Cancer of the stomach in most cases originates at the pyloric or cardiac orifice, and in some degree narrows or obstructs it. It also gives rise to a tumour, which at the end of some months is generally palpable enough; and it *always* interferes greatly with nutrition, causing progressive, and after a time extreme, wasting; while simple ulcer seldom produces any of these effects."*

Moreover,

"A simple ulcer may continue almost stationary—at any rate with little change in the symptoms—for twenty years. Cancerous disease, on the contrary, constantly and steadily progresses; the symptoms become week after week more marked; and although life may be protracted, especially in colloid cancer, for four or five years, the patient generally dies, much emaciated, within twelve months."†

* Budd, op. cit., p. 186. † Ibid.

The treatment of ulcer of the stomach is more dietetic than medicinal. We alluded in p. 36 to the circumstances which impede the healing of the ulcer: it is by lessening as much as possible their unfavourable influences that recovery is best promoted. The patient should eat little at a time, and the food should be of the least irritating kind. According to Dr. Budd, milk, and compounds of milk with farinaceous substances of the most nutritious kinds (such as bread, macaroni, semolina, biscuit-powder, Indian meal, and oatmeal, in preference to arrow-root and other substances consisting mainly of starch), constitute the most appropriate diet in these cases; and if the milk be good, and the other substances duly varied, a person may be kept on such food, in conjunction with tea and sugar, for a long time, without any impairment of strength. Dr. Chambers especially recommends iced milk, with the addition of from one-quarter to one-third of lime-water; if two or three tablespoonfuls of this mixture be taken at short intervals, regular meals are rendered unnecessary, for as much as a couple of quarts may easily be digested in the day. Moreover, he is strongly opposed to Dr. Budd, on the propriety of allowing sugar. "Those who have any ulcers in the digestive mucous membrane, should as cautiously avoid it, as those with tender teeth."* As the condition of the stomach improves, calves'-foot jelly, beef-tea (preferable, we think, if cold), or the yolk of a soft-boiled egg, may be tried; and if these can be borne without exciting pain or vomiting, the diet may be gradually enlarged.

Next in importance to a proper dieting, Dr. Chambers ranks the application of leeches (three at a time, about twice a-week) to the region of the stomach; they do not (he observes) weaken the patient, for weight will almost always be found to be gained during their use, and they are a most powerful means of preventing local congestion, of getting rid of the effete venous, and inducing a passage of fresh arterial, blood through the capillaries of the neighbourhood. It is singular that Dr. Budd alludes neither to local bleeding nor to blistering; the latter, applied to the spine, often relieves the dorsal pain which not unfrequently accompanies this disease.

When the irritation of the ulcer causes an effusion of acid gastric juice into the empty stomach, the trisnitrate of bismuth is of service. Dr. Budd recommends from five to ten grains, suspended in water by means of compound tragacanth powder and syrup, two or three times a-day, a quarter of an hour before meals, and a dose of magnesia at night; while Dr. Chambers holds that "it must be administered in large doses of from fifteen to twenty grains, or no advantage follows, and may freely be increased to two scruples or a drachm, if necessary."

Dr. Budd allows the patient to swallow small lumps of ice when the stomach is very irritable; and to take opium (which is best given in its crude form, and in pills) when the pain at the stomach is very harassing, and the nights are restless. He makes no allusion to astringents (except in the treatment of hæmorrhage), a class of remedies which Dr. Chambers declares "are often of signal benefit." Amongst these he assigns the first place to the newly-discovered salts of metals, whose constitution and form are the same as alum, especially to "iron alum."

* Chambers, op. cit., p. 411.

"Three or four grains of this salt, taken thrice a-day, give an immediate relief to the pain; and this medicine has also the advantage of adding to the blood a metallic constituent, usually much required in these cases, and improving the anæmic condition. As a change, gallic acid, nitric acid, and bitter barks, may be employed; and in these cases, as in external ulcers, I have found great benefit from variety. The original medicine is often much more efficacious after a few days' change to another. Nitrate of silver, given as usual, in pills, by itself, has disappointed me in the treatment of suspected ulcer, and indeed in all gastric complaints. . . . . The blunting of the sensibility of the sore surface by medicine enables us sooner to return to hitherto unborne articles of food, than had drugs not been used, and thus much to shorten the convalescence."*

Dr. Budd observes, that "when the bowels are much confined, an aloetic or a compound colocynth pill should be given: these medicines irritate the stomach less than castor oil, rhubarb, or the saline purgatives." We cannot altogether concur with him on this point. We have more than once seen great aggravation of the symptoms caused by colocynth pills, in cases which could bear castor oil, associated with a few drops of laudanum, with little gastric disturbance; indeed, we regard castor oil and enemata as affording by far the best means of counteracting the constipation that so often occurs in this disorder.

We shall conclude our notice of the therapeutics of ulcer of the stomach by quoting Dr. Budd's directions for treating the hæmatemesis which so frequently occurs:

"The means most likely to restrain the hæmorrhage are, ice swallowed in small quantity or applied to the epigastrium, rest in the horizontal posture, *prolonged fasting*, and medicines which have an astringent or styptic action—such as oil of turpentine, acetate of lead in conjunction with opium, alum, and tannin."†

Of these, oil of turpentine is perhaps the most trustworthy; it is best given in *cold* water, in doses varying from ten to twenty minims, repeated more or less frequently, according to the urgency of the symptoms.

Want of space compels us to omit any notice of Dr. Budd's observations on Perforating Ulcer of the Duodenum, and on Minute Superficial Ulcers of the Stomach. We are thus brought to the eighth lecture, on Cancer of the Stomach; and we quote, with some abbreviations, his observations on the diagnosis of this disease.

"The existence of cancer of the stomach is the more difficult to ascertain, from our not knowing its causes or any circumstances in which it is especially apt to occur. It does not, indeed, often occur before the age of thirty-five;‡ but in persons beyond this age it is met with, and with no observed differences as to frequency, in all conditions of society. . . . . In persons beyond the age of thirty-five, there are no circumstances that give unusual significance to symptoms; and until the disease has lasted some time, the symptoms have seldom any characters that are peculiar or especially significant. Pain or uneasiness, referred to the stomach and increased by food, sour eructations, occasional vomiting, and lowness of spirits—which are often the only symptoms noticed for some weeks, or even months—may all arise from simple ulcer of the stomach, and from many other conditions.

* Op. cit., pp. 412–13. † Budd, op. cit., p. 142.

‡ Andral relates a case in which a woman died of this disease at the age of twenty-two, and in whom it seemed to have begun before the age of twenty; and Dr. Budd has met with an instance of its occurrence in a woman at the age of twenty-six; but such cases are extremely rare.

"After some time, the disease is easier to detect. The circumstance, that the symptoms have continued, or rather that they have got gradually worse, in spite of a restricted diet, and of other means which usually relieve such symptoms when they are the effect of superficial inflammation of the mucous membrane, or of mere functional disorder, leads to the inference that they result from organic disease; while the progressive loss of flesh, and the faded look, and often a morbid moroseness or despondency, must excite a strong suspicion that this disease is cancer. Such a suspicion will be confirmed if much mucus, especially mucus mixed with brown or black flakes, should be thrown up from the stomach. When the mucous membrane is invaded by cancer in considerable extent, this frequently happens; but in simple ulcer of the stomach, the disease most likely at this time to be mistaken for cancer, it happens but rarely. . . . . .

"After the disease has existed for some months, and the patient is much wasted, a tumour may, in most instances, be felt in the region of the stomach. When such is the case, and when the discovery of the tumour has been preceded by the symptoms I have mentioned, and the patient is of an age when cancer of the stomach is common, little doubt can remain that the disease is cancer. Vomiting of a large quantity of matter of a soot-black or dark-brown colour, like coffee-grounds, would of course render this inference still more sure.

"In a person who has led a temperate life, the same inference may be drawn when, without any palpable tumour, the symptoms show clearly that the pylorus is much obstructed. . . . . . In distinguishing cancer of the stomach, it is also very important to consider the time the disease has already lasted, and the actual condition of the patient with reference to it. In cancer the disease makes continual progress, the patient grows gradually thinner, and, in three cases out of four, dies of exhaustion within twelve months; in a very large proportion of cases, within two years. . . . . If the disease, therefore, has existed for several years, or even for many months, without much loss of flesh, the chances are greatly against its being cancer."*

Dr. Budd's observations on the possibility of diagnosing the different kinds of cancer are sound and judicious. We are assisted in our diagnosis by the following considerations:

1. Medullary cancer grows much more rapidly, and becomes sooner and more widely disseminated than scirrhous and colloid cancer.

2. Scirrhous and medullary cancer of the stomach usually (if they extend) affect the liver; while colloid cancer more commonly leads to secondary cancerous tumours in the mesentery.

3. Medullary cancer and scirrhus cause much more pain and constitutional disturbance than colloid cancer.

Both Dr. Budd and Dr. Chambers lay down very good rules regarding the diet in these cases, but as they contain nothing particularly novel, we proceed to extract a few hints from Dr. Budd, regarding the treatment of special symptoms.

When there is an excessive secretion of mucus or gastric juice, bismuth may be administered before meals with advantage. An excess of acid in the stomach (which, by the way, often occasions a great repugnance to food) may be neutralized by lime water or magnesia. Associated with the development of the acid, there is usually an evolution of carbonic acid, resulting from a process of gastric fermentation, which may be checked to a certain extent "by brandy, and by dill-water and other aromatics, combined with bismuth, or with alkalies, when these are necessary." When there are fetid eructations, containing sulphuretted hydro-

* Op. cit., pp. 176–8.

gen, creosote pills (containing from a quarter to half a minim) may be given with each meal; or a few grains of bisulphite of soda; or some finely-powdered wood-charcoal. Finally, "to alleviate pain, and to allay general nervous irritability, the medicines most in repute, and probably the best, are conium and belladonna, which have the advantage of not confining the bowels and checking the secretions, as opium does."

The lecture which we have just noticed forms the conclusion of what may be regarded as the first half of Dr. Budd's volume—that, namely, which treats of the *organic diseases* of the stomach; the greater part of the remaining lectures being devoted to the *functional disorders* of that organ.

It has been known from time immemorial, that sympathetic disorders of the stomach may arise from irritation elsewhere.

"Functional disorder of the stomach," says Dr. Budd, "may result not from organic disease of the stomach merely, but from organic disease of other organs; and that, not by the constitutional disturbance which this disease sets up, or by any change it may cause in the state of the blood, but by an influence transmitted through the nerves. . . . . Irritation of the lung, or of the brain, or of the liver, or of the uterus, from certain kinds of organic disease, frequently leads, as is well known, to *sympathetic* vomiting—that is, to vomiting caused by nervous influence reflected from the seat of disease upon the muscles which perform this act. The matter vomited in such cases is frequently acid, even when digestion is not going on . . . . whence we may safely infer, that the reflex nervous influence excites, not merely the act of vomiting, but also, in many cases, a secretion of gastric acid." (pp. 185-6.)

This sympathetic gastric disease is very commonly seen in phthisis. In this disease, vomiting generally occurs sooner or later, and the functions of the stomach are otherwise disordered. Disorder of the stomach of a similar nature sometimes originates in the liver, especially from the irritation caused by the passage of gall-stones, or by hepatic abscess. It may likewise result from disease of the brain, especially from inflammation of that organ or of its membranes. Andral, who has investigated this subject with much care, and whose results are quoted by Dr. Budd, observes, that "vomiting, or at least nausea, very frequently attends acute inflammation of the membranes of the brain. These symptoms show themselves almost exclusively in an early stage of the disease, and they often mark its onset." Gastric disorder of this kind results very frequently from irritation or organic disease of the uterus. It has been observed, when in tying a polypus, a portion of the uterus has been included in the ligature; it occasionally occurs in women afflicted with cancer of the uterus —an effect, doubtless, of reflex nervous influence; and it is sometimes observed when miscarriage is about to take place; and in conjunction with chronic ulcer of the neck of the womb. In children, the irritation of teething is a frequent source of this variety of gastric disorder.

"The most effectual remedies for the disorder are,—

"1. Sedatives, and other means which lessen the irritation from which the gastric disorder springs.

"2. Alkalies and astringents.

"Alkalies sometimes give immediate relief, by neutralising the acid which the stomach contains: astringents alleviate the disorder rather more slowly, but for a longer time, by restraining the undue and untimely secretion.

"The insoluble antacids—magnesia and chalk—are well suited to this disorder. They serve to neutralize any excess of acid that may be in the stomach; and given under these circumstances, have an astringent action on the surface besides.

"Bismuth has a remarkable effect in restraining undue secretion in the mucous membrane, and may often be given with advantage, either alone or in conjunction with magnesia or chalk.

"When the disorder continues, the diet should consist chiefly of milk and farinaceous food, and little should be eaten at a time. Alcoholic drinks and all stimulating articles of food seldom fail to aggravate the disorder, and should be strictly forbidden.

"If symptoms indicating an inflammatory state of the stomach should occur—namely, a sense of heat in the stomach, and pain excited by food, tenderness at the epigastrium, and a white coat on the tongue, a blister or mustard-poultice may be applied to the epigastrium; and the stomach may be cooled, and be rendered less irritable, by swallowing occasionally, and especially after meals, a small lump of ice.

"If constipation exist, it may be remedied by medicines, such as pills of colocynth or aloes, which do not much offend or oppress the stomach.

"In some cases of this sympathetic disorder all these means are unavailing: nothing will stop the vomiting while the original irritation exists."*

Dr. Budd's tenth lecture is devoted to a subject which is also ably discussed in Dr. Chambers's volume—namely, Deficient Secretion of Gastric Juice. A too scanty secretion of the gastric juice may arise from various causes. According to Dr. Chambers, "There is no defect, moral or physical, so frequently handed down from parent to offspring, as the inability to form a sufficiency of this secretion." Next to hereditary predisposition, we must place overwork of the mind, prolonged anxiety, ambition, &c.; and finally, gluttony, drunkenness, and, in a lesser degree, indolent and sedentary habits, and the consumption of more food (without positive gluttony) than the system, with so little tear and wear, requires. From any of these, and probably from various other causes (as, for instance, glandular degeneration, a subject which has recently been most ably investigated by Dr. Handfield Jones), it may happen that the gastric juice is not secreted in sufficient quantity for the purpose of digestion, and that consequently indigestion will ensue.

The signs of a deficiency of the gastric juice are (according to Dr. Chambers):

1. An arrest of the food in the stomach.
2. Distress after eating albuminoid substances.
3. Decay of albuminoid substances in the alimentary canal, and consequent fetid gases arising from that decay.
4. The appearance of unaltered muscular fibre in the stools.

The food in these cases remains undigested, or only partially digested, in the stomach, for a much longer time than the normal period—sometimes for twelve or even twenty-four hours; and during this slow process there comes on a sense of weight or uneasiness at the pit of the stomach, which gradually disappears as the food becomes dissolved and escapes from the stomach.

"If (says Dr. Budd) portions of food remain undigested many hours, they irritate the lining membrane of the stomach, and cause headache, a slightly furred tongue, and feelings of general disorder.

* Budd, op. cit., pp. 205–6.

"Not unfrequently this irritation of the stomach checks secretion in the liver, so as to render the complexion somewhat sallow; and in nervous persons who dine late it greatly disturbs the sleep.

"When, from permanent weakness of the stomach, the digestion is habitually feeble, the body after a time is imperfectly nourished—the blood is poor in globules, the circulation is feeble, the extremities are apt to be cold, the spirits are depressed, and the various *powers* of the body decline." (p. 212.)

The remedies for habitual slow digestion, consist fully as much in the proper regulation of the diet and general habits, as in purely medicinal agents.

In slight cases a little comparative starvation will do no harm, but when the affection is chronic, the patient may not be in a state to bear starving; and in fact the disease might be aggravated by thus lowering the system. Under these circumstances, Dr. Chambers (p. 373) lays down the following rules:—1. To let the albuminoid food be as liquid as possible. 2. To let the quantity requisite for the day's consumption be taken at frequent short intervals; and (if it is likely to turn sour) 3. To guard it with alkalies. It is unnecessary to observe that Dr. Chambers's third rule is diametrically opposed to the ordinary mode of proceeding.

"I need hardly say (observes Dr. Budd), that when digestion is slow and feeble, care should be taken not to give, at the time of meals, or while digestion is going on, alkalies, the alkaline salts, or other medicines that suspend or enfeeble the action of the gastric juice." (p. 214.)

By following Dr. Chambers's third rule, we may get the food to pass unaltered into the intestines, and by trusting to their digestion, spare the stomach without starving the patient. We have great pleasure in directing the attention of our readers to these rules, not only because we believe them to be of sound practical value, but because they afford an excellent illustration of the direct service rendered by physiology to the treatment of disease. If it had not been for the physiological experiments of Bidder and Schmidt, and of their pupil Zander, we should never have known that the intestinal juice possesses the remarkable and unique property of dissolving and rendering fit for absorption, not only flesh and the other albuminoid bodies, but also starch —in short, that it unites in itself the powers of the acid gastric juice and the alkaline pancreatic fluid.* The following recommendations are also based on the physiological observations of Bidder and Schmidt:

"The gastric juice may in part be replaced by water drank; and hence we often find a draught of this liquid, an hour or two after meals, will remove the discomfort arising from difficult solution of meat meals. It is not merely the dilution of pungent irritating matters which is effected by these means, but the solution, and consequent absorption, of the delayed nutrimentary mass.

"The comfort arising from drinking a short time after meals, has originated the custom of tea and coffee soon after the evening dinner. The liquids taken are, however, not nearly so well suited to the purpose as pure cold water is, and many sufferers from sluggishly-secreted gastric juice will find from this latter beverage a relief which tea or coffee can never give.

"The water may be made pleasanter by being iced, without any consequent injury to digestion, for we know, from Drs. Bidder and Schmidt's experiments,

* For further information on the subject of intestinal digestion, we may refer to Lehmann's Physiological Chemistry (published by the Cavendish Society), vol. iii. pp. 514–17; and to Dr. Chambers's volume, pp. 138–43.

that the freezing temperature does not arrest the functions of the gastric juice, and probably, too, the cold gives a tone to the stomach which its torpid congested condition requires.

"Another addition to the water, not unpalatable, is a few teaspoonfuls of a solution of the superphosphate of lime, which is at the same time a condiment or fillip to digestion, and a complementary food.

"A lemon-water ice is another agreeable substitute for tea."*

The solution of the superphosphate of lime may be prepared, as Dr. Chambers informs us in a note, "by adding to the pharmacopœial solution of chloride of calcium, the rhombic phosphate of soda, and then redissolving the precipitate formed, by the addition of phosphoric acid. The chloride of sodium in the solution only makes it the more sauce-like."

With regard to medicines, Dr. Budd especially mentions ipecacuanha, rhubarb, and cayenne pepper, as substances which, by stimulating the lining membrane of the stomach, cause an increased flow of gastric juice.

"From half a grain to two grains of ipecacuanha and three or four grains of rhubarb, or a grain of capsicum with three or four grains of rhubarb, in a pill, may be taken before dinner, or before breakfast and before dinner. . . . . . As a remedy for frequently-recurring slowness of digestion, ipecacuanha seems to me to be more effectual than any of the other stimulants. . . . . . Where digestion is habitually slow and feeble, much benefit of more lasting kind may frequently be derived from the muriatic or the nitro-muriatic acid, taken for some weeks together, half an hour or three parts of an hour before the principal meals." (pp. 213-14.)

Dr. Budd concludes this lecture with a notice of some of the varieties of gastric fermentation. We extract his observations on one of the most important of this class of affections.

"Severe attacks of vomiting and purging, commonly designated English cholera, seem often the result of fermentation or putrefaction of food in the stomach, by which some highly-irritating matter is formed. If I may judge from my own experience, such attacks generally come on in the evening, soon after dinner, or at night, soon after supper, and are much more frequently consequent on a meal of meat or cheese than on the eating of fruits, to which, perhaps from the greater frequency of the disorder in autumn, they are generally ascribed. I have found no remedy so effectual in checking the disorder as pills composed of creosote and opium." (pp. 221-22.)

The eleventh lecture is devoted to the consideration of that form of fermentation in which *sarcinæ* are developed in the stomach. The remedy on which he places the most reliance—and we can personally support his judgment on this point—is the bisulphite of soda, of which a dose varying from fifteen grains to a drachm may be given, dissolved in water, two or three times a-day. In one of his cases the disorder was much mitigated by large doses of common salt.

In his twelfth lecture, Dr. Budd treats of Indigestion arising from Defective Action of one of the Excreting Organs, or from some Fault in the Nutritive Processes in other Parts of the Body. We shall attempt briefly to analyse the pages devoted to this important practical subject; and shall commence with the disorders of digestion that result from defective action of the excreting organs. It is almost unnecessary to refer to the intimate relation subsisting between the stomach and the

* Digestion and its Derangements, pp. 374–5.

liver; if the secretion of the latter be defective from any cause, the functions of the former are almost invariably disordered; and there is a coated tongue, an impaired appetite, nausea, constipation, and often lowness of spirits, with disturbed sleep. For those who are not strong enough to bear the ordinary blue-pill and black-draught treatment, Dr. Budd recommends fifteen grains of bicarbonate of soda twice a-day, with enough of the potassio-tartrate of soda to act gently on the bowels.

Again, when the kidneys imperfectly discharge their office, the functions of the stomach soon become disordered. This is especially seen in the advanced stages of Bright's disease, where nausea or vomiting is often a prominent symptom. Hydrocyanic acid, conjoined (if there be undue acidity) with small doses of potash or soda, will generally check the gastric disturbance; or, if there be much vomiting, a minim of creosote in a bread pill may be administered three times a day before meals. If these means fail, Dr. Budd remarks that the vomiting and much of the associated gastric disorder may often be stopped for a time by a few purgative doses of cream of tartar and jalap, which probably relieve the stomach by causing a more abundant elimination of the noxious matter of the blood by the bowels. The medicine is best given before breakfast,

> "Since it will then, in addition to the drain it causes from the mucous membrane, only sweep away the refuse of digestion, whereas if it be given at other times, or in repeated doses during the day, it sweeps away food that has been more or less digested, but the nutritious elements of which have not been absorbed." (pp. 248-49.)

But the blood may be rendered impure, and the stomach may be consequently disturbed, not only by the defective action of the great excreting organs, such as the liver and kidneys, but also from a fault in the processes concerned in the disintegration and elaboration of the tissues generally. One of the most important of these states of defective assimilation is that which is characterized by the formation and excretion of an excess of lithic (or uric) acid.

In persons who are in the habit of suffering from the derangement of the nutritive processes which is indicated by this symptom, indigestion is very common; it is chiefly marked by excessive acidity and heartburn, the urine at the same time generally depositing a sediment of what Dr. Budd terms lithate of ammonia, but what in reality is, for the most part, lithate (or urate) of soda. In these cases the bicarbonates of soda and potash are generally the best remedies, and the best time for giving them is two or three hours after the principal meals.

> "Fifteen grains of either, two or three times a-day, is, in most cases, a sufficient dose; and if there be a sense of heat in the stomach, this may be conjoined, as Prout recommended, with a few grains of nitre. . . . . . The use of the alkalies should be continued for some weeks, and if any gouty symptoms exist, a grain of acetous extract of colchicum may be given at night." (pp. 251-2.)

The free action of the liver and of the bowels must at the same time be kept up by occasional small doses of blue pill, and by colocynth pill, either alone or in combination with extract of henbane. Medicine is however of little use in these cases, unless exercise, diet, &c., be duly attended to. The patient should take active exercise, be much in the open air, and should be restricted to a simple and abstemious diet—eat-

ing sparingly of animal food, and altogether avoiding rich dishes, pastry, cheese, &c.

The form of indigestion accompanied with the presence of oxalic acid in the urine, is next considered, but there is nothing requiring notice in his remarks on this subject.

The thirteenth lecture treats of "Forms of Indigestion characterized by some peculiarity in the symptoms—Urticaria—Pyrosis." Nettle-rash may be produced in various ways; but its most frequent cause, and that which especially concerns us at present, is the imperfect digestion of particular articles of food. Amongst the substances that have been observed to bring it on, are shell-fish, especially crabs and mussels, pork-pie, fish, when tainted or out of season, honey, mushrooms, cucumbers, almonds, and oatmeal. The symptoms are too well known to require notice. The main object of treatment is to expel as soon as possible the offending matter. The stomach should first be emptied by an emetic of ipecacuanha or sulphate of zinc, and the bowels then cleared by a warm but quickly-acting purge. To allay the cutaneous irritation, Dr. Budd is in the habit of prescribing a lotion, made by mixing half a drachm of acetate of lead and half an ounce of tincture of opium with eight ounces of water.

In those cases in which the nettle-rash seems to be referable to several substances in common use, rather than to one special substance, it may sometimes be kept off by the administration (before dinner) of the rhubarb and ipecacuanha pill mentioned in page 44, or of a few grains of rhubarb. Dr. Budd gives a case which shows very satisfactorily the occasional efficacy of rhubarb in this disorder.

"It sometimes happens (says our author), especially in women, that the nettle-rash, though depending immediately on the stomach, occurs only when digestion is weakened by over-fatigue, or by anxiety or some other mental emotion, or by profuse monthly discharges, and that remedies of a different class are availing. In some such cases, when all the means I have before spoken of had failed, I have known the eruption disappear under the use of carbonate of ammonia, alone or in conjunction with tincture of gentian." (pp. 271-2)

Dr. Budd enters at considerable length into the causes and nature of pyrosis or water-brash. For all practical purposes, we may regard it as dependent on two general causes—namely, (1) pregnancy, enlarged liver, or some other condition that disturbs the functions of the stomach; and (2) a defective diet, the fault most commonly being, that it consists too much of farinaceous substances. In the latter case, the treatment is chiefly of a dietetic nature, and little benefit can be expected from a purely medicinal treatment.

The medicines that have been found most useful in pyrosis, are (1) *astringents*, as bismuth, lime-water, kino, catechu, logwood, &c.; and (2) *sedatives*, especially opium and the salts of morphia. Medicines of these two classes may often be combined with advantage. As illustrations of such combinations, Dr. Budd mentions five grains of bismuth with a twelfth of a grain of the muriate of morphia, or five grains of the compound kino powder, or an efficient dose of catechu, krameria, or logwood, with opium, to be given before meals, two or three times daily.

Other medicines have at different times obtained a reputation for curing

pyrosis, which cannot be strictly classed either with astringents or sedatives; as, for instance, "nitrate of silver, which may be given in pills, in doses of half a grain, three times a-day; nux vomica, which may also be given in pills, in the dose of from three to five grains, three times a-day; quinine; and the mineral acids." (p. 282.)

When, as is often the case, the pyrosis is connected with anæmia, steel is of great service, both in removing it and preventing its recurrence.

Dr. Budd next describes "another kind of gastric disorder which is closely allied to water-brash, and which is characterized by paroxysms of violent pain in the stomach, often described as spasm, that comes on when the stomach is empty." (p. 282.) During the paroxysm the epigastrium is tender on pressure, the pulse becomes very slow, and the surface of the body cold. The pain is generally relieved by taking food and by lying down. The appetite in the intervals of pain is often good, and the digestion apparently normal; the tongue is usually pale and flabby, but clean. In most cases the patient is weak, and sleeps soundly.

This form of gastralgia seems to be induced in men by family cares, or anxiety in business, or over-fatigue, and is most frequent between the ages of thirty-five and fifty; in women, it is most frequently the effect of profuse monthly discharges. It may continue for two or three months, and is very likely to recur in persons who have once had it.

We extract the following notes, describing a case of this sort occurring in a medical man, aged between thirty-five and forty years:

"About three hours after each meal—or perhaps about four or five hours are nearer the mark—and as soon as digestion appears to be over, he is seized all at once with severe pain in the stomach, which soon becomes so intense as to depress the circulation in a very remarkable way. The pulse falls to thirty-five in a minute; he becomes deadly pale; his hands and feet grow cold; and all his strength appears to have left him. This state of things continues until he gets something to take in. But the very moment he swallows anything—and what is remarkable is, that the effect seems to be quite independent of the nature of the aliment,—he gets complete relief, until the period comes round when the stomach is again empty. In the absence of food, the pain is much assuaged by his assuming the recumbent posture. For the rest, his appetite is perfectly good; he does not suffer at all from thirst; and his bowels are regular as the day. The evacuations perfectly healthy in appearance. At times the stomach becomes enormously distended with wind, but this is not generally the case at the time of the paroxysm. He never vomits; never has nausea, water-brash, or heartburn. Sleeps well." (pp. 283-4.)

Since the time when the letter was written (the summer of 1846), the gentleman to whom it relates has had several illnesses of the same kind, most of them brought on by fatigue. Nothing is stated by Dr. Budd regarding the treatment in this particular case, but the medicine which he has found of most use in this disorder is hydrocyanic acid, given in full doses; opium is likewise of service, but generally ess effectual than the hydrocyanic acid. The patient should live on a light but nutritious diet, have regular meals, and plenty of sleep; and should avoid fatigue, and all causes of excitement.

The next kind of gastric disorder we have to notice is what may be termed the indigestion of drunkards, often the combined result of drink and of exhaustion from want of proper food.

When it results solely from spirit drinking, its chief characters are—

"Want of appetite, and vomiting or dry retching in the morning, with a white or furred tongue, and a slow pulse. The power of digestion is much enfeebled, and if the patient eat at any time what for others would be a very moderate meal, he is apt to vomit soon afterwards, and to be troubled by pain in the stomach, and flatulence. . . . . This disorder, like the vice from which it springs, is most frequent in men of middle age, and is generally associated with more or less of that strange and peculiar disturbance of the nervous system which hard drinking brings on, and of which the most striking effects are inability to sleep, or sleep broken by frightful dreams, despondency in the morning, and tremulousness of the hands and tongue." (p. 286.)

This kind of disorder is very common among the poor of large towns, and sometimes is so severe as to lead to the suspicion that organic disease is present.

The most efficient remedies are bitters, opium, and solid food. Gentian, quassia, and calumba may be taken, singly or combined, in the form of tincture, two or three times a-day, an hour before the principal meals. With these bitters, small doses of opium or of morphia may be very advantageously combined, so as to tranquillize the nervous system, procure sleep, and settle and strengthen the stomach. It occasionally happens, however, where the gastric disorder is severe, that very large doses of opium are requisite. Dr. Budd describes such a case, in which no decided amendment took place during a four months' residence in King's College Hospital, till half a grain of muriate of morphia and five minims of dilute hydrocyanic acid were given every four hours.

"This soon stopped the vomiting, and gave him a full measure of sleep, and the appetite returned. He continued to take the morphia in very large doses, in conjunction with hydrocyanic acid; steadily grew stouter and stronger; and at the end of some weeks left the hospital to resume his accustomed labour." (p. 290.)

In all these cases it is essential that the patient should eat as soon as possible some solid nourishing food.

The lecture concludes with a brief reference to that comparatively rare form of gastric disorder in which digestion is tolerably good in the morning, so that a substantial breakfast causes no discomfort; while a hearty meal eaten in the after part of the day is followed by flatulence and gastric pain, which sometimes ends in vomiting. It is usually observed in old worn-out people. With regard to treatment, half a drachm of aromatic spirits of ammonia, or four grains of carbonate of ammonia, may be given three times a-day, and from four to ten grains of bismuth before dinner. Breakfast should of course be made the principal meal in these cases.

We may pass very rapidly over the last three lectures of Dr. Budd's volume. They are devoted to the "symptoms of stomach disorders—namely, pain and soreness of the epigastrium, vomiting, excessive acidity, and flatulence;" and to a notice of "some of the remedies for stomach disorders—namely, ipecacuanha, bismuth, the vegetable astringents, hydrocyanic acid, the alkalies, the mineral acids, the vegetable bitters, the preparations of steel, and purgatives." The last lecture concluding with a few pages containing "general rules of living." They are excellent lectures for students, but might have been omitted, or at all events much

abbreviated, in a work addressed to the medical practitioner. In his remarks on "excessive acidity," there are some statements of (we should think) very doubtful accuracy,—as, for instance, that the gastric glands secrete carbonic acid, and that oxalic acid may be generated in the stomach.*

The following observations on the vegetable astringents are valuable:

"Chalk, and the vegetable astringents,—kino, catechu, krameria, and logwood, —are generally given to restrain diarrhœa. It does not seem to be generally known that they are just as effectual, perhaps more effectual, in restraining undue secretion from the stomach. Chalk, like bismuth, from its sparing solubility, has little direct action, except on the mucous membrane over which it passes. The vegetable astringents have a more remote astringent influence. This is clearly seen in the colliquative stage of phthisis; where, besides restraining the diarrhœa and stopping the vomiting with increased secretion of gastric juice that often occurs in this state, they restrain, often in a very striking degree, the profuse sweating. They seem all to have much the same effect. I generally give the preference to krameria and logwood. Kino is not conveniently given in solution; and catechu is not only very nauseous, but, from being much used in the arts, is often of inferior quality. The most grateful to the taste is krameria; the most effectual, I believe, is logwood. Logwood has a mawkish taste, which is best corrected by cinnamon. An ounce of logwood shavings, and a drachm and a half of powdered cinnamon, may be infused for four hours in ten ounces of boiling water, and then strained. An ounce and a half of the strained infusion may be given two or three times a day, a short time before meals." (pp. 333–4.)

With regard to the employment of the alkalies he observes, that—

"It may, perhaps, be adopted as a maxim, that alkalies, given to exert their constitutional effect, are most frequently useful to persons who have dry skins and perspire little, and eat largely of animal food and live in towns; that acids are most frequently useful to persons who live in the country, eat largely of vegetable food, and perspire much. If there be one symptom more than another that suggests and justifies the use of soda, it is a furred or coated tongue." (p. 340.)

We shall conclude our notice of Dr. Budd's volume with an extract from his remarks on the vegetable and mineral tonics:

"The most important medicines of this class are the vegetable bitters—quinine, gentian, calumba, strychnine—and the different preparations of iron. Quinine, and the bitters generally, are especially grateful to persons who have injured their stomachs by hard drinking. With such persons they improve the appetite and strengthen digestion, and have a bracing effect upon the system at large. In persons exhausted by over-work, or wherever weakness of the stomach is the result of general debility from other causes, they often do much good in the same way—*by improving the appetite* and strengthening digestion. They do harm in organic diseases of the stomach; in plethoric states of the system; and generally where there is a furred tongue, or where the urine throws down a sediment of lithic acid or of lithate of ammonia. Their most striking effect is, to improve the appetite, when this has been impaired from hard drinking, or from over-work, or from nervous exhaustion from other causes; and the best time for giving them is from half an hour to an hour before meals. The different bitters have not precisely the same effect. Calumba has a sedative influence not possessed by the others, and probably on this account has had a wider reputation as a remedy for mere indigestion. Gentian and chiretta (which is of the gentian tribe, and is much employed by practitioners in India) tend to increase the secretion of the liver, or at any rate do not impede its secretion, which quinine and quassia seem often to do. They

* See p. 306.

are therefore better suited to bilious persons, and to those cases of indigestion where the secretions of the liver are defective. The different preparations of steel are especially useful in the indigestion that occurs in chlorosis, and generally where weakness of the stomach results from anæmia. They do harm in plethoric states of the system, and generally where there is a furred tongue, or where the urine throws down a sediment of lithate of ammonia or of lithic acid. The citrate, or ammonio-citrate, is the most agreeable preparation to the taste, and generally the most grateful to the stomach. If there be any disposition to sickness or nausea, or any tendency to furring of the tongue, it may be given in conjunction with the bicarbonate of soda or potash. This makes a mixture having much the same effect as Griffiths' mixture—the mistura ferri composita,—and far more agreeable. The muriated tincture of iron is more astringent than the other preparations, and may be given in conjunction with dilute muriatic acid, in the forms of indigestion suited to this latter medicine, when these exist in states of anæmia. The sulphate of iron, like the other metallic sulphates, has a tendency to cause sickness, and should not be given in cases where a disposition to sickness exists. Steel medicines do good by improving the quality of the blood rather than by their immediate action on the coats of the stomach, and are best given at meal-times. They then are mixed with the food, and gradually absorbed with the products of digestion, and are less apt to offend the stomach and to cause headache than at other times. Whenever steel medicines are given, it is essential that a regular action of the bowels be kept up. These medicines tend to confine the bowels and to cause evolution of sulphuretted hydrogen in them; and, unless this tendency be counteracted, they are apt to fur the tongue and cause headache." (pp. 343–5.)

We now turn to the concluding hundred pages of Dr. Chambers's volume, which treat of subjects to which Dr. Budd has hardly at all adverted, and we shall commence with the consideration of the "Morbid States of the Small Intestines interfering with Digestion."

Dr. Chambers first considers those abnormal states which exhibit a *deficiency of intestinal absorption*, and then those in which there is *defective excretion.*

Defective absorption may occur (1) in general disease—as, for instance, in the later stages of continued fever; (2) in cases of chronic ulceration, which especially occurs in tubercular persons; (3) in mucous flux of the intestines; and (4) in intestinal struma. We shall briefly notice each of these conditions, especially in reference to treatment; and commence with "the rational and physiological view of the treatment of acute fevers through the digestive organs."

"In these cases (says our author) there is either introduced or generated, or both introduced and generated, in the body a poisonous substance foreign to its tissues; and it seems most proper that this should be evacuated before there is introduced any more fresh matter than is absolutely necessary to keep life up. . . . . For this reason, in acute fevers it is of the highest importance to watch for the time when destructive metamorphosis begins to return. Up to that moment any food we give with a really nutritive intention is either useless or noxious; and any complementary food which arrests metamorphosis is [causes?] a postponement of the favourable turn of the disease. Decoctions of starch, infusions of gum, and fluid gelatinous drinks, are possibly beneficial, by shielding the mucous membrane from irritating substances; but they are not likely to be absorbed, and their chief use is as a placebo to the patient and the friends, who dread starvation.

"How far we should interfere to *promote* destruction, by giving neutral salts, water, purgatives, mercurials, &c., must be a question to be decided by the peculiarities of each individual case. . . . . In almost every instance I am inclined to

think some of these agents beneficial; and among them I would call the attention of practical men to one undeservedly neglected in the present day—viz., the administration of emetics at the onset of fevers. I am sure that by this powerful agent the first period of the fever—the period before the destructive metamorphosis commences—is much shortened, and the subsequent violence of the phenomena abated. The secretions of the alimentary canal, arrested in the mucous membrane, and subjected to chemical decay there, are cleared away, and whatever poisonous or noxious matters may have been received intò the hollow viscera, are removed. Sometimes the recovery of destructive metamorphosis follows immediately on the remedy, and is therefore so slight that the attack is said to be cut short; but more commonly only a temporary stimulation of the vital powers occurs, and the natural latter part of the course of phenomena is gone through.

"The recommencement of destruction is ànnounced by the increased amount of solid matter in the fæces, or the increased specific gravity of the urine or perspiration, with the simultaneous alteration of countenance by the falling in of the features, and other evidences of commencing emaciation. This is the time to let a continuous stream of digestible nutriment begin to flow through the alimentary canal, to be taken up at the auspicious moment when the absorbents are to receive it." *

In his remarks upon the treatment in cases of chronic ulceration of the small intestine, Dr. Chambers lays great stress upon the danger which patients threatened with tubercle before the age of thirty years† run in removing to warm climates—as, for instance, Madeira, the Azores, or Cairo. The risk of injuries to the bowels from the diarrhœa so often induced by the change of food and climate, counterbalances the chance of good for the lungs. Sulphate of copper, in combination with opium, is, in his experience, the most powerful means of checking the diarrhœa which so often occurs in tubercular patients with ulceration of the small intestines.

Our limited space precludes us from noticing his excellent description of the symptoms of "intestinal mucous flux." Children seem more liable to this affection, as well as to the worms which so frequently accompany it, than adults, and females than males.

"The causes of this disorder are usually to be traced to continued low temperature, united to damp, or rather to a want of the occasional variation of a warm dry air; sedentary habits, especially when united to defective air; and in the upper classes, anxiety and intense occupation of mind in study, by impeding proper digestion of the food, are frequently joint causes. And when once the disorder has commenced, there is no more powerful aggravator of it than continued mental exertion on one subject." (p. 464.)

In the treatment of this affection, our author lays far more stress on hygienic measures than on purely medicinal agents.

The bowels having been well cleared out by a free dose of oil of turpentine, or, if the patients rebel at the turpentine, by a few doses of calomel and jalap, we must, if the circumstances of the invalid allow or it, recommend travelling, or, if a complete absence from home cannot be

* Chambers, op. cit., pp. 454–5.

† Dr. Chambers shows, in his Decennium Pathologicum, that the per-centage of those attacked by ulceration of the small intestines in connexion with tuberculosis of the lungs, in the fatal cases at St. George's Hospital, was

from 15 to 30 years of age	33·6 per cent.	
„ 30 to 45 „	23·8 „	
„ 45 to 60 „	18·0 „	

obtained, riding on horseback. We cordially agree with Dr. Chambers in thinking that

"No directions for a tour can be so judicious in these cases as the picturesque parts of our own land: Cumberland, Wales, Devonshire, and Cornwall afford invaluable opportunities for a mixed saunter in carriage, on foot, and on horseback, according to strength or inclination, without the risk of foreign diet." (p. 466.)

His observations on the importance of horse exercise, and of a due amount of sleep, in this disease, are well deserving of the best attention:

"A rapid change in the appearance and smell of the fæces follows the use of *riding*, and that is soon succeeded by restored health and strength. The gentle shaking motion probably induces more active secretion in the liver, and absorption in the intestines at the same time, by hastening the circulation; for the fœtor of putridity, which it is the business of the bile to prevent, and the undissolved muscular fibre, which it is the business of the bowels to take up, disappear simultaneously from the stools. Though with bad external piles it is very inconvenient to ride, yet in mild cases very great relief is often obtained, and I do not think that hæmorrhoidal tumours should ever contra-indicate at least a trial of horse exercise. The value of *sleep* is sadly under-estimated in chronic cases by medical men. . . . . There is probably no disorder in which this is so important as in mucous flux of the intestines, and I have known the expedient of lying in bed till ten o'clock in the morning make treatment effective which previously had been perfectly useless." (p. 467.)

We shall give, in as condensed a form as possible, our author's views on the treatment of intestinal struma as it occurs in children, and the strumous dyspepsia of adults. The two great points to be considered are (1) to supply such a diet as is most capable of being absorbed under the circumstances; and (2) to increase the activity of the vital functions by a combination of those medicines which promote growth, and those which promote destruction.

(1) A nutritious animal diet, given frequently, in small quantities at a time, may be commenced with advantage at any period of the disease, and persevered in throughout. Broths and flesh-teas, slowly cooked at a low heat, with the addition of a few drops of hydrochloric acid, answer well when solid meat excites nausea, although they are probably not more digestible than good old mutton, plainly cooked. "Nothing takes a child's fancy so much as small birds—a lark or a blackbird will often be eaten with pleasure when other things excite disgust; and (perhaps from the gratification of imagination) is easily digested." Milk is seldom borne well in its natural state. The best way is to partially skim it, add one-third part of lime-water, and some sugar; it then forms an excellent ordinary drink to be taken at meals. Soda-water and cream, mixed in the proportions most agreeable to the patient, form a good draught to be taken the first thing in the morning. Bread and potatoes constitute the best vegetable food. If, in the case of adults, any alcoholic drink is required, that which, according to our author's experience, usually agrees best "is Bordeaux wine, of recent vintage, so as to be sound, free from acidity, and with the agreeable roughness of the fresh grape still remaining, to act as a tonic to the mucous membranes." (p. 474.) Burgundy and port stand next in value; sherry, Madeira, and malt liquors do not usually agree, and distilled spirits are still more objectionable.

"In this state of the system," says Dr. Chambers, "sugar will often be found a very digestible, and when digestible a very useful, article of diet, especially for children. Its addition to cows' milk produces a nearer resemblance to the natural nutriment of the young of our species than can otherwise be made. Spread on bread, it is usually relished much; and given quite alone, in syrup, toffee, and the like, often increases the appetite. Whether it acts as a complementary food or merely as an accessory, or both, it certainly augments the quantity of matters assimilated by the body under the circumstances of the disease now spoken of. I have had little patients, affected with strumous derangement of digestion, increase in weight to the extent of from two and a half to four pounds in the first week after commencing the use of an additional four ounces* of sugar to the daily diet.

"Where there is joined to strumous mal-digestion a tendency to disease of the osseous system, to rickets, or softened bones, sugar is particularly indicated by its power of arresting the metamorphosis of the bones;† and Dr. Böcker has found great advantage from joining to it an additional quantity of phosphate of lime to that which is ordinarily found in a bread-and-meat diet." (p. 475.)

2. First amongst the drugs, Dr. Chambers places cod-liver oil; and he prescribes it in a different manner from that in which it is usually ordered.

"I usually," he observes, "order hours for taking the oil to be selected as far as possible removed from the ordinary meal-time. I find also that it is a good plan to make the dose still more of a separate meal by washing it down with a wineglass of half-and-half milk and lime-water, or milk and soda-water, with a mouthful of biscuit, so as to neutralize any rancidity in the oil, and add some more matter for absorption." (p. 476.)

We have ourselves prescribed this medicine very largely, and in almost every possible manner, ever since Dr. Bennett published his treatise upon it (in 1841); and we have certainly found that, as a general rule, it causes far less inconvenience to the patient when taken immediately after meals than on a comparatively empty stomach.

Steel is far preferable to vegetable tonics; and the finely-levigated *fer porphyrisée* of the French Pharmacopœia is especially recommended:

"At the same time, with these medicines, given with the design of increasing the building and formative functions, there should be administered those which augment the corresponding powers that make up life between them—the increase of metamorphosis. A full allowance of water and salt in the diet is desirable; and sea-bathing is beneficial on the same ground." (p. 477.)

Iodine—we presume Dr. Chambers means iodide of potassium—is also useful as an increaser of metamorphosis; the most powerful, however, of these agents is mercury, which must be prescribed very cautiously, lest it outruns the tonics we are administering, and causes destruction to exceed growth. The form in which he recommends that it should be given is hydrargyrum cum cretâ, in one or two-grain doses, with double the quantity of sesquicarbonate of soda, and a grain of powdered rhubarb; and its use should be suspended as soon as the fæces present a normal appearance.

We now arrive at the second portion of this chapter, in which the

* "A quantity larger than is necessary or desirable, but given in the instances above mentioned for the sake of trial."

† The experiments of Dr. Böcker (Beiträge zur Heilkunde, vol. i. p. 38) distinctly show that sugar has a greater power than any other known substance in restricting the waste of the body by decomposition, and that its effects are most marked on the products of the decomposition of bone, the earthy phosphates in the twenty-four hours' urine being lessened by more than one-half when sugar is taken. See the Physiological Part of Dr. Chambers's volume, pp. 240–5.

subject of *defective excretion* is discussed. Dr. Chambers lays down very distinctly the difference in the symptoms produced by a defective secretion or discharge of bile, and a general defective excretion from the intestines, which we are now considering. In local disease of the liver or gall-bladder, there is jaundice of the external surface, accompanied by clay-coloured stools, whenever the flow of bile is impeded, and no change of colour so long as it is free. In general defective excretion from the intestines, there is a dinginess and darkness of complexion, but none of the yellow-green of jaundice, and the stools are scanty but not clay-coloured.

"In every case of arrested intestinal excretion, the bowels are costive and the stools remarkably scanty—usually dark, hard, and dry; the urine from time to time deposits copiously lithate of ammonia, and is often dark-coloured, but not otherwise unnatural. Loss of excretive power in the intestines in some cases leads to a remarkable form of paralysis. The muscles of the lower extremities gradually, by almost imperceptible degrees in general, lose the power of progression. It is not so much that they cannot move, but that they move with excessive slowness; and they are not, as in ordinary paralysis, incapable of sustaining the body in the upright position: the patient can stand pretty well, but cannot go. There is not, moreover, any deficiency of sensation. Sometimes the derangements of digestion, gastric and intestinal, which had preceded this paralysis, vanish on its supervention, but this is not always the case. This partial paralysis is most frequently observed in Europeans returned from an Indian climate, and, I have understood, is by no means uncommon in Mexico and South America. But I have seen it in those who had never left England; and its greater frequency in the victims of temperature is probably simply occasioned by the greater frequency of its true cause—viz., the loss of intestinal digestion in those parties." (pp. 481-82.)

How are we to determine that this loss of power in the lower extremities is not dependent on lesion of the spinal marrow? It is unnecessary to say that this is a point of the greatest importance, because the affection of which we are now speaking is usually amenable to treatment, while the spinal disorders with which it may be confounded are almost always incurable.

"The most marked difference between the two lies in their history. This form of paraplegia comes on exceedingly gradually, so that it is very difficult for the patient to date, not only the commencement of the disorder, but any period at which it has grown worse; while spinal disease has, if not a sudden beginning, yet always sudden aggravations from time to time, and after each aggravation the patient is decidedly worse than at any previous day. The history also, and the present symptoms of deficient intestinal excretion, can guide the practitioner to a diagnosis." (p. 483.)

The treatment of defective excretion from the small intestines is discussed with greater brevity than we could desire. The application of leeches to the anus is of much service in this affection.

"Two or three leeches at night, followed by a tepid sit-bath, should be the maximum dose; but it may be repeated every night or every other night for a fortnight or three weeks together, if the general strength and the making of wholesome blood be at the same time promoted by an easily digestible diet. It is remarkable how the freshness of complexion and the transparency of the skin will gradually return, and often weight be gained, during the employment of this practice." (p. 485.)

The value of hydropathy in deficient intestinal excretion is then alluded to. Our author regards it as a very doubtful remedy, and would

only recommend it in very special cases, and then with great caution; in some cases it has seemed to bring on the paralysis to which we just now referred. The activity of metamorphosis is more safely augmented by the judicious use of mineral waters. "A saline, at first alone, and then mixed with a chalybeate, both of moderate strength, accomplishes the purpose." Waters containing sulphureous gases should be avoided in these cases.

There is a chapter on "Flatulence," from which, if our space had permitted it, we should have made two or three extracts. We must confine ourselves to his remarks on the treatment of *fœtid flatus.*

"Flatus, of which the smell is not that of healthy fæces, but of decomposed organic matter, indicates that the duty of the physician lies in restoring some decided deficiency in the process of digestion. Most commonly it is the liver and small intestines which are wanting in activity, so that the stools are scanty, slimy, and irregular. The most effectual remedy is mercury, in small doses, combined with light vegetable tonics—such, for instance, as a nightly powder of hydrargyrum cum cretâ, and a dose of decoction of sarsaparillæ, or infusion of gentian, or of red bark, thrice a-day. The quantity of unabsorbable woody fibre which is contained in these vegetable medicines appears to be an advantage, as it carries the active portion of the drug lower down the alimentary canal, and perhaps also is useful in forming a more bulky fæculent mass. The dietary should be sparing, and all difficultly soluble albuminous articles should be avoided: such, for instance, as solid white of egg, or meat pastry. Warning also should be especially given against food in a state of half decomposition, such as game long kept, stale fish, ripe cheese, medlars, sour beer, half-fermented champagne, &c. These not only decompose themselves, but cause all that surrounds them to decompose too, turning wholesome victuals into noxious poison." (p. 506.)

"Digestion and its Derangements" is one of the most interesting and instructive works of its kind that we have read; and we believe that Dr. Chambers has done good service to medical science in showing, as he has done, the intimate relation which exists between physiology and practical medicine. We have little doubt that a second edition will soon be called for; and we would suggest that the author might then advantageously omit certain portions of the chapters on the "Physiological Action of Substances submitted to Absorption in the Alimentary Canal," and on "Regimen," and reproduce them in an enlarged form as a "Treatise on Diet and Regimen." None of the existing English works on this subject approximate to the present state of our physiological and chemical knowledge; and we know of no one better qualified than Dr. Chambers to undertake such a task. A well-written scientific book on this subject would be equally serviceable to the practitioner and to the patient.

To those of our readers who wish to make themselves thoroughly acquainted with the anatomy and physiology of the stomach and intestinal canal, we can cordially recommend Dr. Brinton's Article in the Cyclopædia of Anatomy and Physiology. The only reason why we have not noticed it more fully in the preceding pages, is, that we wished to confine ourselves closely in this article to the diagnosis and treatment of gastric and intestinal disorders.

G. E. Day.

Review III.

Researches on Colour-Blindness. By George Wilson, M.D., F.R.S.E., Regius Professor of Technology in the University of Edinburgh.—*Edinburgh*, 1855. pp. 180.

Since the publication in 1794, by Dalton, of his own case, the occasional occurrence of the peculiarity of vision called "colour-blindness," by Dr. Wilson, has been well known to the world.* It has been regarded as a curious phenomenon, worthy of being made the subject of interesting inquiry and of amusing curiosity, even by those who experience it in their own persons, but not of any practical importance. It is true enough that those educated classes to whom the phenomenon is likely to be familiar are rarely placed in positions where their capability of distinguishing colours is of much importance to themselves or others; for a superior intellect finds no difficulty in supplying the want of acuteness of external sense, so that the deficiency is scarcely felt. It made but little difference to such men as Dugald Stewart, Dalton, or Sismondi, to be ignorant whether the ink by which they conferred imperishable inheritances on their race was red, green, or black; the same deficiency would not interfere with the legal acumen of the barrister, or the successful practice of several medical men quoted by Dr. Wilson. But our author has the merit of being the first to call attention to the fact that, in several of the lower walks of life, this bodily imperfection not only incapacitates the individual from following his calling with success, but sometimes may imperil the lives of others as well as his own. Not only does the weaver, the tailor, the gardener, thus affected, commit mistakes, ludicrous enough to the bystanders, yet likely to lead to the starvation of the poor man who by them loses his work; but the railway guard or pointsman, and the sailor, especially in steam service, may, by misunderstanding coloured signals, cause the death of thousands. We think, therefore, much gratitude is due to the Professor of Technology for thus calling attention to the subject, and investigating it with the industry exhibited in the volume now reviewed.

Three forms, or rather degrees, of colour-blindness are distinguished by the author.

1. Inability to distinguish between the nicer shades of composite colours, such as browns, greys, and neutral tints.

2. Inability to distinguish between the primary colours, red, blue, and yellow; or between these and the secondary and tertiary colours, such as green, purple, orange, and brown.

3. Inability to discern any colour, properly so-called, so that black and white (i. e., light and shade) are the only variations of tint perceived.

The *first*, or lowest, degree is *apparently* the rule rather than the exception among persons of the male sex, whose eye has been as little educated as is usual in our unæsthetic community. Dr. Wilson found that of sixty persons attending the Chemical Class of the Edinburgh Veterinary College,

* Before that essay, only occasional notices of the phenomenon had occurred. These are collected by Wartmann, a translation of whose paper may be found in Taylor's Scientific Memoirs for 1846, p. 162.

the great majority declined to give names to any colours but red, blue, yellow, green, and brown. Without care, one might easily be led into thinking colour-blindness to be much more common than it really is. But where the subject has so seldom entered the mind, and where the nomenclature consequently is so defective, it is better to dispense altogether with the *names* of colours in testing cases of this affection, and to set the task of arranging pieces of cloth or skeins of worsted in such a way that the same colours and shades should be classed together. When this is done, it will frequently be found that those who make no mistake in matching full tints of the primary and secondary colours, err in certain of the fainter shades of both, and in all the shades of some of the more mixed colours. Thus the difference between pink and pale blue is a puzzle to many who do not otherwise confound colours.

"Mr. Crombie, dyer, Brown-street, Edinburgh, informs me of three persons known to him, connected with dyeing, to whom the tints in question were a constant source of mistake. Messrs. Grieve, late of South Bridge, had in their employ a person who could match all colours but drabs. Professor S. is never certain, even by daylight, of the difference between blue and green; and many persons confound pink with pale yellow."

Where this slight degree of colour-blindness is congenital, it is just as incurable as the more marked forms. But still it is practically convenient to separate it from them, because up to this point it may be simulated by a want of discrimination, which is simply the result of deficient education, and which, therefore, attention is quite capable of removing.

The *second* form is that which Dr. Wilson has most investigated, and has cited in detail a great number of marked cases. In it, red, blue, yellow, purple, orange, green, brown, &c., are respectively mistaken for one another, or confounded together. In less severe cases, the majority of colours are seen accurately, but two at least (as red and green), and generally four (as red, green, olive, and brown), are not distinguished from each other.

There is a considerable difference between the colours as regards their liability to be mistaken for one another; and curiously enough, it is those which to ordinary eyes appear most violently contrasted, that are confused together, whilst tamer and less vivid tints are correctly appreciated. Thus, while yellow is almost universally recognised, and rarely mistaken for its complementary purple, red, however glaring, is constantly confounded with green, very often with black, and for some appears to have absolutely no existence. Blue, when pure, is usually detected pretty readily, and also yellow, as above mentioned; yet their combination, green, is one of the greatest stumbling-blocks to the colour-blind.

The *third* variety, where all objects are to the eye mere light and shade, and where absolutely no difference between colours can be detected, is extremely rare, and no instance appears to have come under the observation of the author. One, however, which he describes, is a near approach —viz., of the physician (Case VIII.), who confounds *all* colours equally by daylight, though by gaslight and transmitted light he is able to sort them rightly.

The disadvantages of this defect, even in its slightest degree, we hold to be something positive, and worthy of consideration. It is surely a mis-

fortune to miss one of the purest and seldomest-abused pleasures which God has given us: but independently of that, it obviously renders a man less fit for a great number of positions in life to which inclination and circumstances might otherwise lead him. Mourning warehouses are, doubtless, a great resource for the unfortunate haberdasher who matches red with black—but what is to happen to the house-painter accustomed to have his colours mixed by his wife, when he becomes a widower? We have ourselves interceded for a country carpenter who painted half a railing leaden-grey to match a green, and saved him from losing his job as well as his paint; and how a barrister would annihilate the medical witness who could not distinguish an arsenical precipitate by its tint! Dr. Wilson's stories of the tailor who sewed a black coat with red thread —of the chemical lecturer who always had to ask, instead of telling, his pupils the coloured reactions in his experiments—of the physician to whom *scarlet* fever had no existence—of the gentleman who condoled with a female friend dressed in vivid green, supposing her to be in mourning—of the Quaker who bought a bottle-green coat for himself and some scarlet merino for his wife—of the school-girl attempting to arrange the colours in her drawing by the taste, &c., &c., are striking and amusing instances of the difficulties consequent on this defect. One wonders how he managed to come across so many; but that is explained by his having advertised in the 'Athenæum' the fact of his being engaged in these researches, and obtaining in answer the details of their cases from the sufferers themselves.

But of more importance is the consideration that the use of colour in railway and naval signals sometimes places the lives of hundreds on the chance of the interpreter having as full a conception of the chromatic tints, as the captain or the board of directors; and the first discovery of the defect may be at a grim inquest which it has given rise to.

In the Admiralty notice respecting lights to be carried by sea-going vessels, to prevent collision, which is at present in force, the order is that all British steamers are to show, "between sunset and sunrise, a *white* light at the foremast head, a *green* light on the starboard side, and a *red* light on the port side." When, then, one vessel is crossing another's bows, the foremast light goes for nothing, and the steersman of the latter is expected to know by the colour which side of the crossing vessel is towards him, and consequently if it is going to his right or his left—and on a dark night has no other guide whether he should port or starboard his helm. A similar principle is applied, both by night and day, in railway signals.

It may be suggested that it is not likely that an individual should be so long ignorant of the partial want of a sense, and should not be aware of its absence, till called upon in adult life to exercise it. But Dr. Wilson gives several instances of this obtuseness of their own fault in the colour-blind: one especially of a tailor, who never discovered it till he was promoted to be foreman, and had to match cloth for the journeymen, when he distinguished himself by providing a scarlet livery with green strings, and informing a customer that a red and blue stripe was all blue. He had, of course, worked at his trade for many years before being raised to his responsible post, and was, in fact, known for his excellence as a cutter.

It may also be stated, that no case of accident on rail, road, or water, has as yet come before a court of justice, and therefore that the fear of it is a phantom wall. Possibly—but possibly, also, the phenomena may, if thought of and investigated, have explained the strange obtuseness and apparent negligence in officials which has often puzzled juries.

These many inconveniences are not, however, without some advantages to weigh against them. The deficiency in the sense of *Colour* appears to allow fuller scope to the due appreciation of *Form*. In point of fact, even the most healthy eye can seldom take in both these ideas in perfection at the same moment. The late Mr. Dewint, than whom none has contributed more to the love of external nature in our generation, used to teach his pupils that, for the full understanding of any objects, two drawings were necessary—one showing the colours, and the other the form; and that this arose as much from the mental constitution in the beholder, as from the imperfect skill of the artist. The visitors to the Universal Exhibition of Pictures at Paris last year, who enjoyed the unique opportunity of comparing in masses the æsthetic perceptions of each nation, could not fail to remark how the harmonious luxury of colouring in the British gallery was united to drawing very inferior to its neighbours; and how the unequalled delineation of form by the French school with difficulty removed the sense of pain occasioned by chromatic errors. To pass rapidly from either to the other gave a most unpleasant jar to the feelings. It is to be expected, therefore, where there is the extraordinary partial deficiency of the sense of vision which we are considering, that a compensation should exist in the greater acuteness of other parts. And such seems to be the fact. Numerous proofs are to be found in Dr. Wilson's pages. Among the colour-blind, Case III. was the *prize student* of his class as a drawer from nature. Case IX. says, "I find, at times, many of my brother engravers in doubt how to translate certain colours of pictures, which to me are matters of decided certainty and ease. *Thus to me it is valuable.*"

Mr. N., of Torquay (Case X.), thinks his *colour-blindness an advantage to him when engaged as an amateur artist.* "In crayon-drawing I believe I have, in consequence of this defect, a more just appreciation of light and shadow, and the value of chiaro-oscuro in composition." It is remarked, also, that the tailor above quoted keeps his place as foreman, in consequence of his excellence as a cutter, in which occupation of course an eye for form and outline is the prime requisite.

The number of the colour-blind is larger than has been generally supposed. From a table exhibiting the results of the examination of 1154 persons of various classes, at Edinburgh, in 1852–53, it may be inferred that in the Northern metropolis 5·6 per cent. (or more than 1 in 18) are thus defective. Of this 5·6 per cent., 1·8 per cent. (or 1 in 55) confound red with green; 1·6 per cent. (or 1 in 60) confound brown with green; 2·2 per cent. (or 1 in 46) confound blue with green. Dr. Wilson remarks that the distribution of these numbers among different classes of the population is "most capricious," and makes no attempt to trace it as peculiar to any. We would, however, call his attention, in future researches, to one strange fact appearing on the face of his tables—viz., that the very highest per-centages are among those whose circumstances

may be supposed to have best enabled them to overcome natural defects. The largest is among the medical attendants at the Royal Asylum, Morningside; and the next, his own pupils. This proves at least that education is not likely to do much for its removal. That a high appreciation of colour may come without teaching, is also evidenced by a young Caffre, who "named the colours shown him with great rapidity and precision, although his English vocabulary was necessarily limited; and matched Berlin wools and tinted papers with a readiness and unhesitatingness, such as even practised professional colourists might envy." The sacrifice of the human form divine to a love for personal chromatic decoration, as exhibited by some of Dr. Latham's ugly friends at the Crystal Palace, and confirmed by all admirers of savagery, would seem to associate a taste for colour, or at all events the surrender of form for the sake of colour, with an absence of civilization.

As to the influence of sex, Dr. Wilson's researches, both the earlier series included in the body of the work, and latter contained in the appendix, show that colour-blindness is much rarer among females than males. He does not, however, give numbers, but proposes the subject as one open to inquiry by teachers of schools, medical inspectors of factories, &c.

The thirty-four pages devoted by the author to the discussion of the theories of this defect are inconclusive, and, from the very fact of their inconclusiveness, will not bear condensation. All that he deems himself entitled to affirm is, that in the colour-blind "the cerebro-retinal apparatus of vision is unendowed with that sensitiveness to colorific impressions which it possesses in those whose vision is normal;" that is to say, that the cause lies between the humours of the eye-ball and the mind; excluding both the suggestion of Dalton, of its being due to the existence of an abnormal colouring in the lens or vitreous humour, and the idea which might not unnaturally occur to a metaphysical theorist, that the defect was not in the body at all, but in the mind. Dr. Wilson has spent several pages in overthrowing the first-mentioned explanations; and it may be observed that his trouble is not without considerable practical importance, for an Italian oculist, Dr. Trinchinetti, has actually proposed the extraction of the lens as a cure. To stop him, if possible, from carrying his design into execution, a case is recorded of a gentleman who had both lenses removed for cataract, and experienced afterwards, on many occasions, temporary attacks of colour-blindness, which before the operation never occurred.

The great majority of cases of permanent colour-blindness are congenital, and their cause is to seek only in the theories above discussed; but instances of it are related by Mackenzie, White Cooper, and others, where it could be traced to "congestion, hepatic derangement, and dyspepsia."* The disorder is usually temporary under such circumstances, and disappears after the general treatment employed for the state of health. A curious example is, however, detailed among Dr. Wilson's cases, where it was caused by a severe accident, attended by concussion of the brain and long-continued cerebral excitement. Here the colour-

* Article, Vision; Cyclopædia of Anatomy and Physiology.

blindness was permanent, and assumed all the intensity and usual symptoms of the congenital origin.

The cure of colour-blindness by any physical agents seems hopeless; and, according to Dr. Wilson's experience, education seems to do but very little towards removing the defect from the body or the mind, whichever it is situated in. No case is on record among the many highly-educated persons who have thus suffered, of anybody inventing a means of improving their power of chromatic diagnosis.

There are, however, some methods of alleviating or correcting the false judgments which the imperfect sense is led into. One is the *comparison of doubtful with known colours*, by carrying about a chromatic scale, accurately tinted and named. This, however, is available only to a limited extent—that is, as far as the colours of the scale itself can be distinguished. Another is the *employment of touch*, to distinguish the alterations in texture made by different dye-stuffs. This may be of some use to clothiers, weavers, &c., who have a limited number of goods to arrange, but would not assist a customer in purchasing unknown articles. Perhaps to painters *the sense of smell*, higher educated than is usual, might be a help, and would not be open to the obvious objection lying against the employment of the taste.

A more important suggestion, which we owe to Dr. Wilson, is the *substitution of artificial light for solar* in the examination of colours. It certainly is a strange thing that candlelight, which to the healthy eye causes a confusion of delicate tints, should render them more easily distinguishable by a morbid sense. Such, however, is the case, as several examples quoted by Dr. Wilson are sufficient to show. The most striking of these is that of a draper, who has long been in the habit of keeping a gaslight burning in a dark room, to enable him by day to distinguish scarlet from green, and crimson from blue. This is quite sufficient to induce every colour-blind person at least to try the effect of artificial light in correcting his erring perceptions.

From the observation, that it is the yellow colour of ordinary artificial illumination which assists the colour-blind—very white light, such as the lime-ball and electric charcoal light, being as useless as the solar rays—Dr. Wilson inferred that the *employment of yellow or orange transparent media* might be of advantage; and such has proved to be the case in several trials. It is, however, as might be expected, an inferior expedient to the direct use of yellow illumination; and the question of the best colouring substance, whether silver, uranium, iron, or organic matter, is still open to experiment in each individual case.

The employments for which colour-blindness most seriously disqualifies, are those of sailor and railway servant, who may daily peril human life on an indication which a coloured flag or lamp is intended to give. For this evil there are two remedies. First, a careful examination of the parties employed, to test their capability of distinguishing rapidly the signals used; and secondly, to make the shape and movement of signals the chief index of their meaning, so as to dispense with colour except as a subordinate aid to this. It is gratifying to learn that the Great Northern Railway Company have, apparently in consequence of Dr. Wilson's observations, directed that in future all their porters shall be tested

as to their freedom from colour-blindness; and it may fairly be expected that other lines will follow the example. We do not know whether it is in consequence of the observation already quoted, of the rarity of colour-blindness among women, or because they require less wages, that they are extensively employed as signalers on several of the French railways (the Havre, and the Paris and Lyons lines, for example). The other and more effectual mode of prevention, the substitution of form for colour in signals, we fear, has not been taken in hand by any of the managing bodies to whom our lives and liberties are so freely entrusted; and we would call serious attention to the special remarks which Dr. Wilson has made to this subject in the Supplement to his interesting little volume.

Review IV.

1. *On the Disorders of Infantile Development, and Rickets; preceded by Observations on the Nature, Peculiar Influence, and Modifying Agencies of Temperaments.* By A. Schoepf-Merei, M.D., Lecturer on the Diseases of Children at the Chatham-street School of Medicine, Manchester, formerly Chief Physician to the Children's Hospital of Pesth, &c.—*London*, 1855. pp. 218.

2. *An Analysis of* 3000 *Cases of various kinds of Deformities admitted at the Royal Orthopædic Hospital, Bloomsbury-square.* By Edward F. Lonsdale, Surgeon to the Hospital. ('Lancet,' Sept. 1855.)

3. *Lettsomian Lectures: Diseases of the Bones.* By John Bishop, F.R.S. ('Lancet,' Oct. 1854.) *On Deformities*, &c. ('Lancet,' April, 1848.)

4. *Ueber Rachitis.* ('Journal für Kinderkrankheiten,' verschiedene Bände, besonders Bände xvi., xvii., von Trousseau; xvii., von Hauner; xx., von Vogel; xxii., von Stiebel.

On Rickets. ('Journal for Diseases of Children,' vols. xvi., xvii., by Trousseau; xvii. by Hauner; xx., by Vogel; xxii., by Stiebel.

5. *Zur Kenntniss der Periostitis Infantum* (*Rhachitis*). Mittheilung von Prof. Hermann Meyer, in Zürich. ('Zeitschrift für Rationelle Medicin,' von Dr. Henle & Pfeufer, Bänden iii., vi., neue Folge.)

Contributions to the Knowledge of the Periostitis of Infants (*Rhachitis*). By Professor Hermann Meyer. ('Henle & Pfeufer's Journal,' vols. iii., vi., new series.)

6. *Das Normale Knochenwachsthum und die Rhachitische Störung desselben.* Von R. Virchow. ('Archiv für Pathol. Anatomie,' Band v. Heft 4, § 409. Analysed and condensed in 'Monthly Journal of Medical Science' for 1854.)

The Normal Growth of Bone and its Rachitic Derangements. By R. Virchow.

7. *Beiträge zur Lehre von den Rückgratsverkrümmungen.* Von Dr. Eulenburg, praktischem Arzte und Operateur, Direktor des Institutes für Schwedische Heilgymnastik und Orthopädie zu Berlin. ('Journal für Kinderkrankheiten,' Band xxvi. § 47.)—*Erlangen*, 1856.

Contributions to the Doctrine of Spinal Curvatures. By Dr. Eulenburg, Director of the Institution for Iatrogymnastics and Orthopædics at Berlin.

HAD we sufficient space at our command, it would be no uninteresting nor uninstructive matter to trace the general history of "Rickets" from the times of Whistler and Glisson (*circa* 1645–50) to the present day; endeavouring to discover, too, on the one hand, why this affection has been regarded as peculiarly the "English disease;" and, on the other hand, assisted by Dr. Merei, to point out the very probable fact, "that 'rickets' is less frequent in English towns than in many of those of the Continent, and altogether of rare occurrence in Scotland and Ireland." (p. 185.)

But, *tempus brevis, opus multum, et paterfamilias urget*, and all we can do relative to the latter question is to refer the reader to Dr. Merei, thanking him ourselves for enabling us to reject the "soft impeachment" of continental writers. By the "medicine man" of "the brown forests of the Mississippi" alone we may perhaps be thought to have no cause for rejoicing, seeing that among some of the native tribes of North America a bowed form of the legs is esteemed handsome, the utmost care being taken in early infancy to mould the limbs by continued pressure into the admired shape.* A ring through the ear, or a bone through the nasal septum, a bowed leg, or a constricted thorax, *may* be, after all, but matters of taste; nevertheless, each race looks with favour alone on that particular deformity which it is the custom of its people to produce.

> "'Tis with our judgments as our watches, none
> Go just alike, yet each believes his own."†

The subject of *rachitism* has received its fair share of investigation at the hands of modern pathology, and of the *chief* results of which we shall now endeavour to give a general view.

Rachitis is an essentially developmental disorder, having its chief lesional manifestations in the bony tissues or system of support, and intimately connected with the process of the first dentition. It is allied to a general dyscrasic condition of the whole body, clearly evincing itself by a state of chloro-anæmia, and by a degenerate condition of the muscles, ligaments, and skin. In few words, however, the disease may be expressed as a softer state than normal of the bones in infancy and early childhood, which bones yield to pressure, or show divarications from their normal directions and positions in the frame. The child in whom rachitism in its full and perfect form is going to be manifested, becomes, within the first year of life, pale, has its flesh flabby, its digestive functions disturbed, and evinces more or less evidence of general *malaise*. The hair continues thin upon the scalp, the fontanelle wide open, and sweat breaks out upon the head. The little patient at length gets restless at night, bores its head into or rubs the back of it upon the pillow, and cries when the occiput is pressed, or the head lifted up and washed. If the head be now examined, the back of it may be found almost denuded perhaps of hair, the whole skull feels thinner than usual, and as if distended like a bladder, or it is flattened behind at the vertex, and protuberant in front. On careful manipulation with the fingers, the cranial bones may be found very soft in certain points, yielding elastically like cardboard beneath pressure, and

* Cummin, in Cyclopædia of Practical Medicine, vol. iii. p. 616.
† Pope's Essay on Criticism.

giving the feeling as if the skull might be bent inwards by the finger-points upon the brain. We have cut the calvarium (of a child six months old) with a small pair of scissors as easily as cardboard, and folded the skull bones on themselves without breaking.* The disease progressing, the bones of the thorax begin to evince the rachitic state: the child cries when raised up under the arms, by the chest, or when its dress is tightly pulled around it, and the breathing on any excitement becomes embarrassed. Perhaps sickness and emaciation increase, and the child appears to suffer pain or inconvenience except when lying on its back quite still. The sternal extremities of the ribs are now found somewhat swollen or rounded out in a club-shaped manner, and the softened sternum projects more than it ought. In a short time the sternal ends of the *costæ* appear to fall in for some distance straight towards the spine, then to bend outwards towards the sides of the thorax, in a curve whose convexity is towards the vertebral column, so that beneath the axilla there is a large hollow or concavity instead of the normal and rounded form. Hence, what is termed *hühnerbrust*, or "pigeon breast," or "narrow chest," is formed, to which is sometimes added antero-posterior curvature of the spine. With still further pallor of the skin and flabbiness of the muscles, the carpal epiphyses of the radius and ulna become enlarged, which, with a greater relative increase of the metacarpal epiphyses than of the wrist-bones, gives the "knotted" or "double-jointed" appearance so frequently seen. Gradually, like conditions take place in the other joints, along with shortening of the long axes of the cylindrical bones, which appears to be the greater the smaller the transverse diameter of the shaft. The next step of the progress of the rachitic disorder is the curving and bending of the different bones from external pressure, the weight of the body, and the action of the muscles; and, lastly, the occurrence of "infractions," as some call them, or "bent-cracks" of the shafts. According to Vogel—

"Of the humerus such 'infractions' are rare, but are more common of the femur, when a somewhat angular curvature, directed forwards and inwards, is to be observed. If such an 'infraction' be sawn through, on the convex surface, 'compact substance' alone, and on the concave face a thick layer of 'spongy substance,' are found; the medullary cells at the place of fracture are seen elevated by thick ossific growth, which, though afterwards becoming thinner, it is true, yet never permits of recommunication with the medullary canal."†

While the alterations relative to osteogenesis, which we have pointed out, are progressing, and giving rise to more or less change in direction, and to other deformities of the parts of the system of support, the muscles become flabbier and flabbier, the skin more pallid, often cold to the touch, or drier, and throwing off dirty furfuraceous scales. Relative to the structure of the muscles, however, if the observations of Vogel are to be relied on, it may be said that it undergoes no special morphological change, as in only one instance out of many examinations, was commencing fatty degeneration of the cardiac muscles seen, a condition which may be also met with in children who are clearly not suffering under rachitism. The bones of the pelvis may begin to yield and bend when the child endeavours (or is

* Lancet, vol. i. p 95. 1854.
† Journal für Kinderkrankheiten, Band xx. p. 184.

absurdly forced) to try and walk, either in consequence of a more or less partial curvature of the spine—the common cause, or from an inequality in length of the lower extremities—a less frequent circumstance. But the influence of rickets on the pelvis is as yet, it must be confessed, very unsatisfactorily made out. That the pelvis is often enough distorted in the female, is unfortunately too true; but that such distortion is of a rachitic nature is another matter. Trousseau remarks:

"Obstetricians of eminence, and in particular, Nägele, have at length asked themselves the question, Whether the pelvis of a rachitic female always exhibits deformities of such a character as prove with certainty their rachitic nature? and they have been the first (through the facts they have communicated) to oppose the rule that they had themselves established."*

"Rachitis can go through all its phases without prominently involving the bones of the pelvis, and generally has long ceased to rule where, in consequence of a prospective confinement, the pelvis of a woman perhaps undergoes examination; the rachitic child is not subjected to any of the influences which only mechanically operate on the pelvis of the parturient female; all the relations and dimensions are so little different, that a difficulty is rather found in pointing out the analogy than perchance any existing difference."†

Eulenberg‡ asserts, that next to deranged antagonism of the muscles, rachitism is the most frequent cause of *skoliosis*, 4·66 per cent. being due to it. The form of the curve is also said to be different in rachitic skoliosis, to what it is in the other variety, the *convexity* in the former being directed towards the left.

As may be readily imagined, various important functional and organic disturbances of the viscera encased by the rachitic bones, ensue; we can but barely allude to them. From the soft and yielding state of the skull, the brain is liable to pressure, and other irritation; and hence may supervene eklampsia, as we ourselves have witnessed. Elsässer and Lederer have described a *Tetanus apnoicus;* the latter also asserting, that out of 96 cases of "spasmus glottidis," 92 were associated with craniotabes; while Portal and Naumann allude to a form of *hydrocephalus acutus*, as arising from rickets. Of the *essential* connexion of these latter, much doubt may be had; but an overgrowth or hypertrophied state of the brain, with its results, are not uncommon. Dr. Merei states the enlarged brain is often strikingly soft and anæmic; but that whether so or not, the spinal cord appears to be abnormally "thin," particularly in its lower portion, and most commonly softer than in the normal state—not seldom approaching a pultaceous consistence. In the form of the skull, an impress is bestowed, often for the whole of life, upon the countenance; the frontal protuberances and the parietal bones project as it were angularly out, and, as proved also by the measurements undertaken by Mr. Shaw, the bones of the face remain in backward development during youth, so that the adult has often a head of the form of a child. As might be expected, the interference to the functions and growth of the heart and lungs is often great in the extreme, the bones so compressing and displacing these organs, as to give rise to (in our opinion) the more dangerous complications which secondarily affect the rachitic child.§ Dr. Merei well observes:

* Journal für Kinderkrank., Band xvi. p. 256. † Op. cit., p. 267. ‡ Op cit., p. 88.
§ We may refer to a paper by Dupuytren, On Lateral Depression of the Walls of the Chest, as worthy of perusal—Sydenham Society's volume.

"When, in addition to the disorder in the joints and legs, the chest has assumed a notable degree of rachitic deformity—the well-known pigeon-breast, with lateral compression, even though without curvature of the spine,—then any slight bronchial affection is sufficient to form a serious complaint, on account of its being attended, more than in other subjects, with laborious respiration and impeded circulation, with a tendency to passive congestion, and carnification in the pulmonic parenchyma, venous hyperæmia of the brain and spinal cord, diminished innervation to the heart, cardio-spasmus—sudden death." (p. 146.)

According to some observers, the thymus gland is occasionally much increased in size; and Lagrange, Robert, and others, have specially dwelt upon the hypertrophied condition of the tonsils which is frequently present. The liver, as Glisson* pointed out, is very often enlarged in rachitis; but there is no doubt it often appears to be larger than it is, in consequence of the diaphragmatic stricture above it. This circular constriction, with the laterally appressed thoracic walls, and the often tympanitic intestines, may lead to an idea of such abdominal visceral enlargement, as by no means exists. Nevertheless, hypertrophy of the hepatic organ is not an uncommon accompaniment of rachitism; and although Glisson says: "Non male coloratum neque valde induratum aliove aliquo vitio notabili contaminatum, excipienda hic volumus," &c.,† modern pathologists have shown that "fatty induration" and "bacony liver" are occasionally to be met with. Hypertrophy of the spleen and "exsudation process" of the intestinal mucous membrane are also now and then observed.‡ Glisson, observes Dr. Merei,

"Mentions an anatomical alteration in some of the bloodvessels (as having been observed by one of his contemporaries, and which observation he believes to be correct)—namely, that the veins and arteries which run to the 'first affected' parts are uncommonly small, whilst the carotids and jugular veins are disproportionately enlarged. . . . . . So far as I know, this anatomical condition of the bloodvessels has not been noticed in our day; it has escaped at least, I must confess, entirely my attention in the dissecting-room." (p. 152.)

"The anatomical investigation of the dead bodies of rachitic children has not led as yet to the discovery of any such morbid alteration as might tend to a better understanding of the nature of this disease. . . . . The pathological alterations which are observed in those who die at an advanced stage of rachitism—namely, the anæmic aspect of the muscles and internal organs, a dark, over-carbonized appearance of the blood, and enlargement of the liver,—these alterations appear but as the immediate and necessary effects of the compressed state of the chest, and the impeded respiration and pulmonary circulation thereupon consequent,—these are the mere effects of mechanical compression, and devoid of other signification." (p. 156.)

"The learned professor of Cambridge [Glisson] must have seen a great number of very severe cases, and dissected many bodies of children dead of that disease; whilst at present, in spite of the infinitely more extensive medical observation, high stages of the disease and fatal cases directly from this cause seem to be a comparatively rare occurrence." (p. 153.)

Of the deposits of phosphate of lime occurring in rickets we shall speak further on.

In the preceding observations we have had in mind the manifestations of extreme general rachitism. Far less intense forms of the affection are

* De Rachitide, p. 49. † Op. cit. p. 12.

‡ Relative to these abdominal complications, see Bednar, Die Krankheiten der Neugebornen und Säuglinge, &c., Vierter Theil, § 46.

frequently seen, and occasionally the enlargement of the carpo-radial epiphyses and the curvature of the tibia are accompanied with so plump and fresh a condition of the child, that it is with great difficulty the parents can be induced to think there is anything amiss with it. Indeed, some authors have gone to the extent of dividing the disease into two distinct varieties—viz., one marked by a soft condition and curvature of the bones, and an attendant febrile dyscrasia; another, without such soft state, curvature and dyscrasia, and characterized by enlargement of the epiphyses, with a "fat and well nourished" condition of the child. The association of the different deformities with each other is also very variable, likewise the prominence of a particular deformity in different cases, as well as the amount of febrile disturbance and systemic depression. Some late observations made upon a highly acute form of rickets, and those of Elsässer* and Vogel† on *cranio-tabes*, "soft occiput," or rickets of the cranium, comprise some highly interesting points more particularly investigated by recent pathologists. To the former we shall make some reference afterwards; and in regard to the latter we would observe, that whilst rachitic *softening* of the cranium had almost entirely escaped notice until the observations of Elsässer and Widtman appeared, nay, was by some denied to occur, it is now shown to be very common, and to be, in general and extreme rickets, the first manifestation, so far as the bones themselves are concerned. We have seen some very severe cases, and recovery ensue; and never neglect to examine carefully and delicately the bones of the skull when an infant of from six months to a year old comes before us, and of whom suspicion of rachitism exists. This affection of the cranial bones must not be confounded with another peculiar state of the skull of young infants, first described, we believe, by Hauner, and denominated "undersliding occiput" (*untergeschobenes Hinterhaupt*). We were ourselves at first in danger of some confusion in the matter, but can now fully bear out the truth of the double series of observations. We may refer with satisfaction to the paper of Vogel in the twentieth volume of the 'Journal of Diseases of Children,' for much information relative to the two; and in the first volume of the 'Lancet' for 1854, some remarks pertinent to the question before us will be found at page 94.

We will now pass to the histological changes the bones themselves undergo. They may be divided into two great divisions—one relative to the stages of softening, or of *malacia;* the other to those of hardening, or of *eburnation*. The propriety of the term *softening*, as applied to the bones in rickets, is doubted by some; by Virchow, for example, as we shall afterwards see, and by M. le Professeur Gerdy. But whether the bone ever undergoes a process of actual softening after it has been hard, or whether always it has never hardened as it should do, but remains soft, may perhaps admit of discussion. The newly-developed bone-tissue illustrates the latter, that formed before rachitis commenced, the former process. M. Gerdy maintains that when true softening occurs, a *complication* is added to the rickets. Under any circumstances, the bones may be both conveniently and truly said to be in a *soft condition*, though Virchow's views we hold to be correct so far as the explanation of how such

* Der Weiche Hinterkopf, &c. Stuttgart und Tübingen, 1843.
† Journal für Kinderkrankheiten, Band xx. 1853.

a condition *mainly* arises is concerned. If a cylindrical bone of a child who has died with *cranio-tabes*, or well-marked rickets, be examined, the periosteum will be found twice or thrice as thick as normal; in some places highly injected, in others discoloured or marked with milky spots. On its removal the bone looks red, or may even be of a violet colour, is unequally rough, and the *foramina*, as well as the vessels entering them, appear somewhat enlarged in dimension. On section of the bone a quantity of rather tenacious sanguinolent serum is seen to be poured out between the bone and periosteum (where the latter is present), between the bone and cartilage, and even into the osseous tissue itself. If a bone of a still more advanced stage of rachitism be examined on its transverse section, the "compact substance" is found lying towards the interior, whilst in the normal condition it is situated externally. According to Guerin, the sero-sanguinolent liquid before alluded to is at first as thin as water, and easily to be washed away from the denuded periosteal surface of the shaft; but Vogel doubts this, having always found this fluid to be—

"A tenacious red mass, deposited in greatest quantity at the periphery of the bone where it is bounded by cartilage and periosteum. At a later period of the first stage [of softening] this mass coagulates into a firm jelly, in which a new vascular *rete* is organized; so that at length the entire superficies of the bone appears covered with a jelly, containing a thick vascular network."*

At a still later period the epiphyses are found swollen, and the line of demarcation between bone and cartilage is jagged, or irregularly waved, instead of being straight and regular. The minuter changes which occur in the tissues of the system of support have been very closely inquired into of late years by Virchow, Kölliker, Meyer, Vogel, and Stiebel. Virchow's views are very generally accepted, and these, as still further illustrated by Stiebel, will guide us mainly here in our further remarks. To fully appreciate the details of the histology of rachitic ossific tissue, it is very essential the normal structure and growth of bone be kept in view, and the process of osteogenesis, as developed by Sharpey and Virchow in particular, should be carefully borne in mind. It must be remembered that a bone grows both in length and in circumference; in circumference by virtue of a sub-periosteal deposit of what Virchow has called "pumice-stone-like osseous matter," and in length by means of renewed strata of cartilage-cells deposited between the epiphyses and cartilage, and in which cells osteogenesis commences nearest the bone. The "pumice-stone matter" is neither compact nor spongy substance, but something between them, containing large spaces becoming filled with concentric layers (when at an after period compact matter, as it is termed, is formed), and finally going to form the small Haversian canals. During the growth of a bone the central medullary canal becomes increased in size, so that the shaft of a bone in the child might be lodged in the medullary cavity of a like bone of the adult. Now as this increment can only ensue through absorption of the innermost layers, the earlier formed compact substance must gradually approach the central cavity, and become absorbed. We have therefore osteo-formation peripherally, and osteo-absorption centrically, the entire process of osteogenesis and growth being resolved into—

* Journal für Kinderkrankheiten, Band xx. p. 166.

1. Deposit of sub-periosteal, or soft "pumice-stone matter."

2. Transformation of this matter into "compact substance," by the formation of lamellæ in the larger spaces.

3. Resorption of this compact substance, and its metamorphosis into spongy matter in connexion with the increasing diameter of the central medullary canal.

Vogel remarks,

"According to Virchow's doctrines, the whole pathologico-anatomic changes met with in rachitic bones rests on the defective transformation of the 'pumice-stone' mass into compact substance; and this view is so simple, and apparently so true, that we must adopt it unlimitedly, preferring it to all other theories."*

So far as the development of a bone actually proceeds, it ensues in the same manner in rachitism as in health, according to Virchow and Stiebel. There are the same changes, the increase of the cartilage by endogenous cell-formation, the appearance of medullary cavities, the formation of osseous corpuscles, but the ossific process is arrested, for true ossification does not ensue. Relative to the longitudinal growth (or that by epiphysis) of a bone, the histologic differences observed as dependent on this arrest of ossification, are,

"1. Arrestation of the line of ossification, with a relative increase of the preparatory growth-line of the cartilage. In the hyaline layer of the large-celled cartilaginous matter a delicate granular-striped network of intercellular substance is formed, in which lie thick-walled cartilage fossæ, containing the pale cartilage-cells provided with nuclei and nucleoli. The great breadth of the preparatory cartilage-stratum is explained by the dilatoriness of the process of ossification.

"2. Intrusion of the medullary spaces within the line of ossification, or even beyond it, with persistency of the growth of the cartilage.

"3. The formation of fibrous medullary spaces; and the osteoid transformation of their surrounding and distant parts, without the deposition of any calcareous matter. The walls of the cartilage-cells become thickened, their cavities diminish in size, and the bone-cells, with their canaliculi, are formed, but without the deposition of a salt of lime between them."†

According to the investigations of Meyer, yellowish opaque spots arise in the proliferous matrix of the cartilage, in which fibrous formation and softening proceed until vacuities are formed, only to be distinguished from the normal medullary spaces by not containing any lime in their parietes. This view would somewhat modify the above proposition (No. 2).

Connected with the circumferential growth, the following points are the more essential to be dwelt upon:

"1. Increased growth of the sub-periosteal formation, with continuous metamorphosis of the substance into areolæ and cancelli.

"2. Defective ossification of the cancelli, with persistence of the deeper layer of compact margins. The serratures and arches of the areolar bone-stratum are quite devoid of calcareous deposit, looking like shining courses of thick matrix with jagged osteal cells; the deposit of lime is first observed in the deeper layers, and proceeding from within outwards. The rachitic bone has, like the normal one, its thickest layer beneath the surface, but in the latter the layer of 'deposit' is thicker.

"3. Partial cartilage-formation within the areolæ. In the layer of deposit a

* Journal für Kinderkrankheiten, Band xx. p. 164. † Stiebel, ibid., b. xxii. p. 363.

cartilaginous mass sometimes appears, identical in structure with the growing cartilaginous substance of the epiphyses."*

According to Trousseau, if an unbroken rachitic bone is compared with the "healing process" of the fracture of a sound one, a striking resemblance is observed between the first stage of such process and the rachitic condition. The process of ossification is almost identical in both, with the exception that in the latter it proceeds more slowly than in *callus*.

The process of ossification being, however, established and progressing, the bones at length become hard, the stage of *sclerosis* is attained, and in many cases that of *eburnation*, or the assumption by the bone of a very hard and white ivory-like appearance. Microscopically examined, the tissue is found possessing all the characters of "compact substance," but containing comparatively few medullary canaliculi. Whilst before an arrestation of ossific action was seen, now perhaps excessive, osteogenesis occurs. Not only are the sound bones more dense, but sometimes hyperostosis near the epiphyses and sutures is observed. Copland has seen this occur at the terminations of the ribs and commencement of the cartilages, so that the whole was more or less soldered together with the sternum;† and Stanley has observed the bone at the point of curvature converted into a solid substance, the medullary cavity being wholly obliterated.‡ The cause of the latter occurrence has been stated§ to be the bending of the bone compressing into the interior of the curve a larger quantity of cartilaginous substance, which is afterwards filled with earthy deposit, while it stretches and attenuates that on the exterior of the arch, and so diminishes there the matrix for the reception of the earthy materials. In reference to this part of our subject, Mr. Bishop remarks:

"Wilson observed that when the bones begin to recover from the disease, the deposition of osseous matter is most actively carried on in those portions of bones where it is most wanted—that is, 'on the inner or concave surface of the curve;' and Mr. Stanley not only confirms Wilson's remarks, but further states that the thickness of the bone at the part most curved bears an exact ratio to the degree of curvature that the bone has undergone. The deposition of ossific matter is not always, however, adjusted to the mechanical conditions of the parts of the body as means to ends; on the contrary, the bones of the skull are often thickened far beyond their normal state and what is necessary for the protection and support of the brain under ordinary circumstances. It is stated that Sir Charles Bell had in his museum rickety subjects, in which the parietal bones were seven-eighths of an inch thick at their central parts."‖

As regards the history of the lesional changes in the bones in rickets—i.e., the deficient transformation of tissue into "compact substance," and the insufficient deposit of the phosphate of lime in its walls—the chemical inquiries of Fourcroy, Davy, and Bostock may be said to have first illustrated the latter point. Dr. John Davy found 100 parts of the dry tibia of a healthy subject of fifteen years to yield 46·4 of animal matter, and 53·6 of earthy; while the same quantity of the dry tibia of a rickety child contained 74 parts of animal, and 26 of earthy substance. In later years, Marchand, Lehmann, Schmidt, J. Müller, Ephraim, Von Bibra,

* Op. cit., p. 364. † Dictionary of Practical Medicine: art., Rickets.
‡ Medico-Chirurgical Transactions, vol. vii. § Cummin, op. cit.
‖ Lancet, April, 1848.

Stanski, and Dreux, have subjected rachitic bones to analysis, and found the bone-earth often but one-fifth instead of two-thirds of the dry bone. The last analysis we are aware of is one by Professor Böttcher, of the bones of a patient who died of "acute rickets," in the stage of softening, in the Frankfort Hospital, under the care of Dr. Stiebel. The bones were those of a male child, thirteen months old. The femur yielded 20·89, the tibia 24·70, the radius 20·00, the rib 26·65 per cent. of inorganic matter, instead of 60 or even more. Schlossberger, who has lately closely investigated the changes going on in the bones in "craniotabes," states that, as a general result of his whole examinations, the inorganic material was found in the thinned parts of the bone to have sunk to 53 or 51 per cent., and in the thickened spongy parts to 28 per cent., or in slight cases to 40 or 43 per cent. In bones of a normal state, completely dried at a temperature of 120°, the proportion was found to range from 61 to 68 per cent. As the affection becomes cured, the proportion augments, but never to the extent, says Schlossberger, of exceeding the normal one. The proportion of carbonate of lime to that of earthy phosphates is very fluctuating, and that of the fatty matters is not materially changed.

As to what alteration ensues in the cartilaginous basis of the bone in rickets, some difference of opinion has existed. Some deny any change as essential to the disease, or even as commonly present; while at a late period, as affirmed by Simon, "the animal matter is so changed that its extract yields on boiling neither chondrin nor gelatine" [glutin]. It has been maintained by many that the non-existence of any change in the cartilaginous basis is the great mark of distinction between rachitis and osteomalacia, and of which latter such a change is so essential a characteristic.

We may now ask the question as to what is the cause of this deficient solidification of the bones, or the want of the phosphate of lime in them, in rachitism?

"M. de Fouchy by chance saw an ivory spoon which had been left, forgotten for a long time, in some milk, which, when found, had become as supple as leather. This suggested to Hanauld that it was the acidity of the milk which had effected its mollification; and to ascertain the truth of his surmise he immersed some bones in vinegar, which softened them; he then counteracted his experiment by soaking the same bones in water for some time, and drying them, when they resumed their natural induration; and to conclude, he again rendered them soft by a second immersion in vinegar. These facts were deemed sufficient to establish the presence of an acid in the blood of rickety patients, which, according to these theorists, dissolved the medium which held the earthy component of the bones adherent. We then find this opinion adopted by Gagliardi, Herissant, Ruysch, Morand, and many others; but on the strength of an analogical inference, not logical proof."*

Fourcroy long ago asserted that the phosphate of lime taken in with the nutriment was dissolved by a free acid already existing in the system, and carried off by the urine; so that it could never get into the blood as such, and therefore, as such, could not be deposited in the bones. The fact of the urine often containing and depositing phosphate of lime in

* A Treatise on Rickets, with a new Theory of Ossification, by G. Hume Weatherhead, M.D., &c. Second edition, p. 87. London, 1835.

considerable quantity, and the assertions of some pathologists that it was also stored up in the mesenteric glands (a statement now generally regarded as far from the truth), helped to support this theory of an acid dissolving (by excess of itself), either in the stomach or the blood, the salt of lime, and causing it to be deposited elsewhere; whilst others even supposed the excess of acid caused the removal of it from the bones after it had been deposited.

Some modification of this doctrine has been the one generally received, and Dr. Stiebel himself supports it. If the question be asked, What free acid is this that does so much mischief? Marchand and many followers answer, It is *lactic*, formed by the powers of the gastric mucous membrane from the milk sugar, grape sugar, gum, and starch, taken in. Beneke, Ure, and Schmidt, on the other hand, regard *oxalic acid* as the one; Weatherhead thinks it is *phosphoric;* and some have thought that after all it is *hydrochloric.* In all these acids, it is true, the phosphate of lime is easily soluble. When the cause of the *persistent* generation of this free acid has been asked for, the answer has been, "an abnormal change of the portion of the nervous system regulating the functions of the stomach;" or "an increased excitability of the gastric mucous membrane;" or "improper nourishment, or over-feeding with farinaceous or starchy food;" or "too animal diet," &c. According to Dr. Merei:

> "In the action of the saliva itself, which is so copiously secreted in children, we find an explanation of this fact, insomuch as saliva, in its normal condition (by the action of its principle—*ptyaline*), converts starch into sugar and sugar into lactic acid; to the superabundance of which Marchand ascribes the prevented solidification of the phosphate of lime into bony structure." (p. 204.)

That phosphate of lime is not deposited in sufficient quantity in the bones—that an excess of earthy phosphates often exists in the urine—and that a superabundance of free acid is constantly generated in the *primæ viæ*—we hold to be facts fully proved as existing in rachitic children. But that there is more in the matter than the mere excessive generation of acid in the stomach, or its presence in the blood, is clear to our mind. How is it, as Behrend asks, that all children who suffer from acidity—and their name is legion—do not present some signs of rachitism? Admitting, too, that the phosphate of lime intended for the bones passes off by the kidneys, yet the latter must obtain it from the blood; why, then, does the blood carry it on, and not deposit it in the ossific tissues? The only attempt to answer the latter is made in Weatherhead's exposition,* which assumes the free acid to be phosphoric. Further, how are we to account for the wandering pains in the bones in the first and second stages of the acuter varieties; the peculiar chloro-anæmic appearance and cachectic condition of the child; the occasional early outbreak of the malady under no impropriety of diet; and finally, the sometimes almost sudden arrestation of the disorder, when it has run through certain determined stages, without any therapeutic or even hygienic interference of importance?†

The unsatisfactory chemical evidence of the existence of lactic and

* Op. cit., p. 86.

† Behrend, in Journal für Kinderkrankheiten, Band xxii. § 366; Copland's Dictionary of Practical Medicine: art., Rickets, 30, C.

other free acids in the blood, and some late minute anatomical investigations of rachitic bone, have been the means of inducing one or two pathologists to advance a very different ætiology to that generally received. Böcker finds the cause of rickets to be mainly in a too rapid action of the "blood-moulting" process. According to Trousseau and Lasegue the primary fault has its origin in the bones themselves. There is set up in them a special form of inflammatory action, having as its result an absorption of the already deposited salt of lime on the one hand, and serving as an hindrance, on the other, to its further deposition. The fundamental lesion, or primitive morbid action, at the base of rickets, is affirmed by Meyer to be a "hyperæmia of the periosteum or periostitis;" and in proof of which he states he has been able to demonstrate, not only different exudation products, but, in two cases lately observed, even purulent deposits beneath the periosteum. One of the latter cases

"Had a collateral interest, inasmuch as it was one of a new-born child from whom the remains of the umbilical cord were not yet detached; it was, therefore, an undoubted case of *intra-uterine* rachitis. The larger bones of the extremities (and even most of the other bones, especially the scapulæ and iliac bones) exhibited in this child the following remarkable conditions:—Upon a firm and compact *substantia dura* was superimposed a very firm and thick *osteophyt*, cellular, and rough externally. The macerated shafts represented in miniature the thickness, weight, and unevenness, or almost the appearance, which one is accustomed to see in bones with *complete necrosis*, only, of course, neither *cloacæ* nor *sequestrum* being present. Where the diaphysis and epiphyses met, the periosteum was distended, the epiphyses exhibiting quite an unusual mobility. On opening the periosteum at this point, a cavity was seen filled with a thick, fluid, chocolate-like matter. The microscope demonstrated the existence of crumbled detritus, blood, and pus-cells. In this cavity lay the end of the shaft, completely disengaged, and surrounded by the fluid; and it was thus clear that a purulent deposit from periostitis had loosened the entire cartilage, which, at the margin of ossification, had undergone growth by primary cells, but in consequence of the disease had remained *unossified*. At the end of the shafts, bathed in the pus, 'corrosion' was observed in some places; but, in general, the shaft ends were very little affected as yet, and even possessed the thin osseous lamella separating the plane of ossification from the non-ossified parts."*

Whilst we admit the want of *demonstrative* evidence in proof of the existence of lactic acid in the blood, or excess of other acids in the stomach, in rickets, we are far from willing to admit a local inflammatory action of the periosteum or bones in this disorder as its fundamental essence. We acknowledge the force of much of MM. Trousseau and Lasegue's reasoning; nor do we dispute the statement of Meyer, that he has seen exudation products, and even purulent matter, in a bone belonging to a rickety skeleton: but even admitting that *inflammation* did play a part in the train of phenomena, we should require far more stress to be laid upon an earlier *general dyscrasia*, than the pathologists of the school we have alluded to appear to concede. We have general affections of the bones and periosteum in syphilis and arthritis, but then there is something far more fundamental at which we can arrive than a merely local lesion of the tissues of the system of support. So it is in rachitis. We do not deny that some of these pathologists refer to "too-early weaning," "improper nourishment," "damp and ill-ventilated habitations," &c., as liable to predispose

* Henle und Pfeuffer, Band vi. § 151.

to rickets; but we do find more stress placed upon the "local hyperæmia," than on the diathetic causes we shall presently refer to. We are thus brought to inquire into the nature and relations of the constitutional state giving rise to, or accompanied by, rachitism, and into its identity or connexion with one or two important diathetic conditions.

Until about twenty years back, the opinion was very generally adopted, that rickets was *scrofulous disease of the bones.* The fundamental dyscrasia preceding and accompanying lymphadenitis, disease of articular cartilage, certain common affections of the tegumentary surface and its appendages, and all forms of tuberculosis, was regarded as that at the bottom of rickets. Some, however, doubted this; and at length, in 1834, M. Rufz asserted that he had examined the bodies of twenty rachitic children, and had found but six times pulmonary tuberculosis;* and M. Trousseau subsequently added, that not five per cent. of rachitic children were scrofulous. It was afterwards maintained,† that not only is rachitis not scrofula, but that the former appears actually to exclude, in some manner, tuberculosis; and lately, M. Trousseau has taken up the same argument:

"Rachitis and tuberculosis appear to exclude each other; for whilst in children labouring under chronic diseases generally, tuberculosis exists in the majority, scarcely one would be found with pulmonary tubercle in twenty rickety cases. Between scrofulosis and rachitis there exist such striking differences, that while the former is almost always incurable, the latter is pretty generally self-curative, and gives rise to death only by intercurrent affections. Scrofulosis, on the other hand, leads to the deposit of tubercle. Sometimes rachitis is confounded with mesenteric scrofula."‡

Dr. Merei states that in Manchester, where rachitism is frequent, tuberculosis is seldom observed in children; and which fact agrees with his experience at Pesth, where, in upwards of five hundred post-mortem examinations, not one instance occurred of tubercular consumption in a child having a high degree of rachitic spine and chest. The author concludes

"That certain forms and certain degrees of scrofulosis and tuberculosis *may* co-exist with rickets; but that a high degree of rachitic compression of the chest necessarily connected with over-carbonization of the blood, is a condition adverse to the development of pulmonary tubercles. The assertions, therefore, of Rokitansky on this subject seem to be correct. . . . . Finally, my conclusion, based upon careful observation, is, that rachitism is a disease which *may* be associated with, but is distinct in its nature from, scrofulosis and tuberculosis." (p. 197.)

For further information upon this immediate point, the reader is referred to some observations by the writer of this article in the 'Lancet' for August 26th, 1854. We have formed a strong opinion upon the question at issue, obtained after some years' experience amongst the poorer children of a not unimportant part of this metropolis. We would admit, of course, that, so far as the local lesion is concerned, rachitism is not tuberculosis, nor is tuberculosis rachitism; and we would maintain that tuberculosis is not properly a convertible term with scrofula, nor is rachitism. Scrofula is a "general," of which tuberculosis (in the opinion, at least, of very many pathologists) is a "particular,"

* Bibliothèque du Medécin-Praticien, tome vi.: art., Rachitisme

† See Bouchut, Maladies des Nouveau-Nés, p. 804.

‡ Journal für Kinderkrankheiten, Band xvii. p. 246.

and (as we think) so is rachitis. The two forms of disease are local and particular manifestations of a general dyscrasy, which we believe is the same in both—viz., the scrofulous. That tuberculosis is not a *frequent* manifestation, going hand-in-hand with rickets, may be granted, just as well as that marked chronic lymphadenitis, ulceration of articular cartilage, &c., are daily seen to be unaccompanied by tuberculosis; and which fact, we are aware, has, along with other reasons we have not space to touch upon, led some pathologists (like Lebert and Legrand) to maintain that the latter has no essential relations to the scrofulous cachexia.* On the other hand, Laycock, Balman, and Glover have shown† that so close is the connexion between them, that an external manifestation of the scrofulous diathesis is a defence against the other, or acting as a sort of safety-valve to the lungs. That tuberculosis and rachitism do not absolutely exclude each other, is known to us by necroscopic inquiry; indeed, we have met with one of the rarest forms of the deposit—viz., intra-serous grey granulation of the pleura—in a child one year and a half old, with rickets;‡ whilst M. Hervieux having met with the coincidence so often as one in three, is actually led to regard rickets as an important symptom of tuberculosis before three years of age.§ To our minds, that which would reject rachitis as an evidence of the scrofulous cachexia would go far to the rejection of tuberculosis as such, too; and that this has been done by some we have already hinted. The like reasoning also which leads us to acknowledge tuberculosis to be connected with the dyscrasia in question, would incline us to view as such rickets too. Its occasional union with tuberculosis, its not unfrequent complication with the tegumentary, &c., forms of scrofulous inflammation; the occurrence and hereditary transmission of all three manifestations in intense form in the children of the same family, not one of whom escape one or other variety, and one if not both of whose parents are evidently strumous, lead us to this opinion. The manifestations of the scrofulous cachexia, as thus indicated, no doubt appear in themselves to be often of a very opposite character; but let us take a cognate malady—syphilis,—and ask if we were less acquainted than we are with its natural history, of its transit from primary sores to eruptions, to affections of the cutaneous surface and mucous membranes, to glandular enlargements, and through these to wandering nocturnal pains of the bones, to periostitis, to caries, to necrosis, &c., &c.; its transmission to the fœtus in utero, and final culmination in the production of abortion;—should we find it much easier to believe these different manifestations to be truly the offspring of a common origin, than those of the scrofulous cachexia above alluded to? The difficulty here suggested partly existed, indeed, during the sixteenth and seventeenth centuries, as Behrend of Berlin has pointed out.

Boerhave and several writers since his time have actually regarded rickets as belonging to the syphilitic rather than to the scrofulous cachexia; while others have referred the latter itself to syphilis. Vogel says the father of a rickety child has frequently acknowledged to him his syphilitically-tainted system; and thinks the existence of the latter may fre-

* See a review of various writers on the Pathology of Tubercle and Scrofula, in the fourth vol. of the new series of this journal (Oct. 1849); also Rilliet et Barthez, tome iii. p. 314 et seq.
† British and Foreign Medico-Chirurgical Review, vol. x. p. 167.
‡ Lancet, ut antea. § Rilliet et Barthez, tome iii. p. 866.

quently explain the occurrence of rachitism in the children of the better classes. On this point Trousseau observes:

"According to the views promulgated by Ricord upon congenital syphilis, and on the transmission of secondary and tertiary accidents, which may come, after several generations, to represent scrofulosis, rickets may perhaps be descended from the same. In the present day the latter is far more frequently met with among the poor than the rich. With the former, in consequence of neglected treatment, and the operation of external deleterious circumstances, syphilis becomes more obstinate and rooted in its transmission to the following generations; so that rachitis, as Boerhaave, Van Swieten, and Glisson have supposed, may be a degenerate form of syphilis."*

While from our own experience we should be disposed to lay more stress upon the transmissibility of a defective constitution from parent to offspring as the first link in the chain of causation of the general dyscrasia, whatever name it may receive, others, admitting the occasional transmission, lay more stress upon the operation of external causes after birth, regarding the diathetic state rather as *in toto* acquired than as necessarily associated with an hereditary predisposition. We by no means stand alone, however, in this belief of the transmissibility of the dyscrasia of rickets from the parent to offspring. Hennig† affirms true rickets, or osteopsathyrosis, to be in the highest degree hereditary; while, according to Schönlein, too early marriages, and to Küttner, intermarriages, mainly conduce to its transmission.

In reviewing the numerous extrinsic circumstances brought forward as the cause of the cachexia of rickets, it is interesting to observe how they are all those which are more or less intimately associated with the manifestations of the scrofulous dyscrasia—viz., deficient or improper diet, bad air, moisture, cold, want of exercise, and deficient exposure to solar light, &c. Of the relative import of these circumstances in the causation, much difference of opinion exists. The experiments of Guerin on the lower animals go to prove the influence of improper food; while Dr. Cummin observes (op. cit.) that whole broods of young geese and ducks, young pointers and greyhound puppies, and young pigs, have the rickets, or "krinckets," when they have been continually exposed to cold and wet, or have been kept in damp kennels and sties. According to Trousseau, damp and ill-ventilated habitations "give rise to scrofula rather than to rickets;" while in M. Coste's experiments they tended specially to tuberculosis. M. Trousseau states that of a hundred rickety children ninety-eight were either never suckled at all, or were weaned very early; and Mr. Lonsdale is of opinion that rickets—

"Is produced by deficient nourishment during the period of infancy, when the child does not get sufficient from the mother, either in the quality or the quantity of the milk secreted. I have paid particular attention to this point, and have found, I think invariably, that in all rickety children the parents have had little or no milk for their supply, and that they have been obliged to feed them either partially or wholly. The mothers will tell you that the children never grew properly from the first. I do not mean to say that children brought up by hand necessarily become rickety, for a child may thrive well if *properly* fed, and may be strong enough to thrive even if *improperly* fed. This is found by every-day experience. But I believe that a child will not become rickety if the mother be a healthy woman, and has plenty of milk secreted to supply the kind of food nature intended

* Journal für Kinderkrankheiten, Band xvii. p. 247.

† Lehrbuch der Krankheiten der Kinder, § 420.

during the first twelve months of infancy. A healthy child, however, though born of healthy parents, may become rickety if taken from its mother and brought up by hand with *improper* food; while, on the other hand, a child becoming rickety may be restored to health by being put to a strong healthy wet-nurse. I have seen many cases of this kind where, I am sure, this has saved the children from future deformity."*

Böcker, in 1849, published a series of cases, tending to show that it was probably a deficiency of the phosphate of lime in the maternal milk which gave rise to rachitism in cases where the child was suckled. Drs. Vogel and Merei attach much importance to impure air. The former writes:

"Of extrinsic causes, one can alone be affirmed with surety—viz., the want of fresh air, which is universally shown by all observers to be the most frequent in causation of rickets, and evinces its importance by the fact of the infrequency of the disease in southern climates, and during the warmer quarters of our year."†

Dr. Merei believes (p. 186) improper food *alone*, under the influence of a pure atmosphere, to be insufficient to produce rachitism; while the influence of unwholesome air is sufficient to give rise to it, in spite of the most wholesome kind of diet. Hauner regards "great poverty, insufficient clothing, and want of exercise," as more powerful than improper food, though "a highly-carbonized air, as shown by Beddoes and Withering, is equally detrimental."

To our mind, the above are all only so many extrinsic exciting causes (each of considerable, yet, according to circumstances, frequently variable, force) of the scrofulous dyscrasia, which may evince itself by different open manifestations, according to circumstances as yet imperfectly known to us. In one child, chronic abscesses or ulceration, in another, tubercular deposit, and in another, rickets, may make their appearance; the more intense the particular form of outbreak in each case, the less chance perhaps that one class of manifestation shall be complicated with another. In connexion, finally, with this portion of our subject, we have still left for us to inquire into the relationship between rachitis and the *osteomalacia* of adults, a relationship firmly believed in by a party, of which we might take M. Bouchut as the exponent, when he says: "I agree with Boyer and M. Beylard: rachitism and osteomalacia are one and the same disease of the bones, modified by the age of the subjects; for me, rachitism is the osteomalacia of infancy;" and Vogel as that of another, which declares—

"Rachitis and osteomalacia are evidently two different processes—the one quickly progresses in developing bone; the other attacks bones of completed development: the one passes through certain stages to a given point only, and reverts, even if slowly, to a state of health; the other induces various changes, particularly fatty degeneration of the bones (of which nothing is observed in rickets), and progresses irresistibly to its ever-fatal termination."†

We are compelled, however, here to draw our observations to a close, trusting on some future occasion to be able to revert to this particular portion of our subject, as also to that of *acute* rickets. In concluding, we would strongly recommend the perusal of a paper by M. Trousseau, in reference to these matters, and which will be found in the seventeenth volume of the German 'Journal for the Diseases of Children.'

* Lancet, Sept. 1855. † Op. cit. p. 168. ‡ Op. cit. Band xx. p. 164.

W. Hughes Willshire.

Review V.

1. *On the Capacity of the Lungs, and on the Respiratory Functions.* By John Hutchinson, Esq. ('Medico-Chirurgical Transactions,' Vol. xxix. 1846.)
2. *On the Mechanism of Respiration.* By Francis Sibson. ('Philosophical Transactions.' 1846.)
3. *Ueber die Menge der Ausgeathmeten Luft bei verschiedenen Menschen.* Von Gust. Simon.—*Giessen*, 1848.

On the Quantity of Air Expired by different Persons. By Gustavus Simon.

4. *Fabius, de Spirometro ejusque usu dissertatio.—Amstelod.* 1853.

Fabius, Dissertation on the use of the Spirometer.

5. *Krankheiten der Respirations-Organe.* Von Dr. M. A. Wintrich. ('Handbuch der Speciellen Pathologie und Therapie,' Band v. Abthl. 1.)—*Erlangen*, 1854.

Diseases of the Organs of Respiration. By Dr. M. A. Wintrich.

6. *Ueber die Athmungsgrösse des Menschen.* Ein Beitrag zur Physiologie und zur Diagnostik der Krankheiten der Athmungswerkzeuge. Von Dr. Friedrich Arnold.—*Heidelberg*, 1855.

On the Capacity for Respiration in Man. A Contribution to the Physiology and Diagnosis of the Organs of Respiration. By Dr. Frederick Arnold.

That respiration is a function indispensably necessary, not only to the preservation of health, but even to the very continuance of life, is remarkably exemplified in the provision made for it in every living being. Throughout almost the entire animal kingdom we find distinct organs for the aëration of the blood; and these organs cannot be either temporarily or permanently interrupted in the performance of their peculiar office, without producing a corresponding transient or permanent disturbance in the vital functions. Although the structure and situation of the parts ministering to the respiratory process differ widely in the various species of animals, yet the duty devolving upon them all—namely, the absorbing of oxygen and exhaling of carbonic acid—remains invariably the same, whether it be performed by the agency of lungs, as in Mammals; by gills, as in Fishes; by tubes, as in Insects; or by simple sacs, as in some Gasteropods. The lungs, gills, tubes, and sacs form, however, but a small portion of the apparatus necessary for the interchange of the gases in the animal economy; and if we consider respiration in its more extended acceptation, solely as the process by which oxygen is absorbed and carbonic acid exhaled, we are forced to acknowledge that every individual organ and tissue of the body respires; and consequently, that the so-called respiratory organs are far from being the only scene of the breathing process. Few of our readers will, we think, be inclined to doubt that a knowledge of the changes which the air undergoes during respiration is

of the utmost importance to the preservation of health; and many will unite with us in support of the opinion, "that a clear comprehension and investigation of the action of oxygen in the human body, is of no less practical importance than the great and fundamental principle of the circulation of the blood." *

Every new discovery, therefore, which tends to elucidate any of the more obscure points connected with the study of this interesting and difficult subject, will be welcomed alike by the practical and by the scientific members of the profession.

Were we to consider the mechanism of respiration simply as the means by which the gases are carried to, and expelled from, the pulmonary air-vesicles, we might, after the perusal of Dr. Sibson's beautiful and learned researches upon the movements of the chest, be inclined to believe that a perfect knowledge of the respiratory mechanism had been attained. But inasmuch as this function is to be regarded simply as the absorption of oxygen and exhalation of carbonic acid, and as we acknowledge that every organ and tissue respires, we are forced to admit the necessity, not only of a clear appreciation of the method in which the air is conveyed into the vesicles of the lungs, but also of an acquaintance with the laws by which it traverses their walls, enters the general circulation, is transported to the capillaries, and finally received into the tissues, to become incorporated with them in the act of assimilation; all these points must be thoroughly understood before the mechanism of respiration can be definitively discussed.

The valuable researches of Professor Graham on the diffusion, the scarcely less brilliant ones of Henry and Dalton on the laws of the absorption, of gases, and the experiments of Daniel, Draper, and others, on endosmose and exosmose, have gone far towards elucidating the subject under consideration. The question, however, still presents sundry important points on which physiologists differ. One sect, with Valentin for their leader, declares that in respiration the gases traverse the walls of the pulmonary air-vesicles, and enter the blood in obedience to the same law by which they would pass through a dead animal membrane or a porous inorganic substance—viz., that of inverse proportion to the square roots of their densities. But this view, applied to our subject, requires much further consideration; for in respiration the gases are placed in very different relations to each other from those they occupy when both exist in a gaseous state, with only a dead animal membrane or inorganic porous material intervening between them. In the lungs the gases exist, *in a gaseous form*, on one side only of the separating membrane; on the other side they are mixed, and even to a certain extent chemically combined, with a liquid whose presence may entirely nullify the law of diffusion. This is, in reality, proved to be the case. Graham's law of the diffusion of gases in inverse proportion to the square roots of their densities, only holds good providing the intervening membrane be perfectly dry. The smallest degree of moisture immediately annuls the law of diffusion, substituting for it the law of absorption. This we ourselves have had occasion to see beautifully exemplified in an experiment, where

* Dr. Bence Jones's Animal Chemistry, p. 7.

the law of diffusion between carbonic acid gas, on the one side, and atmospheric air on the other, was intended to be demonstrated; but where, in consequence of some moisture accidentally adhering to the membrane separating the two gases, the law of absorption had come into play Instead, therefore, of seeing, as was expected, the air, the lighter of the two gases, going over in greater proportion to the carbonic acid, the heavier gas, a diametrically opposite effect was observed; the carbonic acid went over in greater proportion to the air, the absorbing power of water being stronger for carbonic acid than for atmospheric air.

If, then, a single drop of water is sufficient to render null and void the laws of diffusion, how can we entertain the idea that the blood, whose liquid particles are nothing more or less than water, and that the moisture in the walls of the pulmonary air-vesicles can produce any other effect? Not, certainly, because the membrane in the lungs is living, while that in the above-mentioned experiment was dead. For it has yet to be shown that a living membrane is endowed in this respect with properties not possessed by a dead one, whose tissues are yet unchanged by the process of decay. Science has till now been unable to detect any such difference.

The other sect, at whose head we think Vierordt may be justly placed, bases its theories upon the laws of absorption discovered by Henry and Dalton. Of these two observers, the first has pointed out how the volume of a gas, absorbed by a liquid, depends on, or rather bears a direct proportion to the atmospheric pressure. If, for example, a given quantity of water, under a pressure of one atmosphere, has the power of absorbing 11 volumes of carbonic acid gas, the same quantity of water, under a double amount of pressure—two atmospheres, will absorb a double quantity of carbonic acid gas—22 volumes. Dalton has proved, that where there is a mixture of gases, the pressure of each individual gas alone determines the proportion in which it is absorbed by a liquid, the amount of its absorption being entirely independent of the pressure exerted by the surrounding gases.

Upon these premises, Vierordt and his followers endeavour to explain the mechanism by which the blood absorbs oxygen, and exhales carbonic acid. Let us suppose, for instance, the blood, on reaching the pulmonary capillaries, to contain a greater amount of carbonic acid gas than the carbonic acid present in the air-cells is able by its pressure to maintain in its condensed state in the liquid; a quantity of that gas, sufficient to establish the equilibrium, will at once escape from the blood. In other words, carbonic acid gas will be given off from the blood, until the amount present in that liquid is reduced to the quantity which pure blood is capable of absorbing under a pressure equal to that existing in the pulmonary air-vesicles. As regards the absorption of oxygen, the venous blood, on reaching the capillaries of the lungs, contains a much smaller amount of this gas, than pure blood would be capable of absorbing under a pressure equal to the one there experienced; and the blood will take up a volume of oxygen sufficient to supply the deficit. It is thus seen that the carbonic acid gas and the oxygen contained in the blood, on entering the

small vessels of the lungs, exist under circumstances exactly the reverse of each other; and that in order to restore their respective equilibriums, a directly opposite change supervenes in the case of each. One gives up the plus which it contained; the other receives the minus which it wanted. The volume of each gas is perfectly independent of that of the other, their interchange not being effected (according to a belief which still obtains to a certain extent) by a mutual displacement, but being determined by the respective amount of each gas present in the blood, and the pressure of the corresponding gas in the pulmonary air-vesicles.

We are unable at present to go more fully into the various theories promulgated of late years on this important and interesting subject, but shall devote the space at our command to the consideration of the mechanism of respiration and spirometry. We shall first call our readers' attention to the admirable treatise of Dr. Sibson, a work that bears the stamp of entire originality, while it conveys the assurance that the author is an acute and philosophical observer.

We propose successively to examine some of the more important points of his work. A few years before its publication, Dr. Sibson observed, while examining the chest in persons subject to dyspnœa, that the latissimus dorsi and the serratus magnus muscles acted during forcible expiration; and on further inquiry, he found that neither of them acted during inspiration. He also ascertained that in inspiration the scaleni acted the whole time; that the superior ribs approached each other, while the inferior moved farther apart; and that the *internal* intercostal muscles between the six superior costal cartilages, and the *external* intercostal muscles between the superior ribs, were in action. After having made an extensive series of observations and illustrations of the respiratory apparatus in animals breathing by ribs, he exposed the muscles of respiration in the dog and the ass while alive, and noted which muscles acted on inspiration, and which on expiration. He afterwards made the discovery, that when the lungs of a dead animal were inflated, the inspiratory muscles became shortened when the lungs were distended: an observation of the utmost importance, as it lessened the necessity for the repetition of vivisections.

Numerous observations on all classes of animals breathing by ribs, commencing with the simplest, and gradually ascending to the highest—the human organization—convinced our author that the mechanical portion of the respiratory process was far more complicated than is generally supposed; and that in some important particulars the commonly received opinions on the subject required to be essentially modified.

Dr. Sibson made his first observations upon reptiles of the serpent tribe which possess no sternum, their ribs being connected with the vertebræ only, and having no costal cartilages. In the snake, whose ribs, besides serving the respiratory movements, are also used for the purpose of progression, he found the internal intercostal muscles to be expiratory, and the external ones inspiratory; and by the aid of ingenious diagrams he has succeeded in explaining how, on inspiration, the ribs are raised, move forwards, glide on each other, and increase their intervening spaces. From the snake he passed on to the consideration of the respiratory function in

the chameleon, a cold-blooded animal, of sluggish movements, and therefore breathing slowly and irregularly, yet, nevertheless, requiring at times very deep inspirations, and very sudden and complete expirations. This animal was found by our author to possess a greater amount of rib-movement than almost any other creature; the addition of a complete system of anterior ribs or moveable cartilages affording it twice the power possessed by the snake for the expansion of its lungs. The inspiratory muscles acting on the ribs are the scalenus, the levatores costarum, and the external intercostals: they have the same action and nearly the same anatomical distribution as in the snake. Advancing in the scale of vertebrated animals, Dr. Sibson next transferred his investigations to the feathery tribe:

"The lungs in birds are embedded in the spaces between the ribs; they only have a free surface invested with the pleura anteriorly. The diaphragm in the fowl, forms with this free surface of the lung, an enclosed cavity.

"In mammalia, each lung is enclosed in a distinct pleural sac, the whole lung being free save where the air-tubes and great vessels are attached. This cavity is completely closed above, protected by ribs, muscles, and fasciæ. The first rib is more intimately connected than any other with the sternum; in the majority of animals it either directly articulates with that bone, or is united to it by a short, firm cartilage. It is in the expansion in every direction of the upper part of the chest, and the great range of descent of the whole diaphragm, the progressive lengthening of the ribs, the arched and comparatively mobile spinal column, and the usually small, jointed sternum, that the mechanism of breathing in the mammalia chiefly differs from that in birds; for in birds, the upper part of the chest is not closed in, the diaphragm has but a limited range of motion, the spinal ribs are nearly of a length, the spinal column is stiff, and the sternum is in one large piece." (par. 29.)

In mammals, the costal cartilages are analogous to the sternal ribs of birds. This is particularly observable in the sheep, the cow, and the pig, animals in which the costal cartilages are articulated with the ribs and the sternum by joints, differing from the sternal ribs of birds in the single circumstance of having an osseous in lieu of a cartilaginous structure.

"The relative proportion that the sterno-vertebral ribs, or those of thoracic respiration, bear to those of diaphragmatic respiration, depends on the size and form of the upper lobes of the lungs, compared with the lower.

"In the ass, the upper lobes are small and narrow, the lower are large and full at their posterior part. The lungs are short in front at the sternum, long and broad behind and below." (par. 34.)

Other animals have an exactly opposite conformation of chest. For example, in the dog, the body and the superior lobes of the lungs are large, and they are nearly as long anteriorly as posteriorly.

Dr. Sibson proceeds to discuss at considerable length the question of the costal mechanism; but our limits, and the impossibility of explaining ourselves clearly without the aid of diagrams, compel us to refer those of our readers who feel interested in the subject to the original memoir, and to pass on to review the muscles which act upon the ribs.

While in birds as well as in snakes, all the external intercostal muscles are inspiratory, and all the internal intercostal muscles expiratory; in mammalia, on the contrary, whose superior and inferior ribs have distinctly opposite motions, the superior intercostal muscles are observed to

have an opposite direction to the inferior. The action of the costal muscles being entirely subservient to the motion of the ribs, it is necessary to have a distinct comprehension of the movement of the latter before a just appreciation of the action of the former can be attained. For example: If the external intercostal muscle, situated between two superior ribs that approach each other, be inspiratory, the muscle having the same name and holding the corresponding relative position between two inferior ribs that recede from each other, will, on the contrary, be expiratory. Thus, in drawing a conclusion as to the action of any particular muscle, we must regard, not the system of muscles, but the system of ribs. Sometimes, however, it happens that the different fibres in the same muscle act in various directions. In the ass, our author observed on vivisection, that the superior fibres of the serratus magnus were expiratory, while the central ones supporting the scapula were neutral, and the inferior inspiratory; proving that, in one and the same muscle, the fibres may act in no less than three distinct directions.

The relative proportion of the respiratory apparatus to the size of the body differs exceedingly in different animals, and its magnitude is seen to depend principally on their habits of life. In the seal, the breathing apparatus is very large; the size of the animal's abdomen appearing quite insignificant in comparison with its capacious thorax. In the porpoise, this phenomenon is still more marked; for in addition to the usual thoracic and diaphragmatic space for breathing, a large portion of the lung occupies the neck: the porpoise is thus furnished not only with a thoracic, but also with a cervical respiration. Dr. Sibson attributes to the presence of this extensive breathing apparatus the power possessed by the creature of remaining a very long time under water. He conceives that it has the faculty of renewing at each inspiration nearly the whole volume of air contained in the lungs, the preceding expiration having almost entirely freed them from the adulterated air.

"The animal may descend under water with a far purer and far larger stock of air than land animals can, by any effort, obtain; of course, when the animal dives very deep, the quantity of air in the chest cannot be great." (par. 77.)

In man, the mechanism of respiration partakes, in one or more particulars, of the structure of each of the creatures already mentioned, and is consequently, in a certain degree, illustrated by their structure. The human chest is of ever-varying capacity. Its cavity can be enlarged simultaneously in every direction. The domed roof rises, the floor descends, and the diameter between the walls is increased on all sides. Here, however, varieties in the mechanism of respiration are also found to depend on age and sex. In the female, the development of the abdominal in relation to the thoracic cavity, is considerably greater than in the male; in the child, and still more in the fœtus, this distinction of size is peculiarly marked, from the great extent of the liver compared to that of the lungs. As age increases, the bulk of the pulmonary organs augments, in consequence of the gradual enlargement of the air-vesicles. Dr. Sibson notices that disease has a still greater influence on the respiratory movements; on this point he has truthfully remarked:

"If the larynx be narrowed, so as to permit but little air to enter the lungs, the diaphragm descends so rapidly that the air has not time to fill up those portions of the lungs displaced downwards by the diaphragm; the consequence is, that the pressure of the atmosphere forces backwards and inwards the costal walls on the lungs.

"If the summit of the lung be affected with phthisis, the corresponding portion of the chest is but little dilated, and the ribs are depressed and almost motionless over the diseased portion of the lung; sometimes the rib even falls in at the beginning of respiration.

"In inflammation of the lower lobe of the lung, that lobe is distended by diseased secretions; the chest over it is permanently expanded, and has little or no respiratory movement; at the same time, the upper portion of the same side of the chest is less actively inspiratory. If the diaphragm be inflamed on one side, that side does not act; the diaphragmatic ribs and the hypogastric region do not move forwards on the affected, though they do on the healthy, side.

"If any part contiguous to the ribs, on one side, should be injured by the respiratory motion of those ribs, then that side of the chest is often motionless, though the lung be sound.

"If the air-cells be dilated, the whole chest takes on permanently the form that it has on a deep inspiration.

"These instances show the practical value of a thorough knowledge of the healthy respiratory movements of each portion of the chest; attention is at once called to any point that, owing to disease, has not its due motion." (par. 101.)

This valuable paragraph introduces us gradually to the study of the vital capacity in health and disease, a study which has proved as beneficial as that of the mechanism of respiration to the science and progress of medicine.

In bringing our remarks on Dr. Sibson's paper to a conclusion, we cannot refrain from complimenting its author upon the able manner in which he has prosecuted his researches; to the physiologist they offer valuable suggestions as to the function of respiration, while they materially aid the practitioner in the diagnosis of disease.

The next point to which we shall direct our attention is one in which the scientific and practical are so intimately blended, that even the most empirical members of the profession can scarcely regard it without interest. The immediate result of Mr. (now Dr.) Hutchinson's labours to which we have to advert, was the discovery of a new physical means of diagnosis in pulmonary complaints, and one, moreover, of the utmost significance. The greatest credit is due to this diligent inquirer for having been the first to invent and to employ a philosophical instrument by which the quantity of air taken into the lungs at each inspiration can be accurately measured; and it speaks well for his practical acumen that he so nicely appreciated the true practical value of the researches he instituted, and was able, in the face of preconceived ideas, and numerous circumstances calculated to embarrass inquiry, to arrive at such just and important conclusions. Although some of his theories do not rest on so sure a basis as might have been desired, they were yet arranged and presented in a manner which could not fail to arrest the attention of the physiologist, and to furnish an impulse to further inquiry on a subject offering so magnificent a reward. This effect has in reality followed upon the publication of Dr. Hutchinson's work; since the time of its appearance many labourers in the field of scientific medicine have been induced to inquire

into the vital capacity of the chest; and from a repetition of our author's experiments, aided by others of their own invention, have been forced to unite their testimony with his in pointing out how a knowledge of the vital capacity of the lungs lends invaluable aid in the diagnosis of a disease most subtle at its commencement, and most fatal in its termination —pulmonary phthisis.

As early as 1679, the physiologist Borelli instituted an experimental inquiry into the quantity of air received into the lungs at each inspiration; and from his time to that of Hutchinson, numerous observers, among whom may be mentioned the names of Goodwyn, Davy, Thompson, Kentisch, Abernethy, Menzies, Kite, Allen, Pepys, Seguin, Herbst, and many others, attempted to measure the vital capacity of the lungs; but the instruments they employed and the results they obtained, alike failed to benefit to any great extent either practice or science. Dr. Hutchinson was the first to invent for this purpose what may be called a truly philosophical instrument; and by its use he was enabled to discover that in man the breathing capacity bears a certain relation to stature, and that this relation is much influenced by pulmonary diseases, especially tuberculosis. On account of the discovery of the changes produced upon the vital capacity in certain affections of the lungs, his observations acquired considerable practical value in the diagnosis of these complaints. Moreover, his researches led him to the conclusion that the arithmetical relation existing between the vital capacity of the thorax and the height of the individual, is but slightly altered either by the age or the weight of the body.

Dr. Hutchinson has divided his subject into the following heads:

"Firstly, the quantity of air expelled from the lungs, in connexion with other physical observations on the human frame.

"Secondly, the absolute capacity of the thorax with cubic superficial and longitudinal measurements.

"Thirdly, the respiratory movements and mobility of the chest.

"Fourthly, the inspiratory and expiratory muscular power.

"Fifthly, the elasticity of the ribs, and estimate of the voluntary respiratory power.

"Sixthly, the effect of the decussating, diametrical, and oblique power, in reference to the function of the intercostal muscles.

"Seventhly, general and practical deductions to detect disease by the spirometer, with the method of its application." (par. 15.)

Our author submitted to observation no less than 2130 persons, chosen from almost all classes of society—gentlemen and beggars, soldiers and sailors, giants and dwarfs, girls and pugilists, firemen and watermen, healthy and diseased; and in order to guard against the disturbance caused by adventitious circumstances, each individual was subjected to at least three observations, it being frequently found that, owing to the nervousness or awkwardness of the patient, the result of the first and second could not be deemed conclusive. Dr. Hutchinson found that the vital capacity of the lungs principally depended on three things—firstly, the height of the individual; secondly, the weight of the body; and thirdly, upon age. The first of these being the most important, as we shall have occasion to point out.

The knowledge of the vital capacity of the chest has been further

extended by the researches of Davies, Green, Walshe, and Pereira in England; Fabius and Schneevogt in Holland; Vogel, Simon, Haeser, Albers, Stellwag, Küchenmeister, and Wintrich in Germany; and lastly, by the important work just published by Arnold, which we shall notice in conjunction with that of Hutchinson. Although all the writers agree upon general principles, they are at variance upon several important details; by comparing their researches and conclusions, we hope to present to our readers a tolerably correct idea of the prevailing views regarding the points at issue.

No single individual can perfect any particular department of science. Far from being astonished, therefore, at the inability, even of the lucid mind of Hutchinson, completely to master his subject, we are, on the contrary, inclined to marvel at the number of difficulties which yielded to his inquiries. Viewed in this light, the following remark from his pen will not be unexpected:

> "We have seen," he says, "that the amount of air taken into the lungs at each inspiration corresponds with the height and not with the absolute capacity of the thorax. Why is this the case? I confess myself as much at a loss to explain it as I was the first day I commenced the research. I believe the vital capacity is commensurate with the range of mobility or thoracic movement; but why the mobility increases in arithmetical progression with the height, which appears chiefly dependent on the length of the limbs, and not on the length of the trunk of the body, I am completely incapable of explaining. So completely are mobility and vital capacity affected by the stature, that a man will breathe in different positions different quantities of air. Thus, standing, I blow 260 cubic inches; sitting, 255; and when recumbent (supine), 230; prone, 220: position making a difference of forty cubic inches." (par. 117, p. 196.)

We shall soon see how subsequent observers, and more especially Arnold, have been able to explain paradoxes by which Hutchinson acknowledges himself to have been puzzled.

Besides inventing an instrument to measure the vital capacity of the chest, Hutchinson constructed another to test the inspiratory and expiratory power, which varies greatly in different individuals. This last-mentioned instrument, composed of a simple curved tube, containing a column of mercury as a resistance against the respiratory power, enabled him to demonstrate that, in men standing five feet seven or eight inches, the inspiratory power is at its maximum; and from this height, contrary to what might have been anticipated, strength gradually decreases with the increase of stature. Thus, on an average, men of five feet seven or eight inches, elevate a column of mercury three inches, while men of six feet cannot raise it more than two and a half inches. We see that the researches of Dr. Hutchinson were of two distinct kinds. One was directed to the measurement of the absolute volume of air given out by a forced expiration; the other, to testing the relative strength of the respiratory power. In a practical point of view, the former series of researches is most useful in private practice, the latter in selecting men for military and naval service.

We cannot give a more striking example of the diagnostic value of the measurement of the breathing capacity by the spirometer, than by quoting the case of Freeman, the American giant:

"This man came over to England in 1842, and, in the November of that year, trained for a prize fight. I examined him immediately before his *professional engagement*, when he might be considered in the 'best condition.' His powers were as follows:—Vital capacity, 434 cubic inches; height, 6 feet 11½ inches; weight, 19 stone 5 lbs.; circumference of his chest, 47 inches; inspiratory power, 5·0 inches; expiratory power, 6·5 inches. In November, 1844, exactly two years afterwards, he came to town in ill-health. I then examined him in the same way as before, twenty times, at various intervals, during which his vital capacity varied from 390 down to 340, and the mean of all the observations was 344 cubic inches —a decrease of 90, or more than 20 per cent. His respiratory power had decreased one-fifth, and his weight 2 stone in the whole period. At this time I took him to two physicians well skilled in auscultation, and they both affirmed that they *could not detect* any organic disease. After January, 1845, I lost sight of Freeman; and in the October following, I was kindly favoured with the following account of him from Mr. Paul, surgeon to the County Hospital, Winchester:

"'Freeman was admitted into this hospital on the 8th of October, in an extreme state of debility and exhaustion; he was reduced almost to a skeleton, complained of cough, and was expectorating pus in large quantities. Percussion on the anterior part of the chest, *under the clavicles*, gave on the right side a very dull sound; on the left one, much clearer, but still, I think, less resonant than natural. I made but one attempt at auscultation, but could come to no conclusion, for a rather singular reason—the ribs were so large, the intercostal spaces so wide, and so sunk in from the extreme state of emaciation to which Freeman was reduced, that I could not find a level space large enough to receive the end of the stethoscope—could not, in short, bring its whole surface into contact with the chest. Freeman's great debility, and the clearness of the diagnosis from other sources, prevented my repeating the attempt. Freeman after death measured 6 feet 7½ inches, and weighed 10 stone 1 lb. On opening the chest, the lungs on both sides were found adhering by their apices to the superior boundaries of the thorax, and studded throughout their substance with tubercles. The tubercles, on the whole, were much less numerous in the right lung than in the left; both lungs were nearly healthy at their base; the tubercular matter gradually increased in quantity towards their upper parts, and the apices of both lungs were almost completely occupied by large cavities partly filled with pus, and capable of containing two or three ounces of fluid each."' (pars. 168, 169.)

This one case is sufficient of itself to show the importance of the spirometer in detecting the incipient stage of pulmonary disease, at a period when the other physical means of diagnosis are incompetent to the task. We may add a few words upon its use, and upon the results that have been obtained by its agency.

We have already drawn attention to the opinion of Hutchinson, that the vital capacity of a healthy individual depends principally upon stature, weight, and age, and is most powerfully influenced by the first. So intimate indeed is the connexion existing between the stature of the body and the capacity of the thorax, that for every inch of height (from five to six feet) eight additional cubic inches of air, at a temperature of 60° Fahrenheit, are given out by a forced expiration. The second element, weight, is of minor importance, and cannot be so easily estimated, inasmuch as the weight of an individual increases with the height. In order to obtain the true relation between the weight of the body and the vital capacity of the lungs, Hutchinson first attempted to fix the average normal weight for a given stature, and then proceeded to compare the result with the amount of air expelled by a forced expiration. Numerous observations on this point led him to the conclusion,

"That the vital capacity increases nearly in the ratio of one cubic inch per pound from 105 to 155 lbs; and that from 155 lbs. to 200 lbs. this increase is overpowered, and there is a loss of thirty-nine and a half cubic inches, as the effect of weight. Therefore all weight under eleven stone and a half does not interfere with the vital capacity; but, on the contrary, it increases with the weight up to this point; but above this weight, so far as our table goes (namely, to fourteen stone), the weight interferes with the vital capacity, preventing this increasing progression in the relation of rather more than one cubic inch to the pound. . . . . The weight of man naturally increases with the stature, therefore the relation between the weight and the vital capacity must also vary at different heights." (pars. 55, 56.)

Our author found the influence of the third element, age, on the vital capacity, to be less than that of either of the preceding ones; and concluded, from extended observations on individuals at different periods of life, that from the fifteenth to the thirty-fifth year of age it increases; decreasing, on the other had, in the progression of from nineteen, eleven, and thirteen cubic inches, from the thirty-fifth to the sixty-fifth year.

According to Hutchinson, the circumference and length of the chest have little or no influence in regulating the amount of air taken in at each inspiration; first, because the circumference of the thorax increases with the weight of the body in exact arithmetical progression of one inch for every ten pounds; secondly, because the length of the chest, according to his observations, varies but slightly, the stature of a human being depending on the length of the legs, not upon that of the trunk.

"One man of six feet and half an inch standing, sat only two feet eleven and three-eighth inches; while another of five feet six inches standing, sat three feet high; and therefore the standing height does not appear to correspond with the sitting height, or the length of the body with the length of the trunk." (par. 90.)

The mobility of the chest, Hutchinson, on the other hand, ascertained to have a powerful influence over the vital capacity; the mobility was calculated by a double measurement of the circumference of the chest, immediately above the nipples, with an ordinary tape measure: firstly, during a deep inspiration; secondly, after a full expiration; the difference between the two observations giving the mobility of the thoracic walls. This was observed to vary considerably in different persons; and from a number of examinations he educed the average mobility, in people of middle stature and weight, to be about three inches, seldom four.

These conclusions, at which Dr. Hutchinson arrived after many careful, and oftentimes complicated, inquiries, have been for the most part corroborated by subsequent observers.

Professor Arnold has availed himself in an admirable manner of the published researches of Hutchinson and others of his predecessors. The labours of this accomplished physiologist are fraught with a peculiar value. He employed and compared the various methods adopted by Hutchinson, Fabius, Wintrich, and other observers, and drew out a series of extensive and most useful tables from the results of their calculations and his own. He estimated the average vital capacity of the lungs, for every additional inch of stature, length of trunk, and circumference of chest; tested the influence of weight at the different heights, and fixed the average thoracic mobility at each stature, in order to arrive at a just appreciation of the influence exerted by size, weight, length of

trunk, circumference and mobility of chest, on the vital capacity. He also pointed out the influence of the mode of life, of social position, of age, sex, and other differences in the bodily state, so that he may be said to have given to the spirometer a far greater practical value than it before possessed. A single glance at Arnold's tables will indicate to the practitioner what ought to be the normal vital capacity of the lungs in any given case, and enable him to state whether his patient breathes a greater or less amount of air than the average of individuals under similar circumstances.

Simon has for the most part confirmed Hutchinson's views. He also found that the vital capacity bears a direct relation to the height of the individual, increasing with the increase of stature, in a fixed scale. According to Simon's observations, the advance in the vital capacity from a stature of 156 centimètres (62·4 inches) to 180 centimètres (72 inches), is 1350 cubic centimètres (86·4 cubic inches). This gives for every $2\frac{1}{2}$ additional centimètres (1 inch) in a person's height, an addition of 150 cubic centimètres (9·6 cubic inches) in the vital capacity, 19·6 cubic centimètres (1·2544 cubic inch) more than were found by Hutchinson. Simon and J. Vogel endeavour to explain this discrepancy, by asserting that the experiments of Hutchinson, being made chiefly upon men of very strong constitutions, yielded something more than the average individual capacity. Simon's own experiments had been made upon students between the ages of seventeen and twenty-five, whose constitutions he supposes to have neither more nor less than an average strength. Arnold, in his tables, while steering a middle course between Hutchinson and Simon, nevertheless, in his calculations, gives to the former observer the credit of superior accuracy, asserting Simon's results to be far below the average.

Fabius boldly opposed the assertion made by Hutchinson and Simon, that the vital capacity stands in direct arithmetical relation to the height of the body. The fact already discovered by Simon and Hutchinson, that in some cases the vital capacity cannot be subjected to the rules laid down for its measurement, is asserted by Fabius to be in itself a direct proof of the uselessness of endeavouring to establish a graduated scale between stature and breathing capacity. He even goes so far as to declare, that if any one would measure the ears of a thousand men, and try to discover the relation existing between the size of the ears and the capacity of the thorax, the results would not prove less satisfactory. He states that we must seek in the length of the trunk and the mobility of the thoracic walls, an answer to the question, "What is the vital capacity?" We might, he says, judge *à priori* of the vital capacity of the lungs from the capacity of the thorax; for the greater the circumference and length of the chest, the more will the pulmonary organs be able to expand, downwards as well as laterally; and the greater the mobility of its walls, the greater will be the capacity of the thorax. It will be necessary, therefore, before the vital capacity of a man can be theoretically determined, to measure the length, circumference, and mobility of the chest. As the measurement of the length of the chest is attended with difficulty, he proposes, starting with the idea that the thorax invariably constitutes a fixed portion of the trunk, to measure the latter.

As the chest, however, has not the form of a perfect cylinder, and does not expand in a fixed ratio, the formula according to which the capacity of a cylinder would be found, cannot be applied to it, but a more universal one must be sought.

It appears to us that Fabius has fallen into just as great an error as the one he condemns: when asserting the difficulty of measuring the length of the thorax, he proposes as a substitute the measurement of the trunk, falsely pre-supposing that the thorax bears a distinct and unvarying relation to the abdominal cavity. In two *perfectly formed* persons of equal stature, the size of the thorax will in both cases be the same; and this size will not only be proportional to the whole length of their bodies, but also to the length of their trunks; so that whether the capacity of the chest be calculated in proportion to the stature or to the length of the trunk, the result will in either case be the same. This exact symmetry of body is, however, rarely, we might say scarcely ever, found, and least of all among civilized nations, where occupation, habits of life, and social position have so variously influenced the development of the human frame. In some persons, we find an excessive development of the limbs; in others, of the trunk. In some the cavity of the chest is long and narrow; in others, short and broad. But as, for a given size of body, a fixed amount of air must be necessary for the maintenance of perfect health, the cavity receiving that air must have a normal cubic capacity, so that if it is long, it may be proportionately narrow, and what it wants in length, it may make up in breadth. We should therefore consider it wisest, supposing such a course practicable, to leave all measurements of stature and length of trunk alike out of the question, the inquirer confining himself to the dimensions of the thorax alone. This is the opinion held by Professor Arnold. We shall have occasion to recur to it.

Finally, Fabius inferred from his experiments, that the vital capacity depended upon four things. Firstly, on the length of a man's trunk (imagining the length of the thorax to be always in proportion to that of the trunk); secondly, upon the circumference of the chest at the nipples; thirdly, on the mobility of the thoracic walls; fourthly and lastly, on the age. He agrees with Albers* in the statement, that strong men respire more than weak ones, and thinks, with Hutchinson, that the position of the body has a great influence on the quantity of air expelled from the lungs by a forced expiration. He holds, with Küchenmeister, that the vital capacity of the female is not at all diminished at the time of pregnancy.

We cannot refrain from here noticing the valuable paper published a short time since by Wintrich,† and will briefly sum up the result of his observations, which derive great importance from having been made upon no fewer than 3000 males and 500 females, of all ages between the ages of six and ninety. This learned observer has paid particular attention to the influence of stature, age, sex, position of body, effects of food and drink, state of bowels, of pregnancy, and rapidity of respiration. From his researches, he concluded that no importance is to be attached to the weight of the body, or even to its height, unless taken in conjunction with the age.

* Medicinische Wochenschrift, Sept. 1852.

† Handbuch der speciellen Pathologie und Therapie, Band v. Abtheil. 1.

According to Wintrich, the breathing capacity is at its maximum between the ages of twenty and forty; and this is exactly what theory would lead us to anticipate, seeing that every organ requires for the performance of its function a certain amount of oxygen; and as the quantity of oxygen must increase or diminish with the increasing or diminishing activity, we should naturally expect that the greatest quantity would be required for the system at the period when the frame has reached its most perfect state of development, with every function in most active operation. As the lungs are the principal organs through which this interchange of gases takes places, it is natural to conclude that during the prime of manhood, with every function of the animal body at its climax, that of respiration would likewise be at its maximum.

We have already mentioned the importance attached by Wintrich to age, in connexion with vital capacity; and that he should do so will not excite surprise when we glance at the result of this observer's calculations. In children of both sexes, between the ages of six and eight, he found only between 6·5 centimètres (2 6 inches) and 9 centimètres (3·6 inches) of expired air to every centimètre of stature. Between the ages of eight and ten years the proportion is from 9 cent. (3 6 in.) to 11 cent. (4·4 in.) of expired air to every centimètre of stature; from ten to twelve years it averages 11 cent. (4·4 in.) to 13 cent. (5·2 in.), to 1 cent. ($\frac{2}{5}$ths of an inch) of height. Between twelve and fourteen years the proportion is from 13 cent. (5·2 in.) to 15 cent. (6 in.), to 1 cent. ($\frac{2}{5}$ths of an inch) of stature. Wintrich made few observations on persons beyond the age of fifteen; but the few experiments he instituted had very noteworthy results. Between the fortieth and fiftieth year of life no very appreciable variation in the vital capacity occurs in either sex; between the fiftieth and sixtieth years, however, an important diminution is observed, induced by two causes—the commencement of atrophy of the lungs, and the obesity frequently accompanying this period of life. From the sixtieth to the ninetieth year a great decrease in the vital capacity takes place; but it cannot be reduced to an arithmetical scale.

Various observers had endeavoured to test the value of age in connexion with vital capacity, without arriving at any satisfactory result; the reason of this failure being, that their examinations were confined to persons in the middle period of life. Arnold has reduced the researches of Hutchinson to the following tabular form:

"From the twentieth to the thirty-fifth year there is an increase in the vital capacity of 135 cubic centimètres, in the following proportion:—

" From the	20th	to the	25th	year, increase of	10	cubic cent.	(·64	cubic inches).
„	25th	„	30th	„ „	28	„	(1·792	„).
„	30th	„	35th	„ „	97	„	(6·208	„).

"From the thirty-fifth to the sixty-fifth year there is a decrease of 888 cubic centimètres in the amount of air expired. The proportion is as follows:—

" From the	35th	to the	40th	year, decrease of	266	cubic cent.	(17·024	cubic in.).
„	40th	„	45th	„ „	172	„	(11·008	„).
„	45th	„	50th	„ „	78	„	(4·992	„).
„	50th	„	55th	„ „	64	„	(4·096	„).
„	55th	„	60th	„ „	181	„	(11·584	„).
„	60th	„	65th	„ „	127	„	(8·128	„).

(Arnold, p. 79.)

Here it is seen that, in the 1st period after the twentieth year, the increase is smaller than in the 2nd; and in the 3rd it is considerably greater than in either of the previous ones. On the other hand, the decrease in the 1st period is by far the greatest; in the 2nd it is less; and in the 3rd and 4th smaller still; while in the 5th and 6th it again becomes more marked.

It is not a little amusing to note the difference of the conclusions arrived at by various observers on the same subject. Two observers, even, reasoning on the same fact, offer various, and not unfrequently opposite, explanations.

Wintrich, we perceive, states as his firm belief that age has a most important influence over the vital capacity; and that in the examination of all cases, especially of persons not yet arrived at puberty, and also of those in the decline of life, this influence must be taken into most careful consideration.

After having learned the opinions of other observers on the same subject, this assertion of Wintrich's will at first sight appear somewhat surprising; a moment's thought, however, will explain the apparent anomaly, if we bear in mind that Hutchinson, Simon, and the rest, who attached no importance to the age of the individual under examination, drew their conclusions exclusively from the observation of persons in middle life; while Wintrich went to the very extremes of early and latter age, at which periods alone the influence of age in increasing or diminishing the vital capacity is so definite as to admit of no doubt. His observations are scarcely sufficiently numerous to enable us to estimate with perfect accuracy the effect of age upon the amount of respired air, but they are sufficiently marked to arrest the attention of future inquirers.

The vital capacity of females has been observed to be somewhat less than that of males of equal stature, the average of this diminution being from 6 to 6½ cub. cent. This difference is even smaller than we might have been led to anticipate, when we consider the various modes of life of the two classes. It has been always found that people of sedentary habits respire less than those leading an active out-door life; and the occupations of women are, we know, considerably less active and much more sedentary than the generality of avocations pursued by men. Arnold, moreover, adduces, in addition, the following physical causes as explanatory of this diminished vital capacity: — Firstly, the relation between the depth of the chest and the length of the body; secondly, the relation existing between the thoracic cavity and its external circumference being less in women, in consequence of the size of the mammary glands and the surrounding fatty deposit; thirdly, the diminution in the mobility of the thoracic walls, consequent upon the female mode of dress; and this last cause plays a most important part, for the same female, relieved of her corset, will respire from 100 to 200 cub. cent. more than when encased in her stays.

The smaller vital capacity of women, in comparison with men, may be said, therefore, to depend upon the four following points:—Firstly, the relation between the stature and the length of the thorax; secondly, the small circumference of the internal thoracic cavity when compared to

that of its external surface; thirdly, the diminished thoracic mobility; fourthly, the mode of life.

While we are on the subject of mode of life, in its influence upon the breathing capacity, it may not be superfluous to remark that Arnold's tables demonstrate very distinctly the alteration produced by occupation and mode of life in the vital capacity. He found, for example, that in general the upper classes, students, and paupers (persons not subjected to bodily exertion) have a smaller vital capacity than the labouring classes, such as mechanics, printers, firemen, &c. On the other hand, sailors, soldiers, and recruits have a higher breathing capability than either of the preceding classes. So great is the diversity in these three classes, and so important does our author consider it, that in the calculation of the vital capacity he recommends particular attention to be paid to the class of which the person under observation is a member. If he is to be ranked in the first class, 100 cubic centimètres (6·4 cubic inches) should be deducted from the average amount of vital capacity as given in the tables; if to the second class, 100 cubic centimètres (6·4 cubic inches) must be added; and should he be a member of the third class (sailors, &c.), an addition of no less than 300 cubic centimètres (19·2 cubic inches) is to be made. The low vital capacity of the upper classes and students doubtless depends upon their sedentary mode of life, and the small amount of muscular labour they undergo. The favourable effects of constant employment in the open air are, on the other hand, no less clearly demonstrated in the great vital capacity of sailors and soldiers. The mechanic, again, who, though actively employed in bodily labour, is subjected to a certain amount of confinement, occupies, as might have been expected, a middle position between the other two classes, in respect to vital capacity. The very small breathing power possessed by paupers appears to arise from their having insufficient material to support the respiratory process, it being a well-known fact that in cases of inanition not only the quantity of oxygen absorbed and of carbonic acid exhaled, but even the number of respirations, are notably diminished, the very first day after insufficient nourishment has been taken.*

We have already spoken at some length of the effect of stature on the vital capacity. We must now notice the results of a few of Arnold's very complete series of experiments. Some of these the writer of this article had the privilege of witnessing; he can therefore testify to the care and exactitude with which they were made. These experiments had for their object the appreciation of the relative value of the measurements of stature as asserted by Hutchinson, over the vital capacity in comparison with the measurement of the trunk *per se*, as instituted by Fabius; and the observer came to the conclusion that, although neither of his predecessors was entirely wrong, yet neither was exactly right. Indeed, he believes, as we have already mentioned, that inasmuch as it is the extent of the thoracic cavity alone which regulates the quantity of air the chest is capable of containing, it would be far better to put aside entirely all measurement of the length of the trunk and the height of the person, and consider simply the dimensions of the thorax *per se*. The only measurement of the thorax attended with any difficulty is that of its length;

* Bidder und Schmidt, Verdauungssäfte, § 370. Leipzic, 1852.

but this difficulty may be almost obviated by a measurement of the length of the sternum, which will give a tolerably accurate idea of that of the thorax. The circumference of the chest, which is easily taken, Arnold, in common with Buys, Ballot, and Fabius. considers an important factor in estimating its capacity—i.e., when the increase of circumference is not due to an excessive muscular development, an abnormal deposit of fat, or other diseased condition. For wherever he found an increase of 5, 10, 15 centimètres (2·, 4·, 6·, inches) in the circumference of the thorax, in healthy individuals of similar age, height, and weight, there resulted an increase of 100 cub. cent. (6·4 cub. in.), or even more, of vital capacity. He therefore entirely differs in opinion from Hutchinson and Donders, who imagine that measurements of the chest, so far as regards circumference, may be left entirely out of the question in the calculation of vital capacity.

The third point in the measurement of the chest, and the only one upon which all observers coincide, is its mobility or expansibility; Hutchinson was the first to point out its agency. He found, as might have been *à priori* expected, that the more the walls of the chest expand, the greater is the volume of air they can contain. This is in a remarkable degree demonstrated in Arnold's tables. If the mobility or expansibility of the chest be increased from 3 to 11 cent. (1·2 to 4·4 in.), the vital capacity receives an augmentation of about 1256 cub. cent. (80·384 cub. in.). The increase is thus seen to be very marked; and it will be observed that it increases in as regular an arithmetical progression as is found either by height or circumference.

The next observation of Arnold's which in the progress of our subject it behoves us to notice, is one made in connexion with the weight. His views on this point come into collision with those of the plurality of observers. The relation between the weight of the body and the vital capacity can only, he opines, be said to exist in as far as an increase of weight generally accompanies an increase of height. The weight of the body, independently viewed, cannot be invested with any importance as bearing upon vital capacity; for with the same weight of body the vital capacity increases with the height, while with similar heights it does *not* perceptibly increase with the weight. On the contrary, the inquirer is not unfrequently staggered by the fact that a man weighing several pounds more than another of equal stature, possesses a more limited capacity; so that we often see the lighter individual respiring the greater amount of air.

We have already had occasion to indicate the circumstance, that the vital capacity is influenced to a considerable extent, even in persons with a perfectly-formed chest, and who are entirely free from pulmonary complaints, by the various states of the system and its organs. In illustration of this assertion, Arnold relates an interesting case of the effect of hypertrophy of the liver in a youth aged twenty, whose stature was 175 cent. (70 in.), with a thoracic circumference of 82 cent. (32·8 in.), and whose volume of expired air was reduced from 3750 cub. cent. (240 cub. in.) to 2561 cub. cent. (163·904 cub. in.), apparently from the effects of this hepatic disease. At the time of the first examination, the liver of this youth was visibly enlarged, and the epigastrium painful to the touch; while a year later,

the patient respired 3300 cub. cent. (211·2 cub. in.), the cardiac region being much less painful, and the liver considerably diminished in size. No doubt, as the disease continued to abate, the vital capacity would proportionately augment; it is to be regretted, however, that as the patient did not again present himself for inspection, this could not be proved by a renewed examination. The state of the stomach is equally important in its effect upon the breathing capacity; a hearty meal is not unfrequently found to have the power of reducing the vital capacity of the lungs to the extent of 100 to 200 cub. cent. (6·4 to 12·8 cub. in.) from its former volume. In further illustration of this principle, we may quote the experience of Fabius, who tells us that his servant respired no less than 250 additional cub. cent. (16 cub. in.) after having his bowels well purged by the administration of a very powerful cathartic. The tendency of all these facts is to point out the necessity of taking closely into consideration the condition of the abdominal organs and their contents before we can expect to acquire an accurate knowledge of the state of the thoracic cavity by the employment of the spirometer. It is remarkable that, while an hypertrophied liver, a distended stomach, or a costive bowel, exerts such an influence, the gravid uterus, even in the latter months of pregnancy, when it fills up and distends the abdomen to an enormous extent, is said to manifest no action whatever over the vital capacity. This apparent anomaly can only be explained by the supposition, that as the uterus enlarges, the abdominal cavity extends in a corresponding ratio; so that towards the end of pregnancy, the space in the abdomen unoccupied by the gravid organ is not less in extent, although different in form, than in the unimpregnated state. Arnold has attempted to explain this condition by supposing—firstly, that in females, the extension of the thoracic cavity takes place more in the upper part than in males; and secondly, the decrease of the mobility of the diaphragm from above downwards, consequent upon the enlargement of the uterus, being compensated by the increase in the diameter of the base of the thorax, so that the minus in the length of the chest is counterbalanced by the plus in its breadth. This alleged independence of the vital capacity with regard to pregnancy requires further investigation, as not one of the explanations hitherto given of the fact (if fact it be) can be considered satisfactory.

Our limits, however, forbid our devoting more space to the spirometer; we must therefore now confine ourselves to the enumeration of the circumstances to be regarded in calculating the vital capacity of a healthy person. They are—firstly, the stature; secondly, the circumference of the chest; thirdly, the mobility of the thoracic walls; fourthly, the age; fifthly, the position and occupation; and sixthly, the sex.

To facilitate the calculation of these points in determining the vital capacity in any individual, with a view of judging of his state of health by the comparison of his respiratory powers with those of the average of healthy persons of a similar physical conformation, Professor Arnold has carefully prepared two tables—one for men, another for women, as the vital capacity in the latter sex is somewhat smaller in relation to physical measurement than in males. In these tables, the average normal vital capacity can at a glance be found for any stature between 154 and 191 cent. (61·6 and 76·4 in.), and any circumference of chest between 65 and 100 cent. (26

and 40 in.). It is now an established fact that, in men of equal stature, the vital capacity may vary considerably; and that where the mobility is equal, the difference is principally regulated by the difference in the circumference of the chest. Thus, for instance, a man of five feet eight inches in stature, with four inches mobility of chest, and a circumference of thirty inches, possesses a smaller vital capacity than a man of similar stature and thoracic mobility and thirty-four inches of circumference. The operator has therefore to measure his patient, refer to the tables, and add the quantity given for the circumference to that found for the stature, and divide by 2, in order to discover the normal vital capacity of the person under observation. As a great muscular development or a deposit of fat, however, makes a difference in the circumference of the chest of from two to eight inches, and the before-mentioned tables are calculated at an average development in determining the vital capacity of a man of spare habit, two inches ought to be added to his circumference, and in one of excessive development from two to eight deducted, according as the case may require. In females of very spare habit of body, no less than four inches are to be added to the circumference; and in those of excessive development, from two to four deducted from the found circumference of chest. Another point, and one of perhaps still greater importance, must not be passed unnoticed. This is the thoracic mobility, which modifies to a great degree the quantity of air respired, whether this quantity exceed or fall below the average of persons taken as a healthy standard. The mobility of the chest, though it cannot be said to increase in any marked degree with the circumference, is observed, on the other hand, to progress regularly with the height of the body. Arnold has explained this fact by assuming that, in tall persons, the distances between the ribs are greater than in those of a less stature; while in persons of the same height, even where the thoracic circumference is very different, the width of the intercostal spaces remains the same. The wider the intercostal spaces, the greater, *cœteris paribus*, must be the mobility of the chest. It is probable, in this relation, that we may trace the reason why the mobility of the thoracic walls increases with the stature of the body, and not with the thoracic mobility. The proportion of this increase is seen in the following table:

"Stature	157—160	cent.	(62·8—64 in.),	thoracic mobility	6·5	cent.	(2·6 in.)
„	161—165	„	(64·4—66 „)	„	6·5	„	(2·6 „)
„	166—170	„	(66·4—68 „)	„	6·85	„	(2·7 „)
„	171—175	„	(68·4—70 „)	„	7·49	„	(2·99 „)
„	176—180	„	(70·4—72 „)	„	7·80	„	(3·1 „)
„	181—185	„	(72·4—74 „)	„	8·30	„	(3·3 „)
„	186—191	„	(74·4—76·4 „)	„	8	„	(3·2 „)

"Here the mobility of the thorax is seen to increase with the stature, and had the observations been further extended, the progression would doubtless have been found much more regular. In round numbers, the following may be supposed to be the average relation between stature and mobility of chest.

"Stature	157—165	cent.	(62·8—66 in.),	thoracic mobility	6·5	cent.	(2·6 in.)
„	165—170	„	(66 —68 „)	„	7·0	„	(2·8 „)
„	171—175	„	(68·4—70 „)	„	7·5	„	(3·0 „)
„	176—180	„	(70·4—72 „)	„	8·0	„	(3·2 „)
„	181—191	„	(72·4—76·4 „)	„	8·5	„	(3·6 „)"

(Arnold, p. 70.)

The valne of this factor (thoracic mobility) in calculating the vital capacity, has been distinctly pointed out by Arnold. He has, for example, shown, that if a man of 166 cent. (5ft. 6½ in) in height, with a thoracic circumference of 80 cent. (32 in.), having a mobility of 7 cent. (3 in.), respires 3420 cub. cent. (21·88 cub. in.), being the average amount given in the tables; another man of the same height, and with the same circumference of chest, but with a mobility of 8 (2·3 in.), will respire 180 cub. cent. (11·55 cub. in.) more, or 3600 (230·4 cub. in.). If he have a mobility of 9 cent., which is 2 in excess, he will respire twice 180 cub. cent. (11·55 cub. in.) more, or 3780 cub. cent. (241·92 cub. in.). On the other hand, if instead of an excessive mobility, we have a diminution, as for instance when, with a similar stature and circumference, the mobility is 6 cent. (2·4 in.), instead of 7 cent. (3 in.), the value of 1 cent. ($\frac{2}{5}$ in.) of mobility (= 180) (11·35 cub. in.) is to be deducted from the normal amount of air respired. Ex.: 3420 — 180 = 3240 cub. cent.

It is unnecessary to repeat the remarks already made with regard to the calculations necessary for a determination of the vital capacity of persons belonging to the various classes of society, which have already been indicated as exercising a material influence over the quantity of air respired. When the volume of air emitted into the spirometer by a forced expiration is found to be from $\frac{1}{10}$th to $\frac{1}{8}$th less than the physiological average, as set down in the tables, a diseased state of the pulmonary organs can with every probability be assumed, and the abnormal condition is most probably one of tubercular disease. Indeed, one need scarcely dread giving such a diagnosis, even where neither the stethoscope nor percussion is sufficiently acute to detect the least sign; for, as has been already said, the spirometer is a physical means far more delicate than either in the detection of this complaint. Wintrich, who perhaps has had greater opportunity than any other inquirer for examining the vital capacity in persons suffering from pulmonary disease, considers the spirometer a more valuable means of detecting phthisis in its earlier stages, than either auscultation or percussion; and Schneevogt, moreover, corroborates his further assertion, that he could diagnose tuberculosis by the spirometer, when every other means had failed. Hutchinson has delivered as his opinion that, in the first stage of tuberculosis, the diminution in the vital capacity averages from $\frac{1}{10}$th to $\frac{1}{2}$, and in the second stage from $\frac{1}{6}$th to $\frac{1}{2}$, of the normal amount.

In other pulmonary complaints, especially in their acute stage, the spirometer can scarcely be said to render efficient service; for while acute inflammation is present, each effort at forcible expiration induces an attack of coughing, which completely destroys the exactitude of the observation. The following facts, however, gathered by Arnold, deserve notice:

"Firstly. In slight acute bronchitis the vital capacity diminishes from $\frac{1}{15}$th to $\frac{1}{16}$th, and in chronic from $\frac{1}{8}$th to $\frac{1}{3}$rd below its normal condition. Secondly. Immediately after pneumonia or pleurisy, the vital capacity is diminished from $\frac{1}{8}$th to $\frac{1}{3}$th. Thirdly. In cases of exudation into the pleural sac, accompanied with compression of one of the lungs, the vital capacity sinks from $\frac{1}{3}$ to $\frac{3}{4}$ below the physiological average. Fourthly. Emphysema reduces the amount of air expired from 11 to 60 p. c., but this can be traced in a great measure to the very small thoracic mobility always accompanying those cases." (Arnold, p. 131.)

We shall now proceed to enumerate, in regular sequence, the results we have endeavoured in the foregoing pages to explain. We must from these data allow that the spirometer possesses not only a scientific, but likewise a practical, value.

Apart from practical use, the spirometer has rendered efficient service, in a scientific point of view, in revealing to us the following interesting and important moments:

1. The vital capacity of man increases with his stature, in the proportion of 150 cubic cent. (9·6 cub. in.) for every $2\frac{1}{2}$ cent. (1 in.) increase in stature. If, therefore, a man of 155 cent. (62 in.) stature, possesses an average vital capacity of 2700 cub. cent. (172·8 cub. in.), another of 170 cent. (68 in.) stature will have a breathing capacity of 3600 cub. cent. (230·4 cub. in.), and a third person, 180 cent. (72 in.) in height, a capacity of 4200 cub. cent. (288·8 cub. in.).

2. In men, the vital capacity increases with the circumference of the chest, in the same ratio—namely, 150 cub. cent. (9·6 cub. in.) for every $2\frac{1}{2}$ cent. (1 in.) increase in thoracic circumference. If the chest measures 65 cub. cent. (26 in.), the vital capacity will, on an average, amount to 2580 cub. cent. (165·12 in.). With a thoracic circumference of 80 cent. (32 in.), it will increase to 3480 cub. cent. (222·72 cub. in.); and if the circumference be as much as 90 cent. (36 in.), the vital capacity will be no less than 4080 cub. cent. (261·12 cub. in.).

3. The mobility of the chest has a great influence over the vital capacity. It increases with the stature, from 157 (62·8) to 190 cent. (76 in.), about 2 cent. (·8 in.)—i.e., from 6·5 cent. (2·6 in.) to 8·5 cent. (3·4 in.). Its value, however, augments with the circumference of the thorax, in a proportion of 160 cub. cent. (10·24 cub. in.) for every cent. of increased mobility, with a circumference of 75 cent. (30 in.). Where there are 80 cent. (32 in.) of circumference, the additional volume of inspired air is 180 cub. cent. (11·52 cub. in.); and where the thoracic circumference is respectively 85 cent. (34 in.) and 90 cent. (36 in.), the increase of the vital capacity is respectively 210 and 240 cub. cent. (13·24 and 15 36 cub. in.).

4. The vital capacity increases from the fifteenth to the thirty-fifth year of age nearly 160 cub. cent. (10·2 cub. in.), and sinks from the thirty-fifth to the sixty-fifth year about 900 cub. cent. (57·5 cub. in.), at different periods, and in different proportions.

5. The position, occupation, and mode of life, have an undeniable influence on the breathing capacity. At its minimum among paupers and in the higher classes, the vital capacity is highest in sailors, soldiers, and strong young men with out-door occupations, such as recruits, for instance; and finds its medium in mechanics, compositors, and pressmen.

6. In women, the vital capacity is absolutely and relatively less than in men. It increases in the female sex at the rate of 100 cub. cent. (6·4 cub. in.) for every additional $2\frac{1}{2}$ cent. (1 inch) in height, and with the circumference of the chest in similar proportion. Its medium amount in women of 144 cent. (58·4 in.) stature, is 2000 cub. cent. (128 cub. in.). At a stature of 154 cent. (61·6 in.), it averages 2200 cub. cent. (140·8 cub. in.); at 164 (65·6), 2800 cub. cent. (179·2 cub. in.). With a thoracic circumference of 71 cent. (28·4 in.), the vital capacity amounts to 1900 cub. cent. (284 cub. in.); with 81 (32·4 in.) circumference, 2300 cub. cent. (147·2 cub. in.); and with 91 cent. (36·4 in.) cir-

cumference, the vital capacity measures 2700 cub. cent. (127 8 cub. in.). (Arnold, p. 156.)

Not only is the spirometer, as we have shown, a most important help in the diagnosing of certain pulmonary complaints, and consequently exceedingly useful in practice, and a valuable means of ascertaining the eligibleness of men for military and naval service; but it is also a very efficient agent in calculating the probability of longevity for insurance companies. We were not a little surprised when we learnt, on inquiry a short time since, that many of the metropolitan insurance offices had abandoned the use of the spirometer, alleging as the reason of its rejection, that the indications obtained from its employment were very frequently fallacious, and little to be depended on. The small satisfaction derived from the use of the spirometer, cannot, however, be traced to any inadequacy of the instrument itself as a means of calculating the vital capacity, but to the fallacious principles on which the calculations were based, and the absence of reliable tables giving the average physiological capacity. The improvement in the mode of calculating the vital capacity, and the possession of the very complete tables so carefully drawn up by Arnold, for gauging the vital capacity of any individual, must now be considered as having removed the cause for these objections; and we therefore confidently recommend the re-introduction of the spirometer, as a means of testing the chances of life, feeling assured, as we do, that if used on the principles laid down by Arnold, its employment will entail neither unsatisfactory nor fallacious results.

In cases where artificial respiration is required, the good effects of the means employed have not unfrequently been completely counteracted by excess in the volume of air forced into the lungs to resuscitate the asphyxiated person. The delicate air-vesicles of the lungs having been ruptured during the operation, emphysema supervenes, and the patient is not unfrequently rescued from impending suffocation, to become a martyr to a disease which may ultimately prove fatal. An accurate knowledge of the volume of air which the lungs of any individual are capable of containing, is therefore exceedingly important in regulating the application of instruments for artificial respiration; and this knowledge is easily obtained by a measurement of the chest, on the principles of Arnold, already cited in the preceding pages of this article. This knowledge, moreover, is indispensable to the proper application of the beautiful instrument for artificial respiration, invented by Dr. Marcet, which is so constructed as to regulate to a nicety the amount of air admitted into the lungs at each inspiration. We have frequently watched with extreme interest the application of Dr. Marcet's instrument, in the reanimation of dogs purposely asphyxiated, and have been struck by the exactitude with which the volume of air was each time proportioned to the size of the animal, and with the great success which attended the experiments performed. Judging from the success of this gentleman's attempts in the restoration of animals, we should expect an equally favourable result from the employment of Dr. Marcet's artificial respirator in cases of asphyxia occurring in the human subject. We have thus specially called attention to this instrument, from the fact that the highest advantage it is capable of affording, is only to be reaped when the operator avails himself of the information furnished by Arnold's tables.

In concluding our remarks on the interesting labours of Hutchinson, Arnold, and others, it now only remains for us to thank these observers for the great amount of practical as well as scientific information they have afforded us, and to express the hope that, ere long, they may be enabled to remove the uncertainties which still hang around a few points of this important subject. So much has already been achieved, that we are sanguine in our anticipations of seeing the remaining difficulties solved by these philosophical inquirers.

George Harley.

Review VI.

On Unsoundness of Mind, in its Medical and Legal Considerations. By J. W. Hume Williams, M.D.—*London*, 1856. pp. 238.

The human mind, whether in health or in disease, must ever form one of the most interesting and important subjects for meditation and inquiry which can occupy the attention of the physician. The study is necessarily difficult, and the more so because we can scarcely be said to have arrived at any *positive* knowledge on the subject; and if this is true as regards the mind in a healthy state, it is still more so in its diseased conditions—when, in fact, there is such a departure from its normal manifestations as constitute what we call mental unsoundness. Any contribution to our knowledge on a subject so perplexing deserves our thoughtful attention; and we cannot but express our conviction that Dr. Williams has rendered a great service to the profession and to society at large by the very able and lucid manner in which he has discussed this deeply-interesting question. We heartily hope that his book will be generally read, not only in our own, but in the legal profession, where the most mistaken views are adopted, and a degree of knowledge assumed which could only be the result of personal experience among the insane; and this we know falls to the lot of but very few except members of the medical profession. Our author says:

"Counsel acquire their ideas of soundness or unsoundness of mind as some do their notions of special affections, from nosological books which lay down their fixed descriptions of disease. Physicians may, on examination, admit the general truth of the one, and allow the accuracy of the other. Who is there, however, who has stood by the bedside of the sick, and seen the student of the closet, but has felt that the most important part of his knowledge was wanting, or the capability of applying the information he had acquired? The lawyer is this student of the closet! It would be quite as rational to expect that the jury, if guided by his opinion on the soundness or unsoundness of mind in a particular case, would place equal reliance on his advice respecting their individual states of health, from detailing to him certain symptoms, whose value as indications of various diseases, nosological works have with equal confidence laid down. This is a proposition, we are satisfied, to which few would assent; for in their own cases they would ignore the competency of counsel to estimate the practical application of a science which they feel satisfied must be studied in the great volume of nature, written in works, not words." (p. 27.)

In considering the various degrees of departure from the standard of mental health, our author points out the importance of being properly informed as to the natural standard in the particular individual under

examination. It must ever be borne in mind that this standard is as variable as are the temperament, constitutions, and idiosyncrasies of individuals; and that if we would form correct judgments of their mental states, we must carefully ascertain what has been their natural standard of mental health, and then estimate the apparent departure from it as evidenced by conduct, ideas, and general bearing.

Reference is made to the notorious inconsistency of the law as propounded by the judges, who maintain that an offender is punishable, insane though he be, if it can only be shown that he knows right from wrong—a degree of intelligence undoubtedly possessed by a large majority of insane persons. While we fully concur in the principle that an insane person cannot properly be held responsible for acts committed under the influence of his malady, we yet feel bound to dissent from the doctrine advocated by our learned author, of absolute irresponsibility in every case of mental unsoundness. If insanity be a disease, surely it may differ in its intensity; and according to this differing intensity, it appears reasonable that we should estimate the amount of responsibility *quoad* crime. We do not pretend that the most acute and experienced observer can accurately define the precise mental condition of an individual with a view to fixing the amount of responsibility which may be properly attributed to him; and therefore we conceive that wherever *any* degree of mental unsoundness is proved, considerable allowance should be made for a certain other degree, which possibly is not susceptible of proof, but may nevertheless exist. But while fully recognising the principle that unsoundness of mind may properly be pleaded in extenuation of crime, we yet conceive that it is a dangerous doctrine to hold that any trifling amount of mental unsoundness is to be admitted as an excuse for any amount of crime. It argues nothing against the unity of the mental principle that it may suffer *partial* disturbance, and eventuate in partial irresponsibility. An absolute loss of control over the passions and actions of the individual appears by no means a necessary sequence of a minor degree of mental unsoundness. This power of control may surely be impaired without being lost; the mainspring may be weakened without being broken. An individual may be conscious of a morbid inclination to crime—morbid as something opposed to his natural disposition, and resulting from disease—which he may yet have the power to restrain, but which he may take no pains to check. Insomuch as we have reason to believe that he had the power to impose this restraint upon his inclination, and failed to do so, to that extent we hold that he was responsible; and while we readily admit the extreme difficulty of determining the degree of his helplessness, and would therefore make large allowance, we are not prepared to hold him entirely guiltless. We must not forget that society has its rights as well as individuals, and these are not lightly to be ignored in dealing with a class of offenders of all others the most dangerous, because presumed to enjoy an absolute immunity from punishment. Where there exists any degree of mental unsoundness, we fully concur in the views entertained by our author on the subject of capital punishment, and hold that such an exhibition would be a national disgrace; but we are by no means so satisfied that secondary punishments may not be very properly inflicted, if it were only to warn those who are

disposed to presume upon their eccentricities, which may even be assumed, to serve a purpose of revenge. It is impossible to say to what extent the wicked though pretended attempts on the life of the Queen might have been carried, if the absurdity of acquitting on the ground of insanity the vain and ignorant perpetrators of these atrocities had been persisted in. The notoriety, however infamous, was a distinction eagerly sought by such degraded beings, and constituted a powerful incentive to the crime. In some, at least, it is probable that there existed a degree of mental unsoundness, but the wisdom of treating them as criminals notwithstanding, cannot be questioned. The punishment cured the mania, the warning deterred others, and this class of criminals has ceased from among us.

The sentence of penal servitude for life in the recent case of Westron for the murder of Mr. Waugh, marks the commencement of a new era in our criminal legislation of the highest importance, and recognises a principle which we conceive to be both sound and politic. By such a course we cannot commit any serious amount of individual injustice; for while we give to society the protection which such a warning is calculated to afford, and which it unquestionably has a right to claim, we are yet in a position to transfer the offender to an asylum if his mental condition be found really to require such care and treatment.

We regret to find ourselves at issue with our author on this question of partial insanity, a subject, however, which may receive considerable elucidation by impartial and unprejudiced discussion. We therefore strongly advocate the attentive perusal of the work before us, with the conviction that it will amply repay the reader for the time bestowed upon it.

Review VII.

1. *Actstykker angaaende Cholera — navnlig, Epidemien i Christiania i* 1850.

Documents relating to Cholera—viz., the Epidemic in Christiania in 1850.—*Christiania,* 1851. 12mo. pp. 128.

2. *Actstykker angaaende Cholera-Epidemien i Norge i* 1853.

Documents relating to the Cholera Epidemic in Norway in 1853.—*Christiania,* 1854. pp. 199, 106.

3. *Norsk Magazin for Lægevidenskaben.* 1849.

Norwegian Magazine for Medical Science. 1849.

Two years ago we drew the attention of the readers of this Journal to the important Report on Cholera in Sweden, drawn up by Professor Berg, of Stockholm. In the review of that Report, the bearing of the documents collected in Sweden on the great question of the contagion or non-contagion of this disease was prominently brought forward and discussed. We have now the opportunity of performing the like task in reference to the sister kingdom of Norway, a country which in many respects presents equal, if not greater, facilities for studying the mode of propagation of this malady. In proportion to its population, Norway has a more extensive seaboard, and a more lively commerce with foreign countries, and the question of contagion in reference to quarantine regulations is,

therefore, in the latter country, even of more vital interest than in Sweden.

The documents now before us only refer to the epidemics of cholera as they occurred in Norway in 1850 and 1853; but we have also availed ourselves of some most interesting reports on cholera in Bergen, on the west coast of Norway, in 1847, contained in the 'Norwegian Medical Journal' * for 1849. We have not as yet received the reports of the last epidemic in Sweden, in 1853, or of the terrible invasion of cholera at Copenhagen in the summer of the same year.

The report on cholera in Norway in 1850 is drawn up by the Royal Central Cholera Commission; and that of 1853 by the Medical Committee of Christiania. Both are well compiled; both bear the strong impress of that honesty and impartial search after truth so-characteristic of our Norwegian brethren.

In its general features, the cholera of Norway does not differ in the slightest degree from the characters it has presented in other countries which it has traversed. It were useless, therefore, to go over symptoms already sufficiently known; it would be a waste of time and of our readers' patience to detail the pathology and the course of the malady, or to relate the various attempted modes of cure. Each of the last named occasionally succeeded, and as often failed; and here, as elsewhere, it was only towards the close of the epidemic, when its virulence appeared to be subsiding, that remedies seemed to be of much avail. We shall subsequently notice the treatment that found most favour in Norway; but our chief object, in the present instance, is to continue the investigation we commenced in our article on Cholera in Sweden,† and to ascertain how far, in Norway, the progress of the malady tends to corroborate or refute the opinions we there expressed.

When cholera first appeared in this country, in 1831, we acknowledge that we then held, and for many years after continued to hold, opinions opposite to those we now entertain on the question of the contagion of the disease. In 1831, the majority, we may say, of the European practitioners were decided contagionists; but subsequently to that first invasion of the disease a reaction of opinion occurred, and the question was virulently discussed for years without any definite conclusion. In 1848, when the malady again appeared amongst us, many of the higher authorities coincided with the solemn declaration of the Board of Health, that the malady was not in any way contagious, and that no danger was incurred by attendance on the sick. The experience of that year, however, in the town where we now write, led us to an opposite conclusion; and this change of opinion was still further confirmed by what we observed during the last severe invasion of cholera in Newcastle-on-Tyne, in September, 1853.

It seems to us that the tide of opinion is again now in favour of contagion, modified perhaps, and scarcely so exclusive as that doctrine was held by some in 1831, but still contagion or infection—and we shall use these two words simply to express the propagation of the disease from one person to another. We leave it, however, to our readers to determine

* Norsk Magazin for Lægevidenskaben. † See No. for Jan. 1854.

how far these new documents bear upon this question; we shall lay the facts before them, and draw our own conclusions.

Before proceeding to examine the progress of cholera in the East of Norway, we shall turn our attention to two reports in the 'Norwegian Medical Journal' before alluded to, and which appear to us to contain details of much value in reference to the subject.

Cholera first appeared in Norway in 1832, when it broke out, in the month of October, at Drammen. Christiania, which is about thirty miles distant, then escaped; but in the following year (1833) the disease again appeared at Drammen, spread to Christiania, and from thence progressed along the east side of the Skagerack, in the direction of Sweden. Nor did the towns on the western coast of that inlet then escape altogether; but the western coast of Norway remained still wholly free. In 1831, cholera existed in the ports of the White Sea as far as Kola; but it did not extend to Norway, though vessels in which fatal cases had occurred rode quarantine in Hammerfest harbour. Bergen remained free from the epidemic till the winter of 1848, when the first case appeared on the 11th of December. The disease continued to prevail in the town and neighbourhood till the 10th of April, 1849, when the last case was reported. A very full and careful description of the disease is given by Candidatus Medicinæ, T. J. Löberg, who was attached to the Lazareth, or Cholera Hospital, in the town. Bergen contained at that time nearly 24,000 inhabitants: of these, 1024 were attacked by the disease, and 605 of those so attacked fell victims to the pestilence. Of the general symptoms of the malady, as given by Löberg, it is needless here to speak; the character of the disease was the same as in other countries, and the results of treatment not more satisfactory, as is evidenced by the mortality having been nearly 60 per cent. of those attacked. Löberg, with many of his countrymen, is inclined to regard the constant vomiting, which in this country we have so generally striven to arrest, as a favourable symptom, or as an effort of nature which should be assisted rather than opposed. But while we thus summarily dismiss the careful records of the symptoms of cholera afforded us by Kierulf and Löberg, we wish to express our entire satisfaction with the conscientious and diligent manner in which they have performed their task. It is no fault of theirs if their researches, their post-mortem examinations, their microscopical and chemical investigations of the fluids and solids in cholera, have led to no positive results; for may not the same be said of many, if not all, of the bulky volumes and reports which have from time to time been issued since the first invasion of this pestilence? At some future time no doubt all these researches will be of value, when the master mind shall arise to elicit from the mass of reliable documents and records the true nature, pathology, and perhaps even the appropriate treatment, of the disease. Löberg and Kierulf both avow themselves to be contagionists, after the experience they have had of the progress of the disease in Bergen and the adjacent country.

"I would not allude more to the question of contagion," says Löberg, "were it not that the observations I made during this last epidemic have forced me to draw conclusions opposed to those of the majority of writers on the disease. To form a just idea of the spread of the disease by contagion, one must have practised as a

cholera physician; and the facts thus brought before us soon show that the doctrine of the propagation of the disease, drawn as it has generally been from observations made in large towns, is incomplete and hardly to be relied upon. Such is not the case, however, in smaller communities, and particularly in country districts, where the epidemic advances slowly, and each new case attracts immediate attention. Here the propagation of the disease can often be followed in its minutest details.

"The first case of Asiatic cholera appeared in Bergen on the 11th of December, in the house of a watchman, who lived in the greatest poverty. No communication with any infected person or locality could be discovered. The wife and a child of the watchman were first affected, and both died; after that the husband was attacked; and on the seventh day another child of the same family, which had been removed into another house three days before. The wife's mother, who had for a time attended her daughter, lived in one of the poor-houses of the town, the so-called Asylum; and from this place, when she sickened, she was taken to the hospital. Immediately the disease spread through the Asylum, where many individuals resided in each room. It ran from bed to bed and from chamber to chamber, always attacking those who had attended the sick before they were removed to the Cholera Hospital. In this way, not less than twenty individuals were carried off by the disease in this house (the Asylum) before the 22nd of December; up to which time the malady had not spread in the rest of the town, excepting to one house adjoining the watchman's, where two people were attacked on the 18th of December, and to the Sailors' Poor-house, which lies only one hundred paces from the Asylum. Between these two houses there was undoubtedly intercourse, and the first case in the Sailors' Poor-house occurred on the 17th of December. Here the disease ran exactly the same course as it had done in the Asylum, always affecting those who had attended those attacked before them. In the meantime, a hospital was fitted-up in (Christi Krybbe) 'Christ's Manger,' to which the necessary fittings were gradually brought, after patients had already been placed therein, by men from the workhouse; and in this latter place the disease showed itself on the 26th of December. By this time it had begun to spread through the town, so that it was difficult to follow each individual case; but this much is certain, that the malady advanced only step by step, so that the disorder was at its height in some portions of the town while other parts remained perfectly free. The malady first raged in the few houses wherein it first appeared, and then progressed from quarter to quarter without overleaping any considerable space until it reached Nordnaes, the extreme point of the town towards the north, situated on a peninsula, and far distant from the opposite side of the town where it had commenced. Whole families were carried off in the early part of the invasion; and of the 300 sick in the hospital at 'Christi Krybbe,' two-thirds at least stated that they had been in communication with cholera patients, either to nurse them, or to put on leeches, or to lay out the dead. Then the disproportionately great number of the attendants on the hospital who were affected deserves notice; for of these, not less than one physician, eight porters (*pörtörer*), four nurses (?) (*gangkoner*), the cook and her assistant, and afterwards a second girl in the kitchen, several bearers of the dead, two men who took away the straw of the mattresses on which the patients had lain, two who washed for the hospital, and one who carried out the dirty linen, &c., were affected at one time or other with the disease. It seems to me, therefore, that it is impossible to deny here the operation of contagion. To me it is clear that the disease produces *a volatile infection* (*et flygtigt smittestof*). [?] We believe that we observed certain circumstances tending to show that this miasma is not always confined to the immediate neighbourhood of the sick person. In proof of this we may remark, that the malady appeared in certain houses wherein some of the hospital attendants resided, but who themselves remained free from the disease; and in other cases, communication of the disease took place from infected houses by the means of individuals who themselves escaped altogether. Indeed, I can state for certain, that three of

these cases were the very first cases of the disease in the respective quarters of the town in which they appeared, so that it would be difficult to affirm them to be the results of an epidemic influence which had not as yet reached these districts. One example is too striking to be passed over:—While cholera was for the most part confined to the Asylum and the Sailors' Poor-house, a woman took ill of the disease in the latter establishment, and was attended there by her sister. This sister lived in Nordnaes, a quarter of the town which, as I have before stated, was not reached by the malady till several weeks after, for this occurrence took place in the first ten or twelve days of the invasion of cholera. Two days after this sister had left the Sailors' Poor-house and had returned home, a fatal case of cholera occurred in her house in Nordnaes, in the person of a female with whom she resided; and for several weeks this was the only case of cholera in that part of the town." (p. 298.)

The alterations of temperature that took place during the prevalence of the disease were, as might be expected from the season of the year, very considerable. The climate of Bergen is notoriously bad and changeable at all times; and even in the depth of winter, from the proximity of the ocean, there is not that steady, uniform cold temperature which prevails in Sweden and Russia at the same latitude, or even much farther to the south. A few days before the cholera appeared, the thermometer stood at + 11° Reaumur, but a day or two after it sank to several degrees of cold, and then the temperature continued to change, often very suddenly, fogs alternating with clear weather, followed by snow or rain; but all these changes seem to have had little influence on the disease. Nor did the winter's storms seem to affect the progress of the malady. During the last three months of the epidemic, furious winds from different quarters frequently prevailed, but the malady crept on apace, undisturbed by the tempest. We have generally seen that cholera is most severe during close, still weather, such as prevailed so remarkably in this town (Newcastle-on-Tyne) during the severe epidemic of September, 1853; and we believe that a rapid movement of the atmosphere, by dispersing the particles of the miasm, and not permitting it to gain strength by accumulating in any particular spot, is a most effectual bar to the progress of the disease. In 1853, after a fortnight of the stillest weather imaginable, during which the cholera raged fearfully in Newcastle, affecting the higher and better parts of the town to an equal if not to a greater degree than the poorer and lower situations, a sudden abatement of the malady was observed to follow on the occurrence of the equinoctial gales; and the disease never regained its previous intensity. In Bergen, this was not the case; but the habits of the people may possibly account for this difference. In these high northern latitudes, great care is taken to exclude in winter the access of cold air from without; the air within the rooms is seldom renewed, and thus it becomes charged with the miasm to a high degree.

The opinions and experience of C. T. Kierulf upon this subject will be best learned by a short *résumé* of his report on cholera in the vicinity of Bergen, in the same year. It is extracted from the 'Norse Medical Magazine' for 1849, vol. iii. part 8.

It will be seen from this report that Kierulf admits the existence of a certain miasm in the air in cities affected with cholera. We do not think it is possible to deny that such is the case; and we assert, too, that the same miasm floats in the atmosphere when epidemic invasions of

small-pox or of scarlet fever desolate any crowded community. But we differ from many others in regarding this volatile miasm as the product of the bodies of the sick: we do not believe that it is transported to a great distance, except it be carried by individuals. Of telluric influences we know nothing; of atmospheric influence we admit only a condition of the air highly favourable to the concentration of the poison, and such a condition we believe to exist where the air is still and warm, such as it has so frequently been observed to be during the severe invasions of cholera. Under such circumstances the air of a city, being not renewed by fresh currents from the country, becomes tainted with the miasm from the bodies of the sick, like the air of a room that is not ventilated; and in this way perhaps we can explain, in part at least, the rapid extension of the disease during hot weather. Other conditions of the atmosphere, as yet very imperfectly known, probably influence the spread of cholera, such as the amount of ozone, and the relative electrical conditions, which are known to vary so considerably. We do not, however, think that any of these are of themselves sufficient to engender cholera; they only, in our opinion, act as carriers of the miasm, or as a fruitful soil in which the seeds of the poison, once introduced, will rapidly multiply.

Kierulf observes that in the thickly-populated towns and districts of France and Germany the majority of medical men are (or at least were) opposed to the idea of contagion; while it is upheld more or less by the medical authorities of Denmark, Sweden, and Norway. In the last-named country, he remarks, there are but two or three absolute contagionists, while almost all the others admit that the disease is occasionally propagated in this manner, though they deny that contagion is the sole means, or even the most frequent one, of its extension. He (Kierulf) formerly was a decided opponent of the doctrine of contagion, but his experience during the epidemic in and around Bergen induced him to adopt the contrary opinion.

"From the isolation of the dwellings, and the difficulty of communication on the western coast of this country (Bergen district), it follows that the cases of cholera were more widely spread, and it was more easy to trace the propagation of the disease from hamlet to hamlet. As an example of this, I shall take the district of Fane parish, to which I was attached as a cholera physician during the greater part of the epidemic. This parish lies partly inland and partly on the sea coast, a few miles from Bergen; the nearest part being about three miles and a half, the most distant about fourteen miles from that town. In this district there occurred twenty-three cases of cholera, and in regard to each of these I was able distinctly to ascertain from whence each individual had received the infection, nor was there a single instance among these in which it was necessary to refer to the obscure theory of a miasm to explain its origin.

"Cholera broke out in Bergen on the 11th of December, 1848, and the last case was reported on the 10th of April, 1849. The disease showed itself late in January among the fishermen who had been in Bergen, and from thence had gone to the fishing stations south of that town, and from thence, and from Bergen, all the inhabitants of Fane parish seemed to have received the infection.

1. Andreas Isachsen, Haaland, aged twenty-two, had been in Bergen on the 23rd of December, and had slept in a house there where that day no one was sick, but wherein on the following day two fatal cases of cholera occurred. He took cholera on the 25th, and died on the 28th. 2. Ole Larsen Oppedal, of Hope, aged twenty, took ill on the 30th of January, and recovered by the 12th of

February. He had been in Bergen two days before he was attacked. On the 6th—10th of February, three of the inmates of the farm-house at Hope were attacked with cholerine. 3. Andreas Larsen, Röe, aged thirty-six, took ill at the fishing-station on the 5th of February, was brought home on the 9th, and died the same day. He was buried on the 13th, and of those who attended his funeral, several persons, who lived at two farm-houses a considerable distance from Röe, were affected on the 19th with cholerine. 4. The widow of the last-named patient, Dorthe Hansdatter, aged thirty-five, took cholera on the 13th of February, and recovered on the 18th. Their children escaped the disease. 5. Malene Thomasdatter, Bredvig, aged twenty-seven, had been at the burial of No. 3 at Röe, on the 13th of February, and took ill of cholera on the 14th, and recovered by the 23rd of that month. 6. On the night of the 24th, her husband, Lars Andersen, Bredvig, aged thirty-five, occupied the same bed with his convalescent wife. During her illness he had slept with the children in another bed in the same room. He took ill on the 24th, and died on the 26th, of February. 7. Elling Nielson, Krogeide, aged fifty-seven, had slept at Bredvig, and had nursed No. 6 in that house on the 25th and 26th of February. He took ill on the morning of the 27th, and died the same afternoon. 8. His wife, Kari Johnsdatter, aged fifty-two, took ill on the 2nd of March, and recovered on the 9th. On the 1st of March their three children took diarrhœa, as did also No. 9, the sister of Elling Nielsen, Martha Nielsdatter, aged sixty-two, who lived in a house close to her brother's, and had visited him during his sickness. In this patient cholera was fully developed on the 5th of March, and she died on the 9th. 10. Ole Pedersen, Espeland, aged sixty-six, was brought home dead from the fishery on the 15th of February, having taken ill there on the 12th. He was buried on the 20th, and the funeral banquet was attended by both men and women (*'der blev holdt Gravöl af Mænd og Qvinder'*). 11. Lars Olsen, Espeland, aged thirty-eight, had accompanied his father's corpse from the fishery, and had attended him while sick. He took ill on the 16th of February, and recovered on the 28th. 12. Malene Andersdatter, Espeland, aged fifty-six, who lived in the next room to Lars (11), and had attended upon him, took ill on the 19th of February, and died in twelve hours. 13. Christiane Myntevig, aged sixty, had attended the *Wake* (Gravöllet) at Espeland, on the 20th; she took ill that day, and died on the 22nd of February. 14. Frederik Arnesen, Espeland, aged twenty, came home ill with Nos. 10 and 11, on the 16th of February, but cholera did not develop itself completely till after the 20th, and he died on the 7th of March. 15. Johannes Olsen, Sövig, aged fifty-two, was in Bergen on the 14th of February, and died on the 16th, after six hours' illness. 16. Johannes Michelsen, Bratland, aged thirty, had been in Bergen on the 24th of February, and had been in a house where cholera then prevailed. He took ill on the 25th, in the afternoon, and recovered by the 3rd of March. 17. Lars Larsen, Hamremyren, aged one year, was attacked with cholera on the 24th of February, and recovered by the 10th of March. The servant at Hamremyren, Malene Sjursdatter, aged thirty-six, was several times in the week at Bergen to sell milk; she was in that town, for instance, on the 17th, 19th, and the 22nd of February, and lodged there in a house where cholera prevailed, and wherein, on one of these days, an aged milk-woman died of cholera. On the 22nd, Malene herself took ill in Bergen, and was taken to the Lazaretto in Christi Krybbe. Lars Larsen, her master, father of No. 17, and twenty years of age, took cholerine on the 18th, but recovered by the 26th of February. 18. Jane Monsdatter, aged thirty-six, mother to No. 17, took cholera on the 27th of February, and died on the 3rd of March. 19. Her father, Mons Pedersen, aged sixty-six, who lived with his son-in-law, took ill on the 28th of February, and died on the 2nd of March. His wife escaped the disease. 20. Jacob Aadland, aged forty, was taken ill on the 27th of February, after having called at Espeland two days before on his way home from the fishery. He died on the 28th. 21. Kari Larsdatter, aged forty-seven, took ill on the 28th of February, and died on the 1st of March. Her husband had returned a few days before from the fishery, in company with No. 20,

but was not himself affected. 22. Lars Olai Larsen, of Söreide, aged thirteen, took cholera on the 3rd of March, and recovered on the 13th. His foster father, Tollev Nielsen, aged fifty-eight, belonged to the same fishing station as the men from Espeland, Aadland, and Hope, and had returned from thence on the 23rd of February, suffering from choleraic dysentery, which abated eight days after, but was not finally cured till the 12th of March. 23. The sister of the last-named, Martha Nielsdatter, aged 60, who inhabited the same room, took cholera on the 4th of March, and died on the 8th." (p. 579).

Kierulf observes that these facts speak for themselves; they show that cholera may be propagated by contagion, and that in general from one to four days elapse from the supposed period of infection to the outbreak of the disease. Most frequently the disease appeared on the second day after exposure to infection. It is probable, too, he remarks, that persons affected only with the choleraic diarrhœa can infect others with true cholera. Of this we have no doubt whatever, for we have always considered the diarrhœa so frequent during the invasion of cholera, to be a part and parcel of the disease itself. Kierulf believes, too, that even those who escape the disease themselves, may convey it in their clothes to others. The report of the Royal College of Physicians seems to favour this opinion; and in 1853, we met with one or two cases where this seemed certainly to be the case. We have reason to think that scarlet fever is conveyed in this manner, and we see no cause why this mode of transport should be denied. An example of this kind is given in Kierulf's report.

"Sjur Nielsen, Soltvedt, of Hammer parish, aged forty, was engaged at the fishery, on board of a yacht, which for a time lay moored to a small island on which several cholera bodies lay unburied. A few days after, four men took ill on board of the yacht, two of whom died on the 11th and 12th of February; the other two were on that day brought on shore. No more cases of cholera appeared on board the vessel. On the 20th, Sjur Nielsen came home, and as he was afraid of communicating infection to his family, he changed his clothes before he went into his dwelling. On the following day he went to Bergen, from which place he returned in the afternoon of the 26th February. But already on the morning of the 26th, his wife, and before mid-day, his eldest daughter and youngest son, were attacked with cholera, and the latter died the next morning; while his eldest son took cholerine on the 28th. His family lived in an isolated house, far from any neighbours, and had had no communication with any other persons. There was then no cholera in the vicinity, and indeed hardly any in the whole district." (p. 580.)

Kierulf witnessed several instances where the malady seemed to have been communicated through the medium of clothes, bed-coverings, and the straw on which cholera patients had lain. One singular case occurred where five persons in one house partook of milk which had been brought from a dwelling where cholera prevailed. The milk in question had stood on a shelf over a bed wherein a cholera patient lay. Those persons in the same house who did not partake of the milk, escaped the disease. The fishing stations around Bergen are mostly placed on dry and sterile rocks, and therefore are not placed in conditions supposed to be particularly favourable to the development of cholera.

We now turn to the two volumes of reports alluded to at the commencement of this notice.

The Report of 1850 is in the modest form of a pamphlet of about 130

pages. Appended to this is a map of the town of Christiania, showing the extension of the disease in that capital. We find in this little volume most carefully-arranged details of the progress of cholera in Christiania, in two reports drawn up by C. L. R. Horbye, and by the well-known Professor Frederick Holst, of Christiania. We would gladly have extracted at some length from these reports, but after a careful perusal of their contents, we do not find that they afford any new lights on the symptoms or on the treatment of the disease. Here, as in all other instances, the earliest cases were generally the most intractable; and it was only when the virulence of the malady was subsiding, that it appeared to become more amenable to treatment. More attention has evidently been paid by our Norwegian brethren to the post-mortem appearances in cholera than has generally been done in this country; but we must confess that only negative results have been obtained.

Following these two able reports, which occupy about 80 pages out of the 130, we have an interesting series of remarks by Professor Faye, of Christiania, on the mode of propagation of cholera, as illustrated by the epidemic of 1850 in Christiania. Professor Faye discusses the whole subject with much fairness and candour; he acknowledges that under certain circumstances the disease exhibits contagious properties, but he is not prepared to admit of contagion as the sole, or even a frequent, means of spreading the disease.

Cholera first visited Christiania in 1833, and extended from thence towards Sweden, along the eastern coast of the Christiania Fiord. The disease seems never to have penetrated into the country, but to have been confined almost entirely to the towns along the coast of Norway. The roads and the great lines of communication throughout the interior are very few, yet in each sequestered dale, rarely visited even by the exploring tourist, hamlets and isolated dwellings are to be met with. These almost entirely escaped, while cholera continued to follow along the sea coast, where active communications are constantly kept up both by sea and land. If the disease were conveyed by atmospheric or telluric influences, we can hardly understand how the inhabitants of the interior should have so uniformly escaped, and how the miasm should have crept along the shores alone. True it is that in the larger seaport towns the sanitary condition of the lower classes is here, as in other countries, eminently favourable to the development of disease; but in the cottages of the Norwegian peasants of the interior, a lamentable want of cleanliness and neglect of ventilation are but too apparent to the traveller.

In 1850, cholera broke out at Christiania in the night of the 4th of October, in the suburb called Grönland. During the months of July, August, and September of that year, diarrhœa, cramps, and vomiting had been frequent; but as such complaints are of annual recurrence at that season of the year, they did not excite especial notice. At this time, cholera prevailed at Götheborg (Gottenburg), in Sweden, and southwards, along the coast of that country, while many of the Baltic towns on the Prussian side and elsewhere, as Lubeck, &c., were affected with the disease.

The first patient was a tinman, residing in the suburb of Grönland, a man of somewhat dissipated habits, but who had not, as far as is known,

been in communication with any suspected persons or localities. Cholera was still far distant, it had not approached nearer than Gottenburg, and some of the islands about the mouth of the Götha Elv (Gotha river). It had existed in Malmö, a town at the south-western point of Sweden, since the middle of August. No interruption, however, seems to have been offered to the steam-boat communication with the infected ports, beyond a short quarantine of five days; but as this included the period occupied by the voyage, it proved no great hindrance to traffic. The epidemic was confined to the suburbs of Christiania on this occasion; only three cases occurred in the town itself, and these three were in the general hospital. The duration of the disease was eight weeks—viz., from the 4th of October to the 28th of November, during which time there were 157 cases and 87 deaths. The population of the city and suburbs in 1845 was 31,703. Not only was the disease confined to the suburbs, but to a few streets in those districts; while other streets, equally objectionable in a sanitary point of view, and in the immediate vicinity of those affected, entirely escaped. Not less than 138 cases out of 157 occurred in some low-lying streets and houses near the Aker river, which streets were wretchedly paved, and were traversed by a small brook, which was the receptacle for all the refuse thrown from the houses. The dwellings here are small, and rather deserving of the name of huts than of houses; their floors are of earth, and are frequently below the level of the streets, while the inhabitants are plunged in misery and vice. It is obvious that such a locality as is above described was particularly well fitted for receiving the poison of cholera in whatever way it might be introduced. The second house that was attacked (*Skjaegger-udgaard*) contained in all nine families, but the disease was exclusively confined to two of these families, thirteen persons in all, who inhabited a single room containing only 1100 cubic feet of air! All these individuals were sunk in the deepest poverty and filth, sleeping on straw and shavings, and several of them were habitual spirit drinkers. The cellar beneath this room had been uninhabited since the preceding winter, and was several feet deep in water. On the 5th of October, the day after the first case of cholera had occurred in the neighbourhood, the owner of the property began to pump out this water, and he continued to do so on the 7th and 8th. The water thus raised from the cellar produced an intolerable stench. When the cellar was emptied, dead cats, mice, and rats, with a portion of the skeleton of a horse, were found on the slimy floor. On the second day of the operation of pumping, the first case of cholera occurred in this house, and in a few days two men, one woman, and four children were attacked, all of whom died. Another family, consisting of a man, his wife, and two children, now entered on the occupancy of the vacated apartment, but were speedily attacked by the disease, which carried off both parents and one of the children. Not one of the other seven families in this tenement was attacked; the malady was confined to this chamber alone; and out of fifteen individuals who inhabited this chamber, ten died. It is difficult, by any theory of an epidemic miasm, aerial or telluric, to explain this remarkable circumstance. Yet for the contagionist a solution of the enigma is provided by the report of C. S. R. Horbye, given at page 62 of this volume. One of the occupants of the above-

named chamber was employed in the house of the first case (the tinman's), on or after the 5th of October, and on the 7th she was seized in her own room with the disease; and from thence it spread to the other occupants of that room. The occupants of the other rooms on the same floor escaped, though, as Mr. Horbye assures us, the sanitary condition of their dwellings and of their persons was not superior to that of the individuals who succumbed to the disease. We confess that this case appears to us strongly to favour the idea of a material contagion confined to one particular apartment. We saw many instances of a similar character during the epidemic in Newcastle in 1853.

In 1850 the third and the sixth week of the epidemic afforded the greatest number of cases.

Professor Holst, after quoting at some length Dr. Sutherland's well-known assertion, that all epidemics would cease to ravage our towns if due sanitary regulations were enforced, proceeds to observe on the improvements effected in the sanitary condition of Christiania between the years 1833 and 1850. Alluding to the mildness of the epidemic in the last-named year, he thinks there is good reason for supposing that the improved sanitary condition of Christiania in 1850, as compared with its state in 1833, enabled the inhabitants to resist more successfully the influence of the epidemic. How rudely must the worthy professor's feelings have been shocked by the ravages of the disease only three years after this assertion was made. Either Professor Holst was in error on the sanitary improvements of Christiania in 1850, or that city had wofully retrograded in this respect by the summer of 1853. Most towns have improved; very few have deteriorated in respect to provisions for cleanliness during late years; but the ravages of disease have not been the less severe. On the contrary, in some places where the population has rapidly augmented they have been more appalling than heretofore; and perhaps it is to the overcrowding rather than to the deficient drainage of these cities that we must ascribe the increase of disease.

The preparations made to meet the epidemic when it was approaching Norway appear to have been ample, and the arrangements for medical attendance on the sick most effective. Our far-seeing northern brethren did not rest quiet till the disease was actually upon them, but made their dispositions beforehand. Daily house-to-house visitation was put in force immediately, and dispensaries and cholera-stations were established; while houses of refuge to receive the families of those who had died of cholera were got ready in the more elevated and healthy parts of the suburbs. The advantage of the prompt removal of the families of those affected, was well illustrated by the results of such a proceeding in the town of Stavanger, on the west coast of Norway. This town contains about ten thousand inhabitants; and when the cholera broke out in any dwelling, the inmates were immediately removed from the room or rooms in which the disease occurred, and these apartments were allowed to stand empty for several weeks, and were thoroughly ventilated and cleansed before any person was allowed to occupy them again. Although the epidemic at Stavanger lasted for not less than fifteen weeks—viz., from the 14th of February, 1850, to the 1st of June, only 90 cases occurred during all this period, and 42 died. We have seen houses shut up after several

persons had died in them from cholera, and yet, after remaining closed for weeks, the disease reappeared among those who entered upon the occupancy of them. It is a mistake, we think, to close any such apartment; they should be freely left open to allow of the circulation of air through them, and thus the fomites of the disease may be eliminated by dilution with atmospheric air.

The pay of the medical attendants on the sick seems to have been arranged in Christiania on the most liberal scale, and forms a marked contrast with the niggardly remuneration which reluctant guardians and corporations have accorded in this country to those who risked their lives and devoted all their energies and time to combating the pestilence. A sudden invasion of the premonitory diarrhœa took place in the general hospital of the city in the night of the 23rd-24th of October. To this hospital the first cholera patients from Grönland had been brought about ten days before; but no communication between the affected persons and the wards for skin-diseases and surgical cases, wherein this sudden outbreak took place, could be traced. Not less than fifty patients were attacked with diarrhœa and cholerine in the course of that single night. Speedy succour was given, and the malady did not spread further. Only one cholera physician was attacked during the epidemic; but of the nurses, porters, &c., attached to the cholera hospital, not less than eleven were seized with well-developed cholera, and five died. It is probable that these poor people were rendered more susceptible of the disease by the want of proper nutriment, as at first they were permitted to provide their own food, and this was often insufficient, from motives of mistaken economy on their part. The system was immediately changed, and good and nourishing diet provided for the servants of the hospital, and with the most satisfactory results. With the exception of the cholera physician above alluded to, not a single case occurred in those above the rank of artisans. The relative mortality of the sexes was nearly equal—forty-seven males to forty females. The mortality in the cholera hospitals was greater than among those who were treated in their own houses.

The latter part of the week was observed to be marked by the highest mortality. With a view to the more decorous observance of the Sunday, the day for paying wages had been changed from the Saturday to the Wednesday, and the increased consumption of ardent spirits consequent on this arrangement perhaps augmented the severity of the cases that occurred during the latter half of the week. The weather during the prevalence of the epidemic presented no remarkable features, but the "magnetic intensity" was found to be remarkably less than the average of ordinary years.

Professor Faye's report 'On the Origin and Mode of Propagation of Cholera,' is a document of much interest (pp. 81—101). He maintains that the disease was of spontaneous origin, because, in 1850, the introduction of the malady could not be traced. His idea, that the diarrhœa which so constantly precedes cholera is a separate disease, is, we think, a common but a most erroneous doctrine. We regard that diarrhœa as a part of the disease itself, as the direct effect of the cholera poison; and we find, both in this volume and elsewhere, many instances tending to prove that persons labouring under this diarrhœa alone, and without any other

symptom of cholera, may convey to other persons true cholera, which may develope itself in these individuals in the most virulent form. But while Professor Faye writes himself down a non-contagionist, he honestly and boldly confesses that the cholera poison may so infect the air of a room, as to produce the disease in persons who breathe this air. He is not, however, inclined to admit fully the opinion that the fomites of the disease may attach themselves to the clothes of persons in good health, and may thus be conveyed to other individuals at a distance. We certainly have met with instances of this kind; and the facts related by Kierulf before noticed, and the opinions of the reporters of the Royal College of Physicians of London, are favourable to such a doctrine. This indeed seems to be the main point at issue between the strict contagionists and those who, like Professor Faye, admit of contagion in a modified degree; and its bearing on the subject of quarantine must be admitted to be of importance. Professor Faye is a stanch opponent of all quarantine, and he denied that it ever succeeded in keeping out the disease. We hold, on the contrary, that if a quarantine could be absolute and complete, such as has at times been put in practice in small islands, &c., the disease will not be introduced, especially where these islands are separated from the mainland by a certain space of water sufficient to ensure absolute cessation of all intercourse. But in all populous countries, it is perfectly impossible to establish such a quarantine; while at the same time, the misery inflicted on the labouring population of sea-port towns would infallibly depreciate the standard of health among the inhabitants, and predispose them to the malady which, sooner or later, might be introduced among them. The injury to trade and to the national prosperity thus induced would, even supposing that the cholera could thus be effectually barred out, render our labouring classes incapable of resisting the inroads of other epidemic diseases; and fever, diarrhœa, &c., the results of diminished allowance of food, would probably destroy as many as the disorder, thus sought to be excluded, would have carried off: we therefore fully coincide with our Norwegian brethren in urging the removal of all quarantine restrictions, except perhaps in the case of vessels arriving with cholera actually on board, when the patients might be removed to a suitable locality, and debarred for a time from all intercourse with the yet healthy town.

The visitation of cholera in Norway, in 1853, was far more severe than that of 1850. The malady, in its most malignant form, had broke out in Copenhagen, in the month of June of that year; while at that time no other European city but St. Petersburg was suffering from the scourge. Copenhagen had hitherto almost entirely escaped; but the ravages of cholera in that capital were on this occasion most fearful. We have not as yet obtained the detailed reports of the progress of the cholera in Copenhagen and in Denmark, but hope at a future period to lay these before our readers. The appearance of cholera in the Danish capital had awakened the attention of the Norwegian authorities, and every preparation was made to meet the approach of the enemy. On the 25th of July, the first cases appeared in the general hospital and in the town, but for three weeks the disorder made but little progress, so little indeed, that during the first twenty-one days, only 26 cases were reported. By the end of the month of August, the epidemic was at its height, and the daily

mortality was from 90 to 100, while at the same time the malady spread to the adjoining country. On the 14th of December, the town of Christiania was declared free from cholera. The epidemic thus lasted for the unusually long period of sixteen weeks; the number of cases was 2453, or nearly 5 per cent. of the whole population of the town, while the deaths amounted to 1597, or 65 per cent. of those attacked. Though perfectly identical in its symptoms, the epidemic of 1853 presented many deviations from its general course. The districts of the city and of the suburbs which had escaped in 1850, were in many instances now severely visited. In 1833, the first time that Christiania suffered from cholera, the number of cases was 1430; and in 1850, when only a part of the suburbs was infected, the cases were only 153. The suburb of Oslo, which lies high above the river, and is inhabited by a superior class of the population, was not affected in 1850; but in 1853, 96 cases occurred here in a population of about 1000. The filthy streets called the Toiengade and Nordbygade, which lie close to the affected locality of Grönland, escaped altogether in 1850, and suffered but little in the severe epidemic of 1853. In 1850, with the exception of one cholera-physician, all those attacked were of the class of artisans, or else the dregs of the population; while in 1853, the disease raged among the higher classes also; and among these the relative mortality in proportion to those attacked, was actually greater than among the labouring population, being not less than 75 per cent., or 11 per cent. above the average mortality of the whole. Still more singular is it to find that the mortality among the lowest and poorest classes was positively below the general average; for it only amounted to 64 per cent. The deaths in the cholera hospitals were 67 per cent.; the mortality among those treated at their own houses was 62 per cent., or 4 per cent. less than the former. In 1833, the mortality in the cholera hospitals was 6 per cent. in excess of those treated at home; and in 1850, it was not less than 15 per cent. greater, though the disease, as before stated, was then exclusively confined to the most wretched part of the suburbs. Of the attendants on the cholera hospitals, 15 were attacked, and 10 died; but of the 80 medical men engaged in the town, only one was attacked, and he had previously been in a weak state of health, and soon succumbed to the disorder.

The volume before us contains a general report by Professor Conradi, On the Progress of Cholera in Christiania and its Neighbourhood in 1853, followed by very able special reports from each of the five cholera hospitals established in Christiania. The greater part of the rest of the book is occupied by a most excellent general report On Cholera in Norway, by Professor Faye. We deeply regret that the limits to which this notice has already extended, forbid our entering into any detailed analysis of this masterly essay. We do not agree with some of the doctrines here enunciated; but throughout the whole we observe a spirit of candour and of fair inquiry, which impresses us with the truth of the facts at least on which he bases his opinions. In this essay, Professor Faye seems to us to admit more fully the importance of the accumulated facts respecting the propagation of the disease by contagion than he has heretofore done. Indeed, the number of instances here reported of cholera having been first brought into various districts by individuals returning from infected

parishes and towns, and especially from Christiania, where the disease was most severe, is so great, that it is impossible, we think, to deny the fact that this report greatly strengthens the position taken by the contagionists. We have not room here to quote the examples alluded to, but we may briefly state that, out of fifty-eight parishes and towns, the disease is reported as having been introduced by individuals in not less than forty-eight. The malady spread from town to town along the coast, where the densest population and the most constant intercourse is kept up, while its progress into the interior of the country was but of short duration, and the cases few. The disease broke out repeatedly in some towns, apparently from fresh importations from the head-quarters of the disorder in Christiania. Many of those who thus carried the malady back to their homes had not, when in Christiania, visited any cholera patients; it seemed, says Professor Faye, as if a very short stay in the atmosphere of the infected town was sufficient to imbibe the seed of the malady; and such individuals were often first affected with cholera when they had regained their homes. In each of these cases, one or more persons who inhabited the same room—often the wife or other relatives of the traveller—fell victims to the imported malady; while those who waited on the sick occasionally carried the disease still further, and spread it in their own homes. In all this, we own we cannot see anything like an epidemic constitution. Influenza traverses whole country districts, and affects in a single night hundreds living in isolated spots: it does not follow the main routes of communication, nor does it show any tendency to die out in country-places in comparison to its activity in the towns. No country, as we observed at the beginning of this notice, is better adapted for careful observation of isolated cases than Norway; and yet it has been precisely in Bergenstift, on the western coast, where the isolation of the farm-houses is the most complete, that the strongest facts in favour of the contagion of cholera have been collected under the actual observation of intelligent medical practitioners. In addition to those noticed before, we find in the discussions on the mode of propagation of cholera in the meetings of the Medical Society of Christiania, a vast array of most important facts, brought forward by Kierulf, Professor W. Boeck, and others, which to us carry the completest conviction, that here at least, amid the islands of the western coast of Norway, no epidemic influence was at work, but that the disease was spread by contagion, and by that means *alone*. In great towns, when once cholera has obtained a footing, the air respired by so many individuals labouring under the disease becomes, as it were, one vast focus of infection, especially when, as was so markedly the case in Christiania in 1853, a perfect calm continued in the atmosphere for weeks together. Many facts collected of late years have tended to prove that the period of incubation of cholera may possibly be much longer than that of five to eight days, the period which has been generally assigned to it. If quarantine is, therefore, to be maintained at all, it must be persevered in for an almost indefinite period; and the absurdity of any such regulation will be apparent to all.

We cannot leave these volumes without expressing our congratulations to our Norwegian brethren on the completeness and scientific tone of the reports they contain. Small and unpretending as these two volumes are in

comparison to our portentous Blue Books, they yet afford a mine of information on the important subject they treat of, which will tend to raise still higher in the esteem of the medical world the already well-known diligence and scientific attainments of the medical practitioners of Norway.

Edward Charlton.

Review VIII.

1. *Report of the Commission of Inquiry into the Supplies of the British Army in the Crimea, with the Evidence annexed.* Presented to both Houses of Parliament by command of Her Majesty.—1856. Folio, pp. 432.
2. *Appendix to Ditto.* Folio, pp. 196.

When the Report of the Select Committee on the Army before Sebastopol, and that upon the State of the Hospitals of the British Army in the Crimea and Scutari, were published, we gave our readers a brief and general sketch of the movements of the army of the East from the commencement of the war to the close of the disastrous winter of 1854–5, and analysed at some length those portions of the Reports having reference to the causes of that frightful mortality which the army had experienced, to the conduct of the medical officers, and to the general system of management of the department.

Simultaneous with the appointment of a Select Committee by the House of Commons, Government nominated a Commission to proceed to the Crimea, and on the spot to investigate the whole question of the arrangement and management of the commissariat department; the causes of the irregularity and insufficiency of the supplies furnished to the army, with the sources of supply of provisions, forage, and other articles; and to inquire into the alleged delay in unshipping and distributing the clothing and other stores for the use of the troops. The commissioners appointed to conduct this important inquiry were Sir John McNeill and Colonel Tulloch, and it would have been difficult to have found two gentlemen who, from their antecedents, were better qualified for so delicate and arduous an undertaking. Sir John McNeill had commenced his public career as a medical officer in the H.E.I. Company's service, was afterwards employed by them in a diplomatic capacity, and had ultimately been appointed British ambassador to the Court of Persia. Colonel Tulloch, after having served with his regiment in the first Burmese war, had been employed at home, first in the preparation of the 'Army Statistical Reports,' which have been so often noticed in our pages, and in carrying out the sanitary measures founded upon them; and afterwards in organizing and superintending the military pensioners throughout the kingdom—a body amounting to about 15,000 armed men, and probably four times as many not enrolled and armed. It is not our intention to enter upon the question how these Commissioners discharged their duty, to record their subsequent treatment by the Government which appointed them, or to animadvert upon the proceedings before the Board of General Officers at Chelsea. These topics have been of too general public interest to require notice here, and would be out of

place in the pages of a professional journal; but we purpose to inquire how far the Report of the Commissioners corroborates our former conclusions as to the causes of sickness and mortality among the troops, and to notice a few points which were not brought out in the first investigation.

In the course of their inquiry, the commissioners examined the surgeons, or, in their absence, the assistant-surgeons in charge, of every corps serving in the Crimea, as well as the principal medical officers attached to brigades or divisions, and Dr. Hall himself. In addition to the personal examination, written queries were addressed to the surgeon of each corps, the answers to which are contained in the Report. From these documents, together with the returns furnished by the medical officers, Colonel Tulloch drew up a statistical report, showing the extent of sickness and mortality in the various brigades, with the diseases by which these were occasioned; but Lord Panmure decided that such an inquiry was not comprised in the instructions of the commissioners, and directed that it should be omitted, as it would more appropriately come within the scope of those Reports on the health of the army which Colonel Tulloch has been in the habit of preparing for Parliament. The volumes before us, therefore, do not include the interesting details as to the diseases by which the sickness and mortality were caused, nor do they afford any information as to the relative health of the various arms of the service, or of the troops employed on different duties, and occupying different positions during the progress of that protracted siege. We trust that the mass of information on these points, collected with so much trouble by the commissioners, may be hereafter made available to the profession.

The inquiries of the commissioners were confined, in accordance with their instructions, to the state of the army in the Crimea, and had no reference to the previous campaign in Bulgaria. The general mortality of the army by disease during the seven months from the 1st of October, 1854, to the 30th of April, 1855, including also the deaths at Scutari and in the general hospitals, amounted to 35 per cent. of the strength, and fell most heavily upon the infantry corps stationed in the front, which were furthest from their supplies, and had the most severe and never-ceasing duties to perform. The extent of sickness in some of the regiments may be estimated from a fact stated by Staff-Surgeon Young, that a few days after he joined the 63rd regiment (12th January, 1855), "there was not a man for duty, all were sick" (p. 44).

The causes which gave rise to the enormous mortality of 35 per cent. by disease in seven months, may be briefly summed up as improper food, no means of cooking it, insufficient clothing, no adequate shelter from the inclemency of the weather, want of fuel, excessive duty of a most severe and harassing description (including the bringing up of supplies from Balaklava, and digging roots for fuel), want of medicines and medical comforts when sick, and the necessity of treating disease under circumstances which almost precluded the chance of success.

"No effort," says one of the medical officers, "appeared to be made to economise the health of the force. . . . . A little forethought, prudent consideration, and timely preparation, would have obviated the loss of thousands of valuable men during November, December, and January. In the months specified, soldiers were removed wholesale from the scene of usefulness by diseases which may well

be classed under the designation *preventible*, because they would not have occurred to so fearful an extent under favourable modes of living."

The surgeon of the Grenadier Guards attributes the very great difference in the proportion of disease between officers and men chiefly to the circumstance that

"The officers could command a proper diet, had their beds raised from the ground, had changes of clothing, endured less fatigue, and had no manual labour to perform. The officers were in the same description of tents as the men, and had the same exposure to weather and to the influences of climate, but they have been on the whole more healthy in the Crimea than they were in England."

The diet of the men from the period of landing in the Crimea till the beginning of the following February, consisted almost entirely of salt beef or pork, biscuit, green coffee, and rum. The natural consequence was the appearance of scurvy. The men became disgusted with the salt meat, and frequently had no fuel to cook it with; and as there were no means of roasting the green coffee, it was useless, or if attempted to be made use of, injurious. The men were consequently in many instances reduced to live upon the biscuit and rum. On one or two occasions, cargoes of vegetables were brought into Balaklava, but owing to want of transport, the distribution benefited only the corps in its immediate vicinity. Not only was the food of an improper description, but as fuel was not sent up to the front by the commissariat, the men had to procure it for themselves. This could only be done by cutting down timber or brushwood, or by grubbing up roots,—a labour which involved great additional fatigue to the men, already overworked in the trenches and in carrying up stores, while it also deprived them of a portion of their too limited period for repose. The result was, that they frequently would not make the effort to procure it, and either threw away their meat, or in some instances were seen to eat it raw. They are also stated to have suffered greatly from the defective cooking arrangements, as well as the want of fuel and cooking utensils.

"Each man," says the surgeon of the 49th, "cooked his own rations the best way he could, and if a man felt unable to do it, he went without his food."

And the surgeon of the 18th states,—

"One of the assistant-surgeons now present with the regiment tells me that he was five consecutive days without tasting animal food, because he had no fuel with which to cook it. During that time he lived entirely on biscuit and rum." (p. 371.)

The arrangements appear at first to have been equally defective in all the regiments, but by degrees—in some earlier, in others later—men were told off as permanent cooks, who were exempted from all duty in the trenches, a system which seems to have been attended with very beneficial results. The assistant-surgeon in charge of the E field battery observes:

"In the siege-train camp, where many companies were collected together, it was very striking how much healthier those companies were whose commanding officers devoted their first and most strenuous efforts towards building a commodious and well-covered cook-house, from which all the men of the company were supplied."

Might not officers take a valuable hint from this remark as to the importance of paying more attention to the dieting of soldiers, even in the time of peace?

The commissioners have discussed at some length the subject of the soldier's diet, and judiciously observe that—

"The results of this inquiry having clearly demonstrated how indispensable it is to the soldier's efficiency in the field that he shall be supplied with a sufficient quantity of wholesome food, it cannot be desirable that a matter upon which the success of the whole army depends should be left to chance, or to the prudence, activity, and ingenuity of the soldiers individually, even in countries where supplies can be obtained." (p. 47.)

They have accordingly suggested a "standard ration" for troops in the field, and have pointed out certain equivalents which may be substituted for some of the articles whenever economy of transport becomes important. Their observations on this subject are excellent, and derive additional weight from the circumstance that they were submitted to Dr. Christison, who fully concurred in them, and whose detailed observations are published in the appendix to the Report.

During the early part of the winter, the men were very insufficiently clothed, many of them having only one blanket, while their body-clothes were tattered and worn out; and it was not till the month of January that anything like an adequate supply of warm clothing was distributed.

"During the month of December, the severity of the winter had much increased, and the medical officers describe the sufferings of the troops for want of proper bedding, warm covering, and clothing, as very serious." (p. 25.)

The men suffered also very much from want of boots and shoes, those which were sent out from England having been generally found too small, owing to the feet being much swollen from the effects of cold and other causes; the men rarely ventured to take off their boots, lest they should be unable to get them on again. But the men thus badly fed and clothed were still worse housed, their only shelter from the inclemency of the weather being bell-tents, many of which were old, thin, and worn-out. These were not calculated to keep out either the cold or rain, and in wet weather the floor became a perfect mass of mud, in which the men were compelled to sleep without bed or even paillasse. As there was no fuel available, and no means of using it in a tent even if there had been, the only warmth in these wretched shelters must have arisen from the number of men packed in them, amounting to fifteen in each. After the winter was over, wooden huts were erected, and even at that time were of great use in improving the health of the men.

But another very great cause of sickness and mortality was the excessive amount of duty the men were required to perform. The detailed statements on this head, like those on the prevalence and mortality of the different classes of diseases, were deemed by the Minister for War not to come within the scope of the commissioners' instructions, and have consequently not been published, but enough is stated in the evidence of various medical officers to enable us to form some idea of its extent. Thus, the surgeon of the 4th Regiment says:—

"From want of proper transport, the men have had to do the work of mules, with this important difference, however, that the mules have had every night in bed, and the men have not. . . . . I have very frequently known men to be three nights out of bed—one in the trenches, and the two following on guard. I have known them to be four nights out of bed; and on the night of January 25th, we

sent down seventy men—at that time every available man we had—many of whom had been three nights, and some actually four nights, out of bed previously. Moreover, many of the number had, when the party marched off, not returned from Balaklava where they had been sent on fatigue. These men were forced, on their return, to eat their dinner and proceed to the trenches." (p. 323.)

The surgeon of the 17th Regiment, in the end of April, says,—

"Until very lately, the men of the regiment had barely, on an average, one night in bed for one on duty. . . . . There was no amusement, no relaxation, but day and night the same routine of hard work went on."

The assistant-surgeon of the 23rd observes that "the error consisted in attempting to make one man do the duty of three or four," and that, be it remembered, for a continuous period of four or five months, and exposed to the privations already noticed. The surgeon of the 22nd states that—

"From the commencement of the siege, the men had only one night's rest, going again on duty the next evening . . . . and one time, shortly previous to our allies occupying a portion of our trenches, the whole regiment was on duty three nights in succession,—all, of course, not being in the trenches, but on guard on various posts, &c."

Staff-Surgeon Alexander, in charge of the Light Division, has given details relative to the duty performed by the regiments composing it, which show that during part of the winter even the most fortunate corps had not above one night in camp for one on duty, while some had even less; for instance, in the right wing of the 2nd battalion Rifle Brigade, November, 1854, "one company was five nights out of six on duty; three companies seven out of nine." It is unnecessary to adduce any more instances to show the extent of duty during that fearful winter, but it must be borne in mind that in addition to all the regular duties, large fatigue parties were constantly sent down to Balaklava to carry up supplies of food upon their backs, and that, over roads which had knocked up the horse transport of the army! With this history before them, our readers will doubtless concur with Lord West, when he remarks that—

"After their privations from bad food, want of clothing, and excessive work out of all proportion, unheard of in its severity and continuance, the wonder is that a man is left alive to tell the tale."

The preceding remarks bear reference only to the men, we cannot say in health, but who, notwithstanding their physical powers being reduced by privation and exposure, and many of them actually labouring under serious disease, continued, with unparalleled courage, to discharge to the utmost of their strength the arduous duties required of them. But the unfortunate sick who succumbed under the pressure, and were obliged to enter the hospital tents, seem to have fared no better. Owing to the deficiency of marquees, they were for the most part treated in bell tents, of which the commissioners, after a true, though not very flattering description, say:—

"In short, whatever may be the supposed advantages that have led to its adoption as a barrack-tent, it would be difficult to contrive anything much more unfit for the accommodation of the sick." (p. 38.)

Their rations are stated by the surgeon in charge of the 63rd to have been generally only the same as were issued to the other troops; and the surgeon in charge of the 95th observes:—

"At one time the sick were without any other rations than salt meat, biscuit, and raw coffee." (p. 55.)

Even the medicines necessary for the treatment of the cases, and the medical comforts upon which so much of the success depends, were lamentably deficient, and in many instances not to be procured at all. As this is a point which was much contested before the Sebastopol Committee, it may not be amiss to quote the opinions of a few of the regimental surgeons, who are certainly the most unbiassed witnesses upon it. The assistant-surgeon of the 23rd says:—

"The hospital accommodation, medicines, and medical comforts, were miserably deficient, and numbers died in consequence."

The assistant-surgeon of the Coldstream Guards states:—

"As a general rule, I believe I may safely say that of the numerous requisitions made to the divisional stores for supplies of medicine absolutely required by the sick, very, very few indeed have been returned without the medicines applied for being very much curtailed in quantity, or erased altogether, with an accompanying remark, 'None in store.'"

The surgeon of the 4th Regiment says:—

"Until within about the last two months (his answers being dated 29th April), the quantity of medicines and medical comforts has been very limited. They have been doled out in infinitesimal proportions."

The surgeon of the 1st battalion Rifles remarks:—

"I understand from the assistant medical officers, that during the winter great difficulties existed in procuring medicines, the usual answer to indents being, 'None in store;' and that about Christmas the want of opium and Dover's powder was so felt, that for four or five consecutive days parties were despatched to the store at Balaklava, for the purpose of procuring some; but fruitlessly."

And to show that this involved no small amount of labour, he adds:—

"From the state of the roads and the country, it took these people from fourteen to sixteen hours to perform the transit thither and back again." (p. 312.)

The assistant-surgeon in charge of the 3rd Foot Guards states that—

"During the latter part of November, and all December, there was continual difficulty in obtaining the medical comforts required, as well as the medicines. Of the medicines that could not be obtained, were opium and Dover's powder, during the prevalence of diarrhœa and dysentery. Saline medicines, such as carbonate of soda and citric acid, were also deficient during the prevalence of fever. Port wine, arrow-root, and ground rice, were among the medical comforts that could not at that time be obtained when required." (p. 35.)

Deputy Inspector-General Alexander, in medical charge of the Light Division, states in still stronger language the condition of the army in respect to these necessary supplies. In his evidence, taken 15th May, 1855, he says:—

"The supplies of medicines and medical comforts have, generally speaking, not been in sufficient quantities, the requisitions for both being in many instances curtailed; in others, none were to be had; rarely, if ever, were the requisitions complied with in full, until latterly. Three pounds of arrow-root for the whole division were at one time issued, while cholera was raging; at another, of a requisition for sago, arrow-root, essence of beef, and caudles, the last only were sent. . . . As regards medicines, many important ones were at times only obtained in small quantities,—viz., of pulv. opii and calomel, only four ounces of each were got for a sick-list of 636 cases of cholera, dysentery, diarrhœa, fevers, &c., which was about one dose of three grains each of these two medicines to each

patient. Again, one ounce of pulv. opii and four ounces of blue pill were got for the whole division, consisting of nine regiments, besides marines and artillery, the sick-list being 619 of the diseases above mentioned." (p. 144.)

It is unnecessary to multiply these quotations; enough has been stated to show that there was but too good reason for complaint on the subject of want both of medicines and medical comforts, and the painful inference cannot be avoided, that many a gallant soldier fell a victim to disease who might have been saved had the medical officers been properly supplied with the means of treatment.

The question naturally suggests itself, who was to blame for these deficiencies? As Dr. Hall was at the head of the department in the East, the responsibility must rest with him, unless it can be shown that he was neglected by the authorities at home, that supplies could not be procured on the spot, or that he had used every exertion to remedy the deficiencies, but had failed from causes beyond his control. But such does not appear to have been the case. In his evidence before the commissioners, he has made no complaint of neglect at home. He attributes the deficiency, to a certain extent, to the loss of the *Prince;* alleges unnecessary delay in complying with his requisitions at Scutari; incomplete execution of them by the omission of some, and unauthorized reduction in the quantities of other, medicines; and the neglect of the apothecary at Scutari to notify to him the shipment, so that he was not aware of their having been sent. But Dr. Hall appears to have contented himself with writing to the principal medical officer, to admonish the apothecary, instead of taking immediate steps to replace him by an efficient officer. In his evidence he incidentally supplies us with a probable explanation of the irregularities as regards medicines:

"Periodical returns, showing the quantity of medicines received, issued, and remaining in store, are submitted to him by the apothecary at the quarterly periods; but not at any intermediate periods, unless called for. . . . Has not considered it necessary to call for intimation of any extraordinary demand for any particular medicine." (p. 154.)

To rest content with *quarterly* returns of medicines when in charge of an army situated as that in the Crimea was, the men dying by hundreds, and the numbers sick exceeding those who, though many of them were labouring under disease, continued to do their duty, argues an amount of apathy, or a peculiar constitution of mind, which cannot but excite astonishment. With regard to the possibility of procuring the various articles which were deficient, the assistant-surgeon of the Coldstream Guards states, that when unable to obtain brandy for his cholera cases from the divisional stores, he applied to his commanding officer, who sent to Balaklava and purchased abundance of it; and the surgeon of the 34th states that "Astringents, such as kino, catechu, acetate of lead, were not to be procured, although the medicine bazaar at 'Stamboul' was and is overstocked with these medicines." Surely such sources, open to individual officers, and taken advantage of by them, should not have been overlooked or neglected by the responsible head of the department. Dr. Hall, however, appears to have taken a different view, and in a letter to the chief apothecary, shows an amount of petulance ill becoming an officer of his standing, especially as he well knew the existing difficulties of the regimental medical officers, and the amount of hardship, suffering,

and toil they were then undergoing. On the 27th of November he writes:

"With the creta ppt. you can easily manufacture the hyd. c. cretâ, which is so much run on by some medical officers; and if they cannot obtain the exact article of medicine they require, they say they cannot do justice to their patients." (App. p. 167.)

The commissioners do not leave us in uncertainty as to their opinion of Dr. Hall. They remark,

"Though the correspondence no doubt shows that repeated applications were made by the inspector-general to the reserve depôt at Scutari for medicines and medical comforts, to meet the wants of the hospitals in the Crimea, we feel bound to express our opinion, that at a time when the existence of a great portion of the sick was imperilled by the absence of these supplies, something more than the mere transmission of the usual official demand on the purveyor or the apothecary at Scutari was necessary to relieve the inspector-general of his responsibility; and when he found the inattention to these applications, causing a delay of nearly two months in the arrival of supplies for which the demand was urgent, it appears to us that he ought to have taken some more decided steps to insure attention to his requisitions. A proper officer might have been sent to Scutari with instructions to bring back whatever was most urgently required for the hospitals." (p. 39.)

That the diseases which destroyed perhaps the finest army that ever left the shores of England were *preventible*, is sufficiently proved by the excellent health enjoyed by the troops during the following winter, and by the high state of efficiency of all arms of the service at the close of the campaign. Gratifying as its condition was then proved to be, and satisfactory as have been the results of the nation's exertions, it cannot but be a source of the deepest regret that the lives of thousands of our bravest troops have been sacrificed, and the reputation of our country tarnished, by ignorance and want of foresight at home, and of promptitude and ingenuity on the part of the military authorities and heads of departments of the army in the Crimea. We sincerely trust, that, should it ever again be our lot to engage in a great war, a greater amount of circumspection, prudence, and common sense, may be shown in the selection of the staff to whom the lives of our soldiers are to be entrusted, and by whom the honour of our country must be upheld.

Among the written questions sent to the medical officers was the following:—Have you anything further to state, or any suggestions to offer, with the view of improving the health of the troops? If so, state them. Most of the medical officers have taken advantage of this question to explain their views upon this important point. The suggestions are in many instances given at considerable length, and are well deserving the attention of the authorities. The most important of them may be thus summed up. Reduction in the amount of work; diminution of the weight to be carried by the soldier; an improvement in his clothing, and particularly the adoption of some waterproof covering, and the substitution of flannel for cotton shirts; an amelioration in the quality, nature, and cooking of the rations; the regular issue of vegetables, fresh, preserved, or compressed; the substitution of tea for coffee, and of porter for rum; the establishment of permanent cooks instructed in their duty; the erection of covered cooking places; the adoption of *tentes d'abri* in the field; attention to the ventilation of huts where these are erected; adequate means of ablution; the formation of a sanitary police

in the army; efficient transport for the sick; and a supply of good hospital orderlies and hospital sergeants. The last is a point of great importance, and until the time arrives when these can be furnished in sufficient numbers from the recently-organized medical staff corps, means should be adopted to give the medical officer a voice in their selection, and a greater control over them than at present. The surgeon of the 4th Light Dragoons observes:

"I would recommend that the orderlies be uniformly selected from the well-conducted and able-bodied men of the regiment, and permanently attached to the hospital, subject to removal for misconduct or at the express wish of the surgeon; too frequently when a man becomes unfit for all active duties, or is a dirty, slovenly, badly set-up soldier, he is considered good enough for an hospital servant, and consequently breaks down, either from bodily infirmities or vicious habits, as soon as his services are required most." (p. 418.)

The importance of a good hospital sergeant can be best appreciated by those who have had the misery to be afflicted with a bad one. We cannot afford space to enter fully upon the subject of the recommendations, the importance of which must be fully appreciated by all who have paid attention to the subject of military hygiène.

Since the period of our former notice on the Medical Service of the Army in the East, the Government have recommended to her Majesty to confer the Order of the Bath upon certain of the medical officers of that army.

We regret to learn from our correspondents that the selection which has been made has created a very strong feeling of dissatisfaction. For this, of course, Dr. Smith is responsible, though we presume he has acted upon the recommendations of Dr. Hall. The ground of complaint appears to be, that the order has been conferred rather in consequence of an officer having held a certain position than of his having earned it by his services; and that in some instances it has been bestowed upon officers who left the Crimea before the hardships of that dreadful winter commenced, while others who nobly remained at their posts throughout the whole war were passed over. The selection seems to have been made upon the same principle as among the officers termed "combatant," among whom a very large proportion of the honours were absorbed by the staff, while the hard-working and over-tasked regimental officers were forgotten, or deemed ineligible from their rank. This is much to be regretted, and we trust that on all future occasions the only principle of selection for rewards and honours will be "Palmam qui *meruit*, ferat."

We conclude our remarks with the following warm tribute of praise paid to the indefatigable exertions of the medical officers, by the commissioners:

"The circumstances in which the medical officers were placed, owing to the want of those supplies (of medicines), as well as of every description of diet, accommodation, or comfort, essential to the recovery of the sick, were the most painful that can be conceived. We prefer leaving it to be described in their own words, and shall confine ourselves to the expression of our conviction, that if they were unsuccessful in their treatment to an extent which has seldom occurred in the annals of medical practice, it has not been for want of constant attention and unceasing and energetic efforts on behalf of their patients. The appeals of some of them to those authorities whom they—perhaps erroneously—conceived had the power of assisting them, were most forcible and pathetic; and if all did not make such appeals, it was because they found that when made by others they produced no result." (p. 39.)

Review IX.

The Influence of Tropical Climates on European Constitutions, including Practical Observations on the Nature and Treatment of the Diseases of Europeans on their return from Tropical Climates. By James Ranald Martin, F.R.S. A New Edition.—*London*, 1856.

Many years have now elapsed since the late Dr. James Johnson produced his well-known work on the diseases of tropical climates. After six editions had been exhausted, the demand for it still continued, and Mr. Martin has taken upon himself the responsibility of again bringing it before the public. The present edition, however, is not a mere reprint of the last. Mr. Martin has re-cast and re-written the entire work, retaining a certain portion of Dr. Johnson's observations, but the greater part of it is exclusively his own composition. The present treatise may therefore be considered to a certain extent as a new and original work, and will justify a fuller analysis than we should otherwise feel called upon to afford.

That Mr. Martin has at least had full opportunities of acquiring a practical knowledge of the subject of which he treats we must probably allow, when we learn that he served in various parts of India—in peace and war—amongst natives and Europeans—in hospital and private practice—for two-and-twenty years; that he has turned these opportunities to full account, all those who are acquainted with the sanitary benefits for which the capital of British India is indebted to Mr. Martin's suggestions and exertions, will at once admit. The profession will, doubtless, therefore expect a book full of sound and valuable information, and we think they will not be disappointed.

The first part of the work commences with the consideration of the physiological influences of the climate of the East Indies on European constitutions. The physical climate of Bengal is sketched out, and some interesting information afforded with respect to the geological nature of the soil, which is noticed to be ferruginous in many parts which are notorious for the fatal character of their fevers. The modifying influences of the labours of man are then dwelt on, but our author does not give a very favourable report on the state of agriculture round the metropolis of India; for he states, that with the exception of being able to produce a supply of vegetables for the markets in the cold season, matters are much the same as in the days of Job Charnock, the founder of Calcutta.

An interesting account of the hot and cold seasons, and of their effects on the newly-arrived as well as on the more seasoned European, follows, and we then proceed to the statistics of sickness and mortality in Calcutta.

The subject of statistics is one unfortunately that has been almost entirely neglected by the medical authorities of the Bengal Presidency. It is with great regret that we learn that, owing to this deficiency, no report can be given—

"Of the hospitals of Calcutta, and of the general hospital in particular;—an institution that has existed for more than seventy years, and in which tens of thousands of European soldiers have been treated under three or four different

medical systems; yet no one fact, out of the numerous and important observations made during that long time, is known to any one of us. Its surgeons, and those of the other public institutions,—many of them able and experienced officers,—have, through the neglect of the controlling medical authorities, been rendered, in respect to us and to science, no more than a set of dumb actors in the circle of a routine duty."

Mr. Martin we know has repeatedly remonstrated against this official neglect, but we also know how hard it is to set the official mind right, to stimulate it into wholesome and vigorous action. Owing to the absence of statistical record, it is not easy to give a satisfactory answer to the question as to whether of late years the climate is improved, or the mortality of Europeans diminished. Our author, however, is of opinion that the climate of the actual site of Calcutta is, to a certain extent, more healthy, and that the general mortality amongst Europeans, especially of the better classes, is diminished; but that the chief cause of this will be found in the improved habits of the higher ranks; for, with the troops in garrison, notwithstanding the superior discipline and interior economy of modern times, it would not appear that mortality is much, if at all, lessened; and the same may be said of the nearest military station of former times—Berhampore.

The chapter On the Prevention of Disease contains much that is especially worthy of perusal. Notwithstanding our boasted civilization, we are only beginning to appreciate, as a nation, the higher and nobler offices of the medical profession, in their preventive or prophylactic capacity. In Bengal indeed a systematic plan for requiring from all competent medical officers reports on medical topography, and sanitary statistics of those parts with which they may be best acquainted, has been adopted on the recommendation of Mr. Martin, and his proposals for the sanitary improvement of Calcutta have been carried out under legislative enactments.

In the application of the principles of hygiene to military purposes, the disadvantages of the medical officer are almost endless. In the British armies, whether at home or on foreign service, the rule appears to be to leave even sanitary measures to what are called the proper authorities; the advice of the surgeon is scarcely ever asked, and if volunteered is pretty certain to be disregarded. Mr. Martin mentions, for instance, that once, while serving in one of the most pestilential countries known in India, he made a topographic examination of the localities, and reported the result to his commanding officer, suggesting at the same time what he regarded as the most suitable arrangement for encamping the men against the coming rainy season, when it was well known that a great increase of deadly fever would result. The answer was briefly—"I'll be d—d if I do." The field officer who treated a grave matter of duty in so contemptuous a manner himself paid the penalty of his neglect, for before the year was over he had lost his life.

As M. Thiers remarks, in ordinary histories we see only armies completely formed and ready to enter into action; the effort it has cost to bring the soldier to his post, to feed, to train, and finally to cure him if sick or wounded, are lost sight of. The nation sympathizes deeply with the results of a battle,—the slaughter of a few hundreds by the sword or bullet excites the keenest feelings of regret or of anger; but the tenfold

loss occasioned by mismanagement, by unnecessary exposure to the burning sun or to the heavy dew, by the unhealthy bivouac, or the malarious encampment, passes unnoticed, or is looked on as the necessary consequence of warfare. England, often engaged in great wars, is perhaps of all nations the most slow to learn the real nature of war and of its requirements, and when peace returns, all the experiences of former wars are forgotten in the eager pursuits of commerce.

We may quote here with propriety the remarks with which the author concludes his observations on this interesting subject:

"Finally—it cannot be too often repeated, that on the perfection of the so-called civil establishments—the medical and the commissariat departments—must depend not only the efficiency, but the very existence of our fleets and armies.

"The recent calamitous losses in the Crimea have demonstrated another fact,—viz., that so long as the departments above-named are dependent and subordinate, and under the management and control of officers holding but an inferior rank, consideration, and power, so long will they remain insufficient to their great purposes; but let them once be ruled and directed by responsible officers of a rank and station adequate to command an instant attention to their respective wants—let such officers be largely and immediately responsible—and then, but not till then, will the two most important establishments of our fleets and armies rise so as to be equal to all requirements, whether during peace or in war."

The chapter which follows—the longest in the work—enters minutely into the nature, causes, and treatment of remittent fever,—the subject which the author considers the most important which can engage the attention or excite the interest of the practitioner in tropical regions. He states that on the thorough and complete knowledge of this affection must depend nine-tenths of the usefulness of every medical officer who is called upon to combat the diseases of hot climates, and that he who can treat remittent fever successfully is not likely to be deficient in the skill to manage any other tropical malady. This chapter will no doubt engage the serious attention of our professional brethren in the East, and will amply reward them for the time devoted to its perusal.

The chapter On Fever is followed by an article on the next most important disease of tropical climates and of armies in general, in every region, especially when engaged in active service,—acute dysentery. As William Fergusson states, this is essentially an army disease. "The soldier," he says, "when in the field, may escape fever, but never dysentery, if he lie on the ground." Out of a force of 25,000 men serving in Bengal Proper, between 1823 and 1836, there occurred about 8500 cases of dysentery and diarrhœa. The losses of the French army in Egypt were greater by dysentery than by the plague. During two years and a half only of the time occupied in the Peninsular war, the British army, according to Sir James M'Grigor, lost 4717 men by dysentery, and the last winter campaign in the Crimea has made us only too conversant with the fatal effects of this scourge of armies.

Mr. Martin treats succinctly of this interesting subject in all its known complications, and in its most destructive forms when complicated with scorbutus, as witnessed by himself personally on the expedition to Rangoon during the first Burmese war. He describes it, as well as the forms in which it subsequently appeared in China, and as it has been

seen on the shores of the Black Sea during the war just concluded. The remarks on the predisposing causes, on the best means of preventing this formidable disease, and on the treatment of it when it has actually supervened, well deserve the attention of those surgeons to whom the care of our soldiers and sailors is entrusted.

The next subject which comes under review is the diseases of the liver —diseases which, either as original or as secondary affections, constitute in reality a vast proportion of the sufferings induced by a protracted residence in tropical climates, especially amongst those in active military service. The official returns, indeed, under the special heads of hepatitis, acute or chronic, record but a very inadequate proportion; for when the hepatic affection occurs as a complication or sequela of fevers, dysenteries, or other allied disorders, the cases are classed with the primary disease, the condition of the liver remaining unnoted.

Mr. Martin provides us, however, with a series of statistical tables, illustrating the actual amount of these disorders, of much novelty, interest, and value, for which we must refer the reader to the work itself. We may quote, however, the results of the author's experience with respect to the treatment and probable termination of abscess of the liver:

"When the abscess is small, we may favour the efforts of nature at absorption by care in diet and clothing, gestation in the open air, and change of air, the use of the nitro-muriatic acid, with bitter tonics, &c. When the abscess has happily discharged its contents into the stomach or bowels, or externally, a more nutritious diet will be proper, with the view to support the strength of the patient, and even a little wine may now be allowed. But nothing should be done which may excite the nervous or vascular functions; and mercury in every form is to be carefully avoided, and even the mildest aperients should only be exhibited under necessity.

"Of the instances of recovery within my personal knowledge, by far the greater numbers were amongst such as had discharged the pus by the bowels; and one married lady I remember to have recovered health rapidly, after vomiting the contents of an enormous abscess. I also saw four instances of recovery where the abscess had opened into the lung. Of those in whom the pus was discharged through the external integument, the majority have, within my knowledge, died. But by much the greater proportion of the instances of hepatic abscess end fatally, either before the bursting of the abscess, or within a few hours from its discharge into the peritoneal cavity. And this brings me to the consideration of the propriety of making an opening for the discharge of such abscesses through the external integument. I have often seen this operation performed in India, and I have myself performed it; but in no instance that I remember did the operation appear to me to result in eventual good."

It will be seen that Mr. Martin agrees with Dr. Budd as respects the impropriety, as a general rule, of any surgical interference in these cases.

An article on endemic congestion of the spleen follows naturally upon that on hepatic lesions. The true nature of splenic cachexia is fully entered into, and the fatal nature of the splenic complication, both in malarious fevers and in epidemic dysenteries, carefully pointed out. In the treatment of the chronic enlargement of the spleen, we notice that the combination of tonics and aperients, which forms the general practice in Bengal, coincides very closely with the experience of the natives, who seem to have been long aware of the beneficial results in this complaint

of purgatives, chalybeates, and acids; a very successful native formula, consisting of a mixture of aloes, vinegar, and garlic, with a small proportion of the bazar sulphate of iron, "Kuzees." In Europeans, where the liver is involved in disease along with the spleen, as is so frequently the case, Mr. Martin considers that there is no remedy which, in his experience, can at all be compared in power and efficacy to the persevering use of the nitro-muriatic acid bath, using the combined acids internally at the same time, with bitter infusions, and keeping the bowels freely open.

The chapters On Epidemic Cholera, and On Yellow Fever, in addition to some very interesting historical and topographical notices having reference to the origin and progress of those epidemics, contain a great amount of useful and practical information on the subjects of which they treat. These subjects have, however, been so freely discussed of late in this journal, that we refrain now from any further allusion to them.

Unfortunately, a work relating to the diseases affecting our soldiers and sailors, as well as the civil service, in warm, indeed in any climates, must necessarily include an account of delirium tremens. Some tables furnished by Colonel Tulloch, and given by the author, demonstrate both the frequency and the importance of this affection, the more important as it forms a very frequent complication with many of the acute diseases of hot countries. If we could trust to a report of the statistics of delirium tremens amongst the troops in Canada, during thirty years, by the Inspector-General Henry, we must fear that the habits of our soldiers, in Canada at least, are not improving, the disease becoming more frequent; at least, while in the first fifteen years the ratio of cases to strength was as 1 to 175, in the second fifteen years it was as 1 to 75. We must fear that the same increase extends to other stations, and is still continuing. An interesting table furnishes us at once with proof of the far greater frequency of this affection amongst the troops stationed in our tropical colonies or dependencies, and suggests also some curious and perhaps profitable inquiries as to the causes which give rise to the variations, both in numbers and in mortality, among the different Presidencies. In four years, for instance, in Bengal, the aggregate strength of the European troops being 36,286, the number of deaths was 14, or in the ratio of 1 death to 48 cases admitted. In Bombay, during the same period of time, the aggregate strength being only 18,073, the deaths were 15, or nearly as 1 in 7 of those admitted.

With respect to the prevention of this degrading malady, many practitioners have recommended doses of tartar-emetic or powdered ipecacuanha, to be given at the moment the craving for strong drinks comes on, or to be mixed with the intoxicating liquors, for the purpose of exciting a feeling of disgust. When the person who has made himself a slave to this evil habit of drinking, makes an effort to extricate himself from it, he at first feels himself most miserable, the stomach misses and craves for its usual stimulant, and some substitute must be found. In Canada, according to the experience of Henry, the best medicine under these circumstances consists of a wineglassful of a mixture of the infusion and tincture of gentian, with a little sulphate of magnesia, taken early in the morning, at mid-day, and again in the evening. This would seem

to correspond with the practice which prevails in some of the London prisons, where those who have been in the habit of drinking largely, are allowed, under their compulsory habits of temperance, to drink freely of wormwood tea, which they call for eagerly.

Referring again to military health, we have four very valuable articles —On the Habits of European Soldiers, On the Term of Efficient Service in India, On the Selection and Improvement of Localities for European Troops, and On the Mortality and Physical Management of European Children in Bengal.

In the first of these we find some valuable hints with respect to the improvements which might be adopted in the management of the soldiers. The expediency of finding occupation for the men is strongly advocated. The abuse of ardent spirits, in our various colonies especially, is freely denounced; and another habit—that of using tobacco to excess—incurs justly the author's warmest censure, both as one bad in itself, and as a comparatively new one. He states that he has

"Seen many cases of severe constitutional and cardiac disturbance from its abuse, with perfect recovery of health on the discontinuance of the habit; the digestive functions, those of the heart and nerves, having been seriously affected in the most inveterate smokers. Of the miseries, mental and bodily, which I have witnessed in the persons of young officers from the abuse of cigars, I will only say that they very far exceed those detailed in the 'Confessions of an Opium-Eater.'"

With respect to the selection and improvement of localities for troops, our conduct hitherto has been most reckless and destructive. Appointing as our commanders men, as a rule, ignorant of, and for the most part disinclined even to learn, the most simple rules affecting health; without the official assistance of medical or health officers holding a definite position in the council of military commanders, who might at least give them advice on the subject, the sacrifice of human life to ignorance has been appalling. According to our author, Colonel Tulloch has stated to him, as the results of his investigations in the War Office, that of British soldiers of the Queen's and Company's armies in the East Indies alone, there perished unnecessarily, from 1815 to 1855 inclusive, about one hundred thousand men!

As an example both of the unnecessary destruction of life, and of the benefits to be derived from scientific investigation, we may mention the results as regards the mortality of British troops caused by their removal from the pestilential plains of Jamaica to the healthy mountain regions, first suggested and advocated by Robert Jackson, and finally carried out by the late Lord Metcalfe:

"From 1803 to 1816, and for how long before we do not know, the soldiers perished in Jamaica at the rate of a hundred and thirty per thousand per annum; while by the simple and easily-arranged measure ordered by Sir Charles Metcalfe, the mortality has, since 1842, been reduced to thirty-five per thousand per annum."

The rules and observations on this most important subject laid down by Mr. Martin merit the closest attention.

From the opportunities which our author enjoyed during a long and varied course of service in India, of observing the effects of climate on the moral as well as on the physical nature of Europeans, especially among

the military classes, the conclusion at which he arrives is, that the term of thirty years of actual residence in the East is the very utmost during which persons under ordinary health, and of an average constitution, may be expected to retain their British vigour of thought and action. If this be the case, the evils of an absolute rule of seniority promotion to a scientific corps like the medical department of the army of Bengal must necessarily be great, attended as it is with the detention of the whole administrative staff till long after the period of efficient service in India is passed away.

On the subject of European children in Bengal, Mr. Martin states, that for children of strumous habit, the climate of Bengal and of India generally may be considered favourable. The diseases of childhood run their course very mildly; and, with proper precautions, up to the age of five or six, European children stand a reasonable chance of thriving. At this age, however, they begin to emaciate, to outgrow their strength, to exhibit the necessity for a change of climate. It was at one time imagined that child-bearing was peculiarly fatal to women in Calcutta, that puerperal fever prevailed to a great extent; we learn, however, from the author, that, from whatever cause it may arise, the very contrary holds good at present, parturition being attended with even less risk than in Europe.

"Puerperal fever I never saw in Calcutta; and during sixteen years, in which I was familiar with the state of health among the better classes of Europeans there, I heard of but one death connected with child-bearing, independent of previous disease, and I remember but very few instances of children being born dead."

The alteration may perhaps, to some degree, be accounted for by the fact, that in olden times the management of parturition was left to ignorant native nurses, the public being solemnly warned, in a Calcutta journal of 1780, against the *improper* interference of European physicians!

We have now arrived at the conclusion of the first part, and here we cannot refrain from calling attention to the importance of the subjects of which it treats, to the necessity for a thorough and earnest study of them by all who are called upon to manage the diseases of Europeans in our inter-tropical possessions. The great responsibility which attaches to the medical practitioner in these regions, where the disease is both acute and rapid, where the treatment to be effectual must be both prompt and decisive, is urgently and most properly represented by Mr. Martin, and cannot be too seriously considered.

The second part is no less interesting to the practitioner at home, who is liable to be consulted by the traveller returning with his health more or less shattered, perhaps with organic visceral derangements, or at any rate suffering from the change from a hot to a cold country, obliged as it were to re-acclimatize himself, to alter once more the habits and observances to which his frame had become accustomed. As in the former part of the work, so in this, the investigation of the particular diseases is preceded by an examination of the physiological influences of climate as met with both in the tropics and in temperate regions. Even the children of Europeans in India, who remain there only during the first five or six years of their life, exhibit, after their arrival in this country, an unusual restlessness and mobility of the nervous system, the effect, no doubt, of a

long-continued application of heat. This nervous excitement is associated with a comparative incapacity for study, leading to frequent complaints on the part of the schoolmaster. Fortunately, this condition yields gradually to the effects of time and air, till it finally disappears between the ages of fourteen and eighteen.

Passive congestion, affecting, as the case may be, the cerebral, the thoracic, or the abdominal organs mainly, is the condition most prevalent under European cold, and requires great care on the part of the sufferer, who is only too apt to expose himsef to the damp and cold of our atmosphere. Anæmia, again, is common to fifteen out of twenty of the tropical invalids on their return to Europe. The nervous and muscular systems of the heart and uterus are relaxed and weakened, and hence to a certain degree the diminished power of the forces that circulate the blood, and the intermitting character of the pulse so common to old Indians, as well as the frequency of uterine hæmorrhage in females who have resided long in that country.

These introductory observations are followed by an interesting chapter on the anæmia from which so many tropical invalids suffer. Whatever may have been the nature of the previous acute disease—fever, dysentery, diarrhœa, hepatitis, or cholera—we are pretty sure to find the blood impoverished in quality as well as in quantity. One not uncommon source of this deficiency of blood, is the custom which prevails too frequently in India of putting on an enormous number of leeches, and allowing the oozing to continue. One patient, whose case is mentioned by the author, had, in little more than twelve months, 1200 leeches applied to his side; whilst another had, for weeks in succession, upwards of 150 leeches per week; this last patient calculated, that in six years at least 3000 leeches had been draining him of blood. The general remarks on this subject, with the rules laid down for its treatment, are highly suggestive, and appear to be most judicious.

After some interesting notes on the treatment of invalids suffering from the sequelæ to the fevers of tropical climates, we arrive at the chapter On Chronic Diarrhœa. This affection, which was described by Dr. Baillie as extremely fatal in its character, whether originally contracted as diarrhœa in India, or forming one of the sequelæ of numerous diseases, is seldom simple in its pathological nature—that is, confined to the mucous digestive surface alone.

"On the contrary, a careful exploration of the abdominal regions, coupled with an attentive consideration of all the antecedent and attendant circumstances, will generally show that diarrhœa, in a large proportion of instances, is complicated with, if not mainly dependent on, chronic disorders and diseases of the liver."

The pathology of this affection, the author remarks, has hitherto been ill understood. Whilst chronic dysentery is the result of inflammation of the mucous lining of the large intestine, chronic diarrhœa, as it occurs in Europe, usually presents

"No appreciable inflammation of any portion of the mucous digestive surfaces; indeed, when diarrhœa has existed for any length of time, we find an opposite state of the general system and of the bowels to that of inflammation—an atony of the system, and consequent atony of the digestive functions, and a specially atonic

condition of the discharges from the whole of the secreting organs of the abdomen, including those of the mucous membranes. The brain and nervous centres are likewise in a state of complete anæmia."

In some instances, the diarrhœa is simple in its nature; in too many, it is associated with hepatic or splenic disease.

Chronic dysentery, the next subject treated of, is considered by our author a more manageable disease than chronic diarrhœa, the process of healing an ulcerated bowel in the former being more readily brought about than the restoration of the hepatic function in the latter affection. It must be remembered, however, that many of the cases of severer hepatic complication die off at once in India, and never reach this country. With respect to the treatment, Mr. Martin states that he seldom has recourse to mercury in the chronic dysentery as met with in England. Where there is recent congestion of the liver—from the application of cold, for instance—a few mild doses of mercury, with sudorifics and purgatives, aided by the warm bath, are admissible; when, on the other hand, chronic enlargement of the liver is associated with chronic dysentery, the condition of the system being anæmic, the nitro-muriatic acid bath is preferred to mercurials.

After some very judicious observations on the relaxed condition of the mucous membrane near the anus, with its hæmorrhoidal complications, the author makes some remarks respecting the performance of surgical operations in returned Indians, which it may be well perhaps for the surgeon here to bear in mind. To treat them with justice under these circumstances, he says—

"Requires a careful regard to their peculiar habit—care beforehand and care in the after-treatment being necessary. The nervous system of the returned Indian is excitable, and if but recently from the East, he is often anæmic—conditions of the system which obviously require much attention. I saw a lady but recently arrived in England, in whom the opening of a small encysted tumour nearly proved fatal; and, in the case of a gentleman, the same dangerous symptoms resulted from the removal of a small internal hæmorrhoidal tumour by ligature."

The chapter On Chronic Affections of the Liver is one of much interest. Torpor of the hepatic function under the cold and damp of an European climate would seem, *à priori*, likely to follow on the over-stimulation of the organ under the ardent sun of the tropics:

"To the increased biliary secretion of the hot and rainy seasons in India, followed by proportionate diminution of the hepatic function in each succeeding cold season—alternations extending over many years—a more enduring impairment of function has now succeeded, accompanied generally by great reduction in the reparative powers of the constitution."

This torpor of the liver is usually accompanied by an anæmic state of the whole system, and in some cases is so great, and the case proves so little amenable to treatment, that we are forced to suspect some alteration, more or less extensive, or some permanent injury to the elementary secreting structures of the organ. In the management of the simpler forms of this affection, the author very properly cautions the practitioner against pushing remedies too far, against administering too large and powerful doses—against, in fact, too heroic a line of treatment. The restoration of

the impaired functions must be solicited rather than urged; the debility can only be relieved by gentle and slow degrees.

Where there is reason to conclude that organic changes have taken place in the liver, or that its torpor is associated with induration or enlargement of the spleen, Mr. Martin has recourse at once to the nitro-muriatic bath. It is in this condition, and especially in the chronic enlargement of the liver, that this acid, used as a bath, as well as internally, proves of so much service. First employed in this manner by the late Dr. Helenus Scott, of the Bombay army, for upwards of twenty years, Mr. Martin has persevered in its use, and warmly advocated its efficacy.

The concluding remarks are devoted to the distinction which should be drawn between the free administration of mercury rendered necessary in India, where the symptoms and progress of disease are so acutely violent as to leave but a very few days between recovery and death, and the treatment which should be adopted with respect to the administration of that drug in this country in the returned Indian. In the case of the latter, the patient is usually emaciated, relaxed, enfeebled, and anæmic; the affections are chronic in their nature; their progress naturally slow; in him the free and continued use of mercury, therefore, would be as injurious as in the former case it may be beneficial; and is, accordingly, strongly discountenanced by the author.

To the preceding rapid sketch, we have now but to add the expression of our candid opinion, that Mr. Martin has not only sustained, but materially added to, the reputation which the work in its original form established for him.

Review X.

Cell Therapeutics. By W. Addison, M.D., F.R.S.—*London*, 1856. pp. 84.

The idea that *informs* this work of Dr. Addison's is contained in the following passage from his introduction:

"If cells and nuclei are of such generality and importance as to take rank as agents in normal growth, so that *all* healthy changes of structure and function be referred to them, they can hold no inferior rank, being present in *all* therapeutical reactions against injuries and diseases. The cell doctrine has been gradually advancing its footing in physiology for twenty years; it has also gained a strong hold in pathology; and it seems now required that therapeutical changes in the qualities of the blood should be shown to be either in harmony with it, or capable of a rational explanation without it."

He subsequently follows this idea out to its development in seven chapters—On Reparation, Granulations and Pus, Blood Distempers, Inflammation, The Distinction between Vascular Tissue and Parenchymatous Substances, Organic Disease and the Process of Repair, and Chronic Inflammation. In Chapter VIII. he presents us with conclusions; and the volume closes with a section On Unity of Design and the Physiology of Inflammation.

Dr. Addison is known as an original worker and careful observer; and

his professional brethren will, we are sure, look on any of his labours as just objects of their respect and attention. We proceed to lay before our readers an exposition of the author's views, adding thereto sundry comments of our own.

Taking first his chapter On Cells and Cell Growths, On Granulations and Pus, we find evidence collected to show that cells have special and independent properties—such that one cell may, though in immediate contact with another, form a product visibly unlike that of its neighbour: the chief proofs of this are derived from the vegetable kingdom. He then claims the property of selective absorption for all cells, and especially for the cells of granulations and pus; quoting Kölliker to the effect that cells do not admit every kind of matter indiscriminately, but take up one constituent and reject another. We must say that, to our mind, the evidence for this is exceedingly unsatisfactory, and indeed, utterly overborne by the counter-proofs. Surely, there is no manner of doubt that cells will absorb all manner of things they were never intended to, as well as what they naturally ought. The epithelial cells of the skin or of the intestinal canal absorb mercury brought into contact with them, or arsenic, and a multitude of other matters beside; arsenic applied to the granulating surface of a sore is absorbed, and so it is if laid on the vaginal mucous membrane; the cell-layer on the interior of an ovarian cyst will absorb iodine. We should really think it nearer the truth to say that cells do absorb indiscriminately. Even plant cells, though they will certainly take up from the same soil different proportions of different salts, according to the nature of each individual, yet have no power of refusing solutions of matters that are poisonous to the structure. The view of cells having a special power of selective absorption is invoked to explain the occurrence of abscess and ulceration without hæmorrhage. Dr. Addison thinks the pus cells capable of absorbing the living boundary-tissue in each case, of severing bloodvessels without bleeding, and of making openings in the older vessels for the junction of new ones. How the pus-corpuscles are to do such things we cannot well think. In our view, the process of ulceration is one of gradual decay and liquefaction of a tissue whose normal nutritive action in some way is arrested, it may be by withdrawal of nervous influence, by absence of a due supply of arterial blood, as the result of inflammation or of pure debility. Pus has no essential connexion with ulceration; it represents a quantity of plasma effused in excess, and corpusculating. It belongs more to reparation than to ulceration. In the formation of abscess, we have (take the case of a common phlegmon) effusion of fibrinous fluid and arrest of the normal nutrition of the part; the effused matter suppurates or forms pus, with which the dead (sloughing) tissue is blended; the inflammatory process goes on, more tissue dies and more exudation suppurates, and at length, the most advance being made in that direction where there is least resistance, the boundary is perforated, and the pus escapes. We entirely agree with Mr. Paget, that—

"Inflammation appears to be not only conducive but essential to the spontaneous opening of abscesses; for where it is absent the matter of chronic abscesses will remain, like the contents of any cyst, quiet for weeks, or months, or years; and when, in chronic abscesses or in cysts, inflammation ensues through the whole

thickness of their coverings, it is usually certain that their opening is near at hand."*

The non-occurrence of bleeding is owing to the circumstance that the exudation, the result of the inflammation, compresses and seals up the vessels as it takes place, the blood at the same time coagulating in them. If this exudation be of poor and bad quality, and the blood indisposed to coagulate, the ulceration becomes phagedenic, and bleeding may occur. It is as a sign of the presence and abundance of healthy exudation that pus is termed "laudable." It is not the used or useful part of the exudation, but the excess, that is let go to waste. It is not at all necessary to repair, which, in its best mode, is effected without any of it. The formation of bloodvessels in granulations, continuous with those of the general circulation, is in all probability effected by the process of outgrowth; loops being thrown out by the extension of one capillary channel coalescing with that of another, or in some such way. Again we refer to Mr. Paget's account of this process;† and we would observe that this common simple phenomenon, occurring as it does by a gradual modelling and growth of pre-existing tissue, just puts before us the utter mystery of vital sculpture. It is no cell doctrine or cell physiology that will ever explain to us how it comes to pass that a capillary plexus should extend and ramify through a layer of new exudation. The fact remains as inexplicable as if we had never heard of cells.

In the chapter On Blood Distemper, after certain generalities respecting the action of miasms on the blood, Dr. Addison proposes the hypothesis that the poison of variola acts specially on the glandules of the skin, which being separate, the pustules are discrete; whereas in scarlet fever, "the poison, in virtue of a property of selective absorption, acts most, or primarily, upon the deep cells of the epidermis." He intimates that, in these and like cases, the pus of the pustules, or the desquamating cells of the cutaneous surface, act therapeutically, conveying in themselves the morbid matter out of the system.

> "In measles and scarlet fever," he writes, "recovery from the disorder is concomitant with an exfoliation of cell particles from the skin; and that these discharge poisonous matter from the blood seems proved by the contagious properties which it is well known they possess."

We will grant that, in these and such like instances, there may be discerned, dimly shadowed forth, a therapeutic or conservative tendency; but we must say, at the same time, that such lame and impotent efforts are exceedingly unlike the real workings of nature in her methods of cure. Why does a patient get well of variola, or typhoid fever, or any other exanthematous disease? Is it because he has cast out of his body, through the various emunctories, the morbid matter which was vexing it? Surely no: but because *in* his body that morbid matter, that fever-stuff, has undergone such changes that it has ceased to exist. To use the old phrase, the poison has become concocted, *cooked up* (somewhat like railway accounts, and, like them, changed!); and now it is no longer a vexing and mischievous thing, whatever after becomes of it. Contrast with this the case of glanders, and its ever-recurring abscesses. If elimination was the mode of cure, that which nature really must have for relief, then surely

* Surgical Pathology, vol. i. p. 410. † Ibid., p. 215.

we should see more good come of purging, or sweating, or diuresis in fevers; and when they terminated, it would be always by some critical secretion. The pre-Sydenhamite plan of treating variola would prove more successful than that which we now follow. Moreover, if the epidermic-cell therapeutics are of any real moment, anything more than a "*Nebenwerk*," what are we to think of the pulmonary therapeutics? That vast surface which clears off such an amount of poisonous gaseous excreta daily, and from which we know the infectious miasmata do pass off, must surely be far more potent in its eliminating agency than the much less vascular skin. But cell structure has nothing to do with the extrication of the carbonic acid and other gaseous matters from the lungs; mere physical laws effect their transfer from the blood to the atmosphere. Does not this consideration throw cell therapeutics rather into the background? Again, if in variola the pustulation had any really therapeutic intent in the way of discharging the virus, we might surely expect to see the same end attained in a much more natural and less roundabout way by utilizing already-existing cell growth, rather than creating new and abnormal. Having recently examined a variolous pustule with the aid of the microscope, we think we can be sure that the morbid action is not located in the glands of the skin, as Dr. Addison supposes, but occupies a spot of the plain surface. Now, had the object been to eliminate the virus by cell agency, would it not have been far better to have devolved this function on the perspiratory glands than to have raised up a crop of abnormal cells for the purpose? Would not a flow of perspiration have been far better than a flow of pus? The same will apply to the other exanthemata. Such teleological reasoning is quite warranted when we consider and contrast with the so-called cell therapeutics the real conservative or therapeutical actions of nature, which are so well adapted to the purpose they have to serve.

The chapter On Inflammation opens with these words:—"Inflammation is sometimes acute, sometimes chronic. The argument is, that inflammation is new cell growth in the common vascular tissue. The term 'acute' means rapid. Cell growth is a rapid growth." To the argument, thus stated, we enter a decided protest. Inflammation in its most typical instance is quite another thing than new cell growth in the common vascular tissue (by this we believe Dr. Addison means the areolar). The new cell growth is an accident, the mere result of the overflow of plasma, which cannot get through the obstructed vessels, and yet is poured on in abundance through dilated arteries. The proper and true termination of inflammation is gangrene. Arrest of nutritive action is essential to inflammation; and if that arrest be carried somewhat farther, the tissue dies *en masse*, sloughs. Dr. Addison's definition of inflammation would better suit the formation of a fibrous tumour or a cancer. Nor is cell growth within the inflamed vessels an essential part of inflammation; the best authorities are agreed that production of an unusual number of white corpuscles in the blood is no necessary part of the process.

To the inference which he draws from a fatal case of blood-poisoning, the virus being the result of puerperal peritonitis, we make no objection, absolutely. No doubt, the inflammation of the absorbent glands had in some faint degree a beneficial tendency, and was not the cause of death;

yet we hardly think that such an action as this should be considered as mainly therapeutical. To us it appears as an occurrence almost inevitable, by reason of the anatomical arrangement, and not exhibiting any marked conservative effort. A poisoned lymph current irritates the gland through which it passes, and makes it inflame and suppurate;—but what then? Will this adenitis get rid of the poison that has passed into the blood? We trow not. Mr. Simon gives an instance much more to the point, though he does not call it cell therapeutics, when he describes how "a sufferer can commonly identify the material which is producing his diarrhœa as that which had previously been inhaled by him, for it retains its peculiar smell, and imparts it to the increased excretion." Thus, as the result of dissecting a porpoise or a dog—"the evacuations present quite unmistakeably the smell of the one beast's skin, or of the other's blubber." Here it may be fairly supposed the epithelial particles of the Lieberkuhn follicles do really excrete morbid matter from the blood; and such a proceeding as this is worthy of that agency of which it is said, "*Natura nihil agit frustra.*"

In the chapter On the Bloodvessels and Connective Tissue, our author draws just distinctions between them and the various parenchymata; and in the next he endeavours to substantiate the argument, "that when parenchymatous degeneration or organic disease so affects the bloodvessels as to render bleeding imminent, new cell growth in the vascular tissue arises to anticipate or prevent it. Signs of inflammation being combined with the evidences of organic disease." The cases he selects for examples of this therapeutical tendency are "softening of the brain, fatty degeneration of the liver or kidney, and tubercles in the lungs." With regard to the three first, we must confess that we know of no instances in which any such conservative action takes place. Diseased bloodvessels in the brain are the commonest cause of cerebral hæmorrhage, and if softening precedes the hæmorrhage, it surely favours rather than opposes it. Fatty degeneration of the liver is attended with no interstitial cell growth, nor is any similar state of the kidney. In the case of fatty liver, there is marked anæmia of the organ, and no tendency to hæmorrhage; in the enlarged fatty kidney, the refluent pressure of the blood on the Malpighian tufts favours the occurrence of hæmorrhage. We do not see, therefore, how these cases make for Dr. Addison's argument. In pulmonary tuberculosis we have indications of a conservative tendency, provided the morbid process is not intense, in the closing of bloodvessels, the development of layers of false membrane on the pleura covering a superficial vomica, and the formation of a fibroid lining to a quiescent vomica. But in all these instances, surely we must regard the quality of the exudation, and think much more of its consolidating, fibrefying property, than of its cell character. Dr. Addison is writing on cell therapeutics; but we affirm that these are rather instances of fibre therapeutics, and that the more there is of *cell* in them the worse. In the tubercles themselves, and in the pus around them, are plenty of cells, but these are the mischief; as these cease to be produced, and as fibroid production goes on, so does recovery. When a mass of tubercle becomes quiescent and obsolete, it is by the wasting and disappearance of its corpuscles. When Dr. Addison quotes cirrhosis of the liver as an instance in which there "is slow

wasting or decay of the parenchymatous cells of the organ without bleeding, and as gradual a replacement of the lost parenchyma by a growth of fibrous connective tissue which occupies the void," he seems to us to put the cart before the horse. It is the growth of the new fibrous tissue which starves the natural cell parenchyma. The case is an instance of cell-, or rather fibre-, destructiveness, not therapeutism.

The chapter On Chronic Inflammation contains several just and interesting remarks relative to various circumstances which may retard or prevent the process of repair (cell therapeutics); as well as some suggestions why, when no useful purpose can be accomplished, cell growth yet continues. For these we must refer to the work itself. The chapter of Conclusions is very much a *résumé* of what has preceded, and therefore our previous remarks will apply to its contents.

Our own general conclusions respecting the author's views we must state as follows:—*First.* To a certain limited extent, his leading conception is correct, but does not possess, it appears to us, much novelty. Everybody has known now for some good while, that granulations and pus are made up of celloid particles, and every one is familiar with the conception, that the eruptions of the exanthemata are eliminative efforts. We do not see what is gained by applying to such processes the name of cell therapeutics; we may add that, in our own private belief, these actions are not at all really eliminative, any more than a chronic eczema, psoriasis, or pemphigus, which we are very glad to cure without troubling ourselves as to what becomes of the materies morbi. In our view, these and the like are vascular disorders of nerve origin, and have herein a certain affinity with fevers, according to the doctrine respecting the genesis of the latter, maintained so ably by Virchow and Parkes. *Secondly.* The author cuts down his facts too much to suit his idea. He speaks much of the elimination by the skin in variola, measles, scarlet fever; but omits any reference to that by the lungs, or to the alteration, concretion, of the virus in the system. He dwells on the exfoliation of the cuticle about the inflamed joints in gout, as a means of eliminating the materies morbi (giving, however, no proof that such is the case), and says nothing of the tenfold greater evacuation which takes place through the kidneys. He after cites the granulation process of repair, in which there are cells in plenty, but does not even mention the modelling one of Dr. Macartney (much the more preferable), in which cells must be far less numerous. *Thirdly.* He does not seem to consider sufficiently, that if cells may play a therapeutic part in some degree, they may also play a fearfully destructive part. A treatise on cell destructiveness might just as well be written as on cell therapeutics. A cancer or a fibrous tumour is made up of cells, or celloid structure; and the more there is of cells in the cancer, the more intensely malignant is it. A miliary tubercle is full of cells, yet it is a focus of mischief. What is pulpy degeneration of the synovial membrane, with consecutive abscess, but very undesirable and disorganizing cell growth? What is scrofulosis of the absorbent glands but pretty nearly the same thing? What shall we say of ulceration of cartilage, or cystic disease of the testicle, or of the ovary? In fact, the simple truth is just this, that cells occur almost everywhere, and are concerned in almost all operations, healthy or morbid. To dwell, therefore, especially on cell

therapeutics, conveys a false idea, as if cells ministered in some especial and peculiar way to the operations of cure. It does not appear to us that medical science can gain much by such vagueness.

Before concluding, we would make one remark which this work has suggested to us. The terms, cell doctrine, and cell physiology, which the author uses evidently with full acceptance, appear to us far more pretentious and assuming than our knowledge justifies. That cells enter largely into the composition of many important organs is a demonstrable fact; that nuclei or cells are universally present in developmental periods is also established. These are plain fact, not theories. But what then? what new doctrine on physiology comes of them? Do we understand one whit more how a muscular fibre or a nerve tube lives and acts, because we know that cell nuclei were concerned in their formation? Do we know anything more about the secretion of bile, since the microscope showed us that the liver was made up of cells? Does the abundant presence of cell-growths in the embryo afford us even a gleam of light as to the laws of development? Here comes a liver, and there a heart, and there a kidney, and there certain bloodvessels connected with them; but does the fact (not theory) of cells or nuclei constituting the main part of these organs, explain to us in the very least the enigma of living matter taking such shape and such relations constantly? Both testis and kidney are formed on the same plan, consisting of tubes lined with epithelial cell-growth, whose concernment in the work of secretion we cannot doubt. Yet these cell-growths certainly do not act alike. One *makes* the spermatozoa, and the other does not make, but merely *eliminates* the urinary salts. What gain we, then, from the knowledge of their cell structure? Should we not be about as wise if we merely knew that each organ was made up of tubes? Excepting that in most instances the acts of nutrition and secretion appear to resemble each other more closely than was previously supposed, we do not see that any actual gain has accrued to physiology from what is called the cell doctrine. The liver is made up of cells, and so is the grey matter of the brain; and the physiology of these organs is just where it would be if we were ignorant of their morphology. It is in the things that we cannot see, the secret vital qualities, that they differ.

We have now done; but though we have dissented in many respects from the views advanced in this work, we would not at all wish to speak disrespectfully of it. It is an honest effort in the cause of science; and we hold the belief strongly, that none such is in vain. In the intellectual as in the physical world, stagnation is decay. It is the turning over old soil and old doctrine, bringing new forces and new ideas to bear on old material, opening fresh channels for living matter and living thought, that makes cultivation in both cases effectual and fruitful. For this, and for all his other endeavours in this good cause, we return Dr. Addison our sincere thanks.

Review XI.

1. *On the Results of the Operations of the Gotha Life Assurance Bank for the First Twenty-five Years of its Existence, particularly with respect to the Mortality amongst the Lives Assured.* By G. Hopf, Esq., Manager of the Gotha Life Assurance Bank, and Corresponding Member of the Institute of Actuaries in London.*

2. *On the Medical Selection of Lives for Assurance.* By William Brinton, M.D., Fellow of the Royal College of Physicians; Honorary Fellow of King's College; Physician to the Royal Free Hospital; Lecturer on Physiology and on Forensic Medicine in St. Thomas's Hospital; Physician to the Mutual Life Assurance Society; Examining Physician to the Indian Railways, &c. &c.—*London*, 1856. pp. 58.

The statistics of life assurance companies must be carefully distinguished from the statistics of the population at large, since the aggregate of insurers may be regarded as the most provident as well as the most healthy part of the community. The deductions obtained by an analysis of the numbers presented by life assurance companies can therefore only be put into comparison with the results obtained from similar data; but the data being essentially the same, we may expect, in comparing different countries with one another, to acquire very interesting and valuable conclusions. The value of such comparisons does not, however, only refer to the duration of life or the rate of mortality, but also to social questions and political relations, by which we may account for the greater or less frequency of the withdrawal of policies, of the occurrence of fraudulent declarations, and the like. Thus, Mr. Hopf finds, in comparing the results of the Gotha Company, the oldest German insurance association (established 1827), with the experience of fifteen English companies, that whereas in the former the number of deaths was somewhat greater than of those whose policies lapsed or were withdrawn, in the latter, the number of deaths was 3928, while that of lapsed policies was 11,226. This would appear to bear out Mr. Hopf's observation, that "the German heads of families therefore seem to execute with greater perseverance the resolution they have once taken of being assured;" it is explicable on the ground of the majority of the insurers in Germany having fixed annual incomes, and of the changes of fortune incident to a commercial population not generally affecting them in the same manner as it does the bulk of insurers in Great Britain. The general fact of insurance indicating a vigorous class of the community, is equally borne out in all the countries from which data have been obtained. Thus we find, in Germany and Great Britain, the rate of mortality to be much lower among the insurers than among the population at large. The same has been observed to apply to our trades' friendly societies, as shown by Neison and Finlaison. Both in a scientific and in a moral and practical point of view, these questions are of great importance. The following table is a condensed view of two tables given by Mr. Hopf, in which our readers will find a comparison between the average mortality of different periods of life in the Gotha and other insurance companies and friendly societies, and the mortality of the same periods in mixed populations:

* The Assurance Magazine, July and October, 1855.

Mortality of the Gotha Company compared with that of other Companies and of Mixed Populations.

Ages.	Insurance Companies and Friendly Societies.						Mixed Populations.				
	Gotha Company.	Equitable Society.	Seventeen English offices.	Friendly Societies (Neison).	Friendly Societies (Finlaison).	Prussian Widows' Fund.	England (Farr).	France (Demonferrand).	Belgium (Quetelet).	Hanover (Tellkamp).	Saxony (Leonhardi).
26—30	0·87	0·78	0·81	0·73	0·75	0·70	0·97	0·88	1·50	1·11	1·07
31—35	0·92	0·88	0·89	0·80	0·80	0·85	1·10	0·95	1·58	1·25	0·99
36—40	1·00	1·08	0·99	0·89	0·95	1·09	1·25	0·94	1·72	1·35	1·15
41—45	1·04	1·18	1·13	1·04	1·18	1·35	1·42	1·17	2·16	1·48	1·57
46—50	1·45	1·38	1·43	1·29	1·37	1·74	1·62	1·48	2·43	1·77	2·07
51—55	1·82	1·85	1·91	1·70	1·77	2·24	1·87	1·93	2·47	2·50	2·71
56—60	2·77	2·68	2·65	2·24	2·45	3·11	2·71	2·50	3·15	3·61	3·71
61—65	3·83	3·72	3·79	3·05	3·12	4·60	3·95	4·10	4·24	5·70	5·43
66—70	6·08	5·48	5·55	4·62	4·75	6·57	5·75	5·60	5·83	7·91	7·37
71—75	9·04	7·89	8·13	6·85	6·70	10·04	8·32	9·25	8·65	9·34	10·39
76—80	11·35	11·18	11·88	8·84	10·32	13·18	11·94	12·79	12·86	12·74	15·48
81—85	23·94	17·97	17·22	11·97	15·31	19·32	16·90	18·62	18·01	18·50	21·74

It appears from the table that the ratio is particularly favourable to our own population, whether belonging to the class of insurers, members of friendly societies, or the mixed population. The rate of mortality is especially low for the advanced periods of life, and Mr. Hopf, in trying to account for this undeniable feature, admits that we can only come to the conclusion that, in general, mortality is less at the higher ages in England than in Germany.

Another feature which appears to characterize the class of persons who insure their lives, and results from Mr. Hopf's analysis of the Gotha statistics, is the much greater mortality of women at the earlier periods of life; in mixed populations, the reverse holds good. Thus, in the quinquennial periods, 26 to 30, 31 to 35, 36 to 40, the mortality of men is respectively 0·77, 0·88, and 0·98 per cent., while that of women at the same periods of life is 1·66, 1·79, 1·92. After 40, the difference ceases, and at the most advanced periods the females acquire an advantage over males. The Gotha bank do not insure pregnant women, nor have they ever succeeded in determining a case of fraud on the part of a female; and yet, as the author observes, the numbers before us clearly prove that "females understand better than males to gain advantage in the assurance." The following is his explanation of the fact:

"I think we must seek the principal cause of it in the circumstance that women, from the greater bashfulness peculiar to their sex, frequently do not communicate all their bodily infirmities and irregularities to their physicians, much less to others, and feel themselves much less under obligation to give notice to the assurance office of what they consider their own secret respecting the condition of their body."

And again:

"There is no doubt that a greater proportion of females who assure their lives at the younger years, die early. The deviation is too significant and too constant to be considered accidental. We are not able to explain it by any other supposition than by the circumstance that women feel internal hidden infirmities and defects in a higher degree than men, and have a presentiment of approaching danger in consequence of them, which impels them to assure their lives, or that they understand better and more skilfully than men to hide the true state of their health, and to deceive by it even their medical men."

It is, however, to be observed that the greater mortality of females below the age of forty does not apply in England, where the mortality of the two sexes is equal at that period of life. Our own experience would tend to show that this greater mortality among females before the climacteric, in Germany, is due rather to the greater fatality of childbirth, than to the hidden defects adverted to. We throw this out merely as an impression obtained by inspecting numerous returns of foreign agencies, than as a fact, since nothing but the comparison of extended statistics can serve to determine such a question. We should have no difficulty in accounting for the circumstance, if proved to be based in truth, from the much more frequent employment of midwives during labour, in Germany, even among the higher classes, than among ourselves.

We pointed out at the commencement of our remarks on the subject of life insurance, that insurers, as a class, present a much more favourable average duration of life than their uninsured compatriots. This, however, would not be the case, were it not for the surveillance exercised by the police of the insurance companies—their medical officers.

Persons who feel the taint of any disease that may sap their vital power, are even more likely than others to offer to insure their lives, in order to secure a provision for their wives and children. Were they admitted at the ordinary rates, the favourable averages spoken of as peculiar to the insured would soon be reduced below the average of the general population. It can only be by the careful and conscientious appreciation of all the injurious influences to which mankind are subjected, and by a deliberate weighing of the circumstantial as well as the direct evidence bearing upon the health of an individual, that a medical examiner to an insurance company can completely fulfil the duties of his post. He has to guard against nervous anxiety in watching over the interests of his company, quite as much as against a laxity in examining the applicants for the benefits of the institution. The shock to a person in average health on being declined on the ground of some imaginary predisposition, and the injury inflicted upon him by thus refusing him the benefits of assurance, not easily obtained elsewhere when once refused, are matters for the serious consideration of the medical officer of an insurance company. Dr. Brinton, we think, has put together all the points at issue in an extremely fair manner. He shows why it is necessary that the medical man should be made in a measure the guardian of the insurance company, and he gives a succinct outline of the principles by which the medical officer should be guided, as well as practical directions for the best mode of arriving at a satisfactory conclusion. While the limits of a single lecture are necessarily too circumscribed to discuss fully the bearing of various morbid conditions or previous diseases upon the prospects of ultimate longevity, or to enter minutely into the numerous details which are constantly suggesting themselves to medical men who are engaged in this kind of business, we would recommend a perusal of Dr. Brinton's lecture to all young hands who are about to undertake the duties of medical officers to insurance companies, and are not fully aware of the double responsibility which they assume, and probably not entirely acquainted with the points deserving their special consideration, or the manner in which they should be elicited.

PART SECOND.

Bibliographical Record.

ART. I.—*A Report of some Cases of Operation, treated for the most part in Addenbrooke's Hospital, Cambridge, and in the year* 1855. By GEORGE MURRAY HUMPHRY, Esq., Surgeon to the Hospital.—*London*, pp. 67.

A GROUP of such interesting and instructive cases it has rarely been our lot to find within limits so small as those of this unpretending little pamphlet. We congratulate its author on the valuable surgical experiences which have fallen to his share, and still more on the admirable manner in which he has turned them to account.

We will briefly notice the cases recorded in the order which they occupy in the pages before us. The report commences with cases of stricture, in which Mr. Syme's operation of external division was performed. These are three in number, and in each the results appear to have amply justified the course which was pursued. Believing, as we do, that this operation is rightly characterized by Mr. Humphry "as one of the greatest improvements in modern surgery," we hail with satisfaction a new illustration of the fact, that its merits are becoming appreciated by provincial surgeons, not on the ground of authority, but on that of successful experience.

Three cases of division of simple stricture of the rectum follow. The author believes that the successful issue of the proceeding is promoted by division of the sphincter ani at the same time, whether the complication of fistula is present or not.

Mr. Humphry is happy in being able to report two successful cases of amputation at the hip joint. The particulars of each will well repay the reader. An excision of the condyle of the lower jaw, for some morbid condition of that part involving enlargement, in a young woman, aged twenty-one years, is next recorded. Chronic rheumatic arthritis, analogous to that which is familiar to all surgeons as affecting the hip at middle and advancing life, is regarded as the cause. It is to be regretted that no account of any histological examination of the specimen is presented. The next section is headed "Three Cases of Excision of the Knee-Joint." In his remarks upon these operations, the author writes:

"Although it is a common thing for severe operations upon bones, such as those for necrosis, to be followed by very slight constitutional disturbance, I was nevertheless surprised, and I am sure other persons who witnessed these excisions of the knee-joint were also surprised, at the very slight amount of febrile or other unfavourable symptoms which ensued." (p. 31.)

We do not altogether concur in this feeling, because it must be observed that in no case was there any diseased action in the bones or soft parts entering into or surrounding the joint at the time of the operation. In case No. 2 it does not appear that the cavity of the joint had been affected by any chronic disease, the cause of impaired functions in the limb being an irretrievably damaged patella. These were examples of what has been called an "operation of convenience," their object being not to remove disease, but to promote osseous union between the femur and tibia, in order to render the limb a support to the trunk, instead of an inconvenient appendage, and so they very much resembled the procedure adopted when the two ends of an ununited fracture are sawn off to accomplish the same end. They were not performed for the removal of diseased epiphyses, or while the synovial membrane was in a state of active inflammation. In such cases, and it is in these that excision has been generally applied, the constitutional disturbance has been frequently considerable. The extent to which the object proposed has been attained in Mr. Humphry's cases will be most readily estimated by quotations from his own report. Of the first he writes, after twelve months from the operation had elapsed, "there was pretty firm osseous union between the bones, and every probability of her being able to walk upon the limb." (p. 28.) Of the second, nearly five months after the operation, "the wound had long been soundly healed, and the bones firmly united. He could raise the limb from the bed, and bear some weight upon it; could move about very well with crutches, and there was every probability of his being soon able to walk without them." (p. 30.) Of the third, "the healing of the wound was completed in little more than a month, and in about two months there was firm union between the bones." (p. 31.)

Our space will permit us only to enumerate the remaining cases. One of ovariotomy by the lesser section, and promising to be successful, but cut off on the twelfth day by tetanus; four of tracheotomy; two cases of encysted urinary calculus are given, in one of which the patient submitted to lithotrity thrice, to lithotomy four times by the lateral, and once by the urethro-rectal method, during a period of six years and three quarters; death occurred after the operation last-named, in which a cyst was opened, and the calculus removed from it. In the other case, lithotrity and lithotomy were each performed twice, and a stone successfully removed from its enveloping capsule, a vesical pouch. The details of these cases are extremely interesting, and are well worthy perusal as examples of judicious, persevering, and skilful efforts to deal with one of the most formidable difficulties which engage the powers and tax the resources of the operating surgeon.

ART. II.—*Climate, Weather, and Disease; being a Sketch of the Opinions of the most celebrated Ancient and Modern Writers with regard to the Influence of Climate and Weather in producing Disease.* By ALFRED HAVILAND, M.R.C.S., &c.—*London*, 1855. pp. 144.

THE purpose of this book is the development of a dominant notion which has obtained so large a place in the author's mind as to modify the aspect of every question discussed in its pages. According to him, climate is

the original after which not only man's physical, but his moral, nature is moulded. The climate of Eden was perfect; the sentence of woe pronounced on our first parents was inflicted by means of its deterioration. The herb of the field, which had before been endowed with endless life, became subject to decay; the resulting miasmata "brought death into the world and all our woe." Throughout the world's history, climate has exercised a no less powerful influence on the destinies of man; "in studying climate we study man;" by it "arts, sciences, ethics, religion, war and servitude are regulated" (p. 5); so that, in fact, climatology is synonymous with anthropology. The whole subject may be divided into two parts, ancient and modern. The former is taught in the aphorisms of Hippocrates, "a book filled with well-recorded facts, and some few obvious deductions from them" (p. 10); the latter "in that valuable epitome of knowledge, the Registrar-General's Reports." (p. 11.) The one represents the youth of science, "sportive, imaginative, impressionable;" the other her maturity, "grave, rigid, exact, and unpoetical." (p. 12.)

A chapter is devoted to each of the four constitutions of the seasons, which are described in the treatise on epidemics of Hippocrates; and much loose reasoning is employed in the attempt to show that the statistics of modern times serve to illustrate the truth of the doctrines of the ancient medicine. Of the chapters on the *κατάστασις λοιμώδης*, great part is devoted to the climate of Greece; under which heading many pages are occupied in the discussion of the Hippocratic doctrine regarding the influence of aspect and position on the diseases prevalent in a town or district. According to Mr. Haviland, the ancient Bœotia was like modern Ireland; the country was boggy, and the atmosphere "filled with cold and humid fogs." The ancient Bœotian resembled the modern Irishman, "dull and stupid"! The climate of Arcadia was inclement, and necessitated a constant struggle with the elements; the people, accustomed to a "vagabond and restless life," and "deficient in the first element of patriotism, the love of home," resembled the modern Swiss!

The most useful portion of the work is the series of tables exhibiting the comparative prevalence of certain diseases in London at different periods of the year, projected in curves, the time in weeks being taken for abscissæ, and the variables for ordinates. The diseases selected are phthisis, bronchitis, and pneumonia; small-pox, measles, scarlatina, and typhus; diarrhœa and cholera. In this way the influence of season and temperature on the prevalence of disease is presented to the eye at a glance, and in such a form as to be intelligible with the least possible amount of mental application.

The text is interspersed with numerous passages, not always either aptly or accurately quoted, from classical authors.

ART. III.—*Records of Obstetric Consultation Practice, and a Translation of Busch and Moser on Uterine Hæmorrhage, with Notes and Cases.* By EDWARD COPEMAN, M.D., F.R.C.S., Physician to the Norfolk and Norwich Hospital, &c.—*London*, 1856. pp. 223.

THIS volume, although unpretending in size and character, contains some practical papers which may be perused with advantage. They are re-

spectively upon Puerperal Fever—The Use of the Vectis—The Induction of Premature Labour—Puerperal Convulsions and Craniotomy. The essay on uterine hæmorrhage, although elaborate in its details, possesses, in our opinion, but little novelty or interest, and contains no reference to the latest researches of British obstetricians upon the subject. We shall therefore confine ourselves to a brief notice of the original papers, which in truth embody nearly all that the volume contains of the author's records of obstetric consultation practice.

The employment of turpentine in the treatment of puerperal fever is the principal point dwelt upon in the first paper. Twenty-one cases are reported in which it was given, and of these, fifteen were cured, and six died. The author appeals to these results as being strongly in favour of the medicine; and he is of opinion that there is no other which possesses so much power of controlling the disease. We should be sorry to discourage the use of a remedy so strongly recommended as this is by Dr. Copeman, but we are bound to state that a perusal of his cases has not satisfied us of the justice of the encomiums he has passed upon it. They were, for the most part, of a sporadic character, having been scattered over a period of five years, and were consequently devoid of that danger and intractability which are so common to the epidemic forms of the disease. Occurring, moreover, in private rather than in hospital practice, they cannot be said to have possessed its worst features; whilst many were of so mild a type, that we are persuaded that recovery might have been expected under ordinary treatment. Now, bearing in mind these facts, and that it has been calculated that of any given number of cases of the disease, however occurring, two-thirds, properly treated, may be expected to recover, which very nearly represents Dr. Copeman's success, —that in the hands of our best authorities, it has either signally failed, or occasioned much disappointment,—that of the six cases reported by Dr. Brennan, the original introducer and eulogist of the remedy, one-half died,—that Dr. Copland, whose testimony is so much relied upon by the author, combined with its administration large and repeated doses of calomel, camphor, and opium,—and that of twenty cases in which it was given by Dr. Clarke, not one recovered, we are compelled to dissent from the doctrine that it is possessed of special curative powers in regard to this disease. Not that we have any doubt as to its efficacy in various morbid states of the system which are common to the puerperal period. Thus, for the relief of intestinal irritation, which so frequently imitates or simulates the phenomena of puerperal fever, and also as an external counter-irritant or rubefacient, we know of nothing better; but regarded as an agent possessing specific curative powers, our experience is in accordance with that which we have quoted, and Dr. Copeman's cases do not induce us to alter our opinion respecting it.

The vectis, in the hands of our author, appears to have been a very useful instrument. He remarks that it is available in many instances in which the forceps would be inadmissible, and that as it is equally efficient with the forceps in cases where they may be used, its greater range of applicability (to say nothing of the greater facility with which it can be employed) is an argument in favour of its having its proper share of an accoucheur's attention, if not indeed the preference. Further on the author remarks, that as employed by him, the mechanical power of the

instrument is that of a hook, and that its principal moving power is that of traction. He enters very fully into a description of the instrument he employs, as well as its mode of application; and appends the histories of twenty-four cases, in which he appears to have used it with much success. We cannot here enter into a discussion of the comparative value of this instrument and the forceps, but may observe that we have always been led to consider them as adapted to two different purposes. If mère tractile power is required, we cannot doubt that the general opinion of the profession would be in favour of the forceps; but there is a class of cases occasionally met with in which this instrument is inadmissible, and here the vectis is eminently useful. We speak of malpositions of the head, in which the vertex is either arrested in its descent, or directed backwards instead of forwards. With the aid of the vectis we believe that these and other irregularities might be easily corrected, and thus the progress of the labour very materially assisted. In other words, we conceive that the vectis should be used for the purpose of rectification, and the forceps for extraction. We are well aware, however, that the frequent use of an instrument facilitates its employment, and therefore it is not improbable that, under certain circumstances, the vectis may become a convenient substitute for the forceps.

In inducing premature labour, Dr. Copeman recommends a proceeding not commonly practised, and one which, if it could be relied upon, has some advantages over those generally in use. It consists in passing an œsophagus bougie or tube a distance into the uterus, between its inner wall and the chorion. We need not insist upon the superiority of this proceeding over that of rupturing the membranes, or of its simplicity in comparison with the douche, or the introduction of sponge tents; and two cases appended speak favourably of its efficacy. We do not observe any particular novelty or points requiring notice in the papers on puerperal convulsions and craniotomy. The cases appear to have been treated upon established principles, and are chiefly interesting for their practical details; regarded as such, and as a truthful record of clinical experience, they are well worthy of perusal.

Art. IV.—*The Sanitary Condition of Paddington.* By Graily Hewitt, M.D. Lond., Fellow of the Royal Medical and Chirurgical Society, Physician to the Westminster General Dispensary, Lecturer on Comparative Anatomy and Zoology at St. Mary's Hospital Medical School, and one of the Registrars of the Hospital. Author of 'The Pathology of Hooping-cough.'—*London*, 1856. pp. 41.

To dislodge an enemy it will not suffice to take a general survey of his position, but it is necessary to attack every point that he occupies. A single battle, crowned with victory, may weaken him, and expose his entrenchments; but to secure permanent success and complete conquest, every advantage must be followed up, until no doubt as to the issue remains. So it is with the great social warfare waging against stinks and impurities of all kinds. We are beginning clearly to know the power of our enemy, and to estimate the extent of his fortifications; but although, to carry on the simile, certain pitched battles must be fought, a guerilla

warfare is equally necessary to expel him from all the dark lanes and filthy courts where he takes refuge.

Dr. Hewitt has followed up the question of sanitary reform as regards the parish of Paddington; and by reference to the Registrar-General's returns and other documents, shows that the laws that apply to towns at large, are equally true of their individual parts; that the health and longevity of the inhabitants stand in a direct ratio with the amount of sewerage, street ventilation, water supplies, and the like, to be found in their district. We conceive that to vestrymen and others interested in the welfare of the parish, Dr. Hewitt's little work must prove very instructive and suggestive.

ART. V.—*An Essay on Intermittent and Bilious Remittent Fevers, with their Pathological Relation to Ozone.* By E. S. GAILLARD, M.D.—*Charleston*, 1856. pp. 59.

THIS Essay has been published partly because, as the author informs us, "by those to whom were intrusted the scales of justice, it was awarded the Annual Premium of the Medical College of the State of South Carolina," "at the period of his graduation." It is the production of a clever student; but though it evinces extended and varied reading, it betrays so much hasty generalization, and so little close and independent observation, that we must differ from those of the author's friends whose "repeated requests" induced him to commit it to the press. Although comparatively a small portion of the treatise, the great feature is undoubtedly the proposition, that the presence or absence of ozone is the efficient element in the arrest or propagation of disease.

It would answer no useful purpose to follow Dr. Gaillard through his mazy arguments; a single quotation may suffice to show both the author's style and his mode of handling his subject. After adverting to the methods employed for generating and testing the presence of ozone, and theorizing on its power of destroying malaria, and, when in excess, of producing pulmonic disorders, he goes on to say:

"We are forced to make a reasonable deduction from the fact of ozone being found by tests, in a 'bottle partly filled with turpentine, and exposed to the action of light and air.' It has long been supposed that the pines of our southern country exerted a chemical effect upon the poison of malaria. This, for a long time vague, conjecture now finds, thus, an interesting corroboration. What more probable than that Nature should, in the magnitude and magnificence of her laboratory, produce the same principle that we, in our miniature efforts, have boasted to originate; or what better calculated to impress us with the happiest conceptions of Omniscience, than that these stupendous agents which produce it should be placed just where this purifying element is most needed and consumed? On reference to chemistry, we find the turpentine to be C^5H^4, and with this, the *now* interesting remark: 'Bottles in which rectified turpentine, not purposely rendered anhydrous, has been preserved, are often studded in the interior with groups of beautiful colourless prismatic crystals, which form *spontaneously;* these crystals contain $C^{20}H^{16}H^6O^6$, or four parts of turpentine with the incidental H^6O^6 *added.*' It needs but a glance to see that we have here the elements of ozone, though not in correct proportions."

It does not appear improbable that the numerous observations that are being made on the presence of ozone at different points and at different periods, may ultimately establish a definite ratio between the presence

of this agent and the existence of certain forms of disease; but we must decline adopting the conclusion at which Dr. Gaillard has arrived,—that we are in a position to found upon our present acquaintance with ozone a theory of the causation of fevers, or of any other class of maladies.

ART. VI.—*Ueber den Verlauf des Typhus unter dem Einflusse einer Methodischen Ventilation.* Von Dr. L. STROMEYER, General-Stabsarzt der Königlich Hannöverschen Armee.—*Hannover*, 1855. pp. 48.

On the Influence exerted by Systematic Ventilation upon the Course of Typhus. By Dr. STROMEYER, Physician-in-Chief to the Hanoverian Army.

ANYTHING coming from the pen of so distinguished an observer and author, and so practised a medico-chirurgeon as Dr. Stromeyer, is certain to command attention among his countrymen. We rejoice, therefore, that he has not thought it beneath his dignity to devote a pamphlet to the subject of ventilation in connexion with the treatment of disease. Ventilation, as a means of prevention and cure, has not yet secured that attention in Germany which it merits; possibly the vagaries that we have been led into in connexion with this subject may, in a measure, have served to deter the Germans from its proper investigation.

Dr. Strömeyer's object is to prove that a uniform current of air playing over a patient is the best curative agent we possess in typhus:

"I hope," he says, "to live long enough to see physicians, when called to a case of typhus, send first to a carpenter, and then to the apothecary, by which means they may obviate the necessity of requiring the services of the former as often as they now do, at the conclusion of their treatment."

Dr. Stromeyer accordingly relies more upon isolation of the patient, and a thorough and constant ventilation of his apartment, than upon any other proceeding. Dr. Stromeyer supports his views by a table of cases treated in his hospital, 77 of which were typhus (under which head he includes typhoid and relapsing fevers), 5 of which proved fatal. He remarks that, with the exception of one, who had an old pulmonary abscess, none would have proved fatal had his method of treatment been adopted earlier, and carried out more rigidly.

Our readers may be inclined to smile at this assertion, but Dr. Stromeyer evidently feels warmly on the matter; and for this reason, as well as on account of the real importance of the object he pursues, we may look over the somewhat dogmatic and peremptory tone that characterizes the pamphlet.

ART. VII.—*A Practical Treatise on the Diseases of the Testis, and of the Spermatic Cord and Scrotum.* By J. B. CURLING, F.R.S., Surgeon to the London Hospital, &c. &c. Second edition, revised and enlarged, with numerous wood engravings.

MR. CURLING's book has already taken its rank among the standard works of the day. The opinions which it contains have been, in various forms, so often before the public, that it will be necessary at present to notice only the improvements which have of late years been made in the treatment of some of the most important diseases of which it treats.

Instead of the ordinary operation for hydrocele, the tunica vaginalis may be emptied by a puncture made with a needle. By acupuncture, anasarca of the scrotum is substituted for a common hydrocele, and the effused fluid is subsequently removed by absorption.

After this operation, it has been observed that "the re-accumulation follows less quickly than after the fluid has been evacuated at once by a trocar, and in many cases does not take place at all." (p. 102.) Iodine injections for the permanent cure of this disease were first employed by Mr. Martin, in India, and have now come into very general use. "The apparatus for iodine injections is simpler and more portable than that which is required for other fluids." (p. 115.) It consists of a medium sized trocar, a half-ounce glass syringe with a metallic nozzle, and a small stop-cock adapted to the canula. The metallic parts are made of palladium. The injection employed by Mr. Curling consists of two scruples of iodine, half a drachm of iodide of potassium, and one ounce of spirits of wine. Two or three drachms of this is injected, and allowed to remain in the sac for five minutes. The fluid is then allowed to flow back through the canula, and about half a drachm is left in the sac.

In the treatment of orchitis, Dr. Fricke, of Hamburgh, first suggested the practice of *compression*. It may be applied both in the acute and chronic forms. The object is to afford support to the dilated vessels. This plan of treatment has lately been very generally adopted, but "when there is much effusion in the vaginal sac, strapping the tumour does not seem to act with much effect."

"Copaiba must not," according to Mr. Curling, "be employed so long as any active disease is going on in the testicle." Our experience in this respect is at variance with his. We have found that copaiba, in combination with sulphuric acid, allays the inflammation of the testicle, as well as that of the urethra, from which the affection of the testicle has commonly its origin. In chronic orchitis, Mr. Curling places his chief reliance upon mercury. "It is desirable to affect the gums slightly, and to keep the patient under the influence of the remedy until all swelling has subsided." (p. 259.) In syphilitic orchitis, mercury is still more evidently required. In tubercular disease of the testicle, on the other hand, mercury is generally prejudicial. (p. 291.) No kind of treatment, either local or general, is of service in cystic disease of the testicle. "The only means that can be adopted is the removal of the tumour." (p. 329.)

Of all the diseases of the spermatic cord, varicocele is by far the most common, and consequently the most important. The improvements that have been made in the treatment of this disease have also been of as great importance as any that have been noticed.

Sir B. Brodie performed the operation for varicocele in St. George's Hospital, by cutting the varicose cluster through its centre. The patient recovered satisfactorily. Sir E. Home cut down upon, and tied the spermatic veins: "Venous inflammation took place, attended with so much constitutional disturbance, that the patient nearly died." (p. 432.)

In order to avoid the risks consequent upon this operation, surgeons now generally pass a needle between the varicose vessels and the vas deferens, and include the former, together with the skin covering them, in a ligature, twisted in a figure of 8 round the extremities of the needle. The

adhesive inflammation only is excited by this proceeding, and the dangers arising from the decomposed and softened contents of the veins (so liable to accompany suppurative inflammation) prevented. The mode of performing this operation *subcutaneously*, however, possesses still greater advantages. This operation is performed in the following manner:

"The vas deferens being separated from the mass of veins, and the latter being pinched up with a fold of the scrotum, a needle set in a handle, with the eye near the point, armed with a double-looped thread, is passed beneath them. When the needle has traversed from one side to the other, the loop is to be drawn out, the needle retracted, and the veins let go, the skin alone being now held up. A second needle, similarly armed, is then to be passed through, over the veins, entering at the same hole by which the first needle was thrust out, and emerging by the same hole at which it entered. The second loop is next to be drawn out, and the needle withdrawn. The bundle of veins is thus included between two double threads, of which one passes over, and the other beneath it. The ends of the thread on each side are then to be passed into the loop of the other, and now, by drawing these ends in opposite directions, the vessels are tied beneath the skin. By this mode of applying the ligatures, the vessels may be either suddenly constricted or be tied gradually by means of a *serre-nœud*." (p. 434.)

This operation has the advantage of not including the skin in the ligature, which we regard as a very material point; for if the skin be included, a certain amount of unnecessary pain is inflicted; and what is of more importance, as soon as the skin begins to ulcerate, the ligature will become relaxed. The circulation through the compressed veins may then probably be restored, and portions of decomposed clot which may have been contained in the vessels, are liable to be carried in the course of the circulation.

In concluding this brief notice of Mr. Curling's work, we heartily recommend it to the notice of every practical surgeon.

ART. VIII.—*On the Nature of Cholera as a Guide to Treatment.* By WILLIAM SEDGWICK.—*London*, 1856. pp. 182.

THE object of this little book is to prove the proximate cause of cholera to consist in functional disorder of the sympathetic system, induced by some impression made upon it through the stomach. Mr. Sedgwick does not appear to claim the proposition as one originating with himself, as he refers to the writings of Dr. Hamilton Bell, Dr. Loder, and others, who have regarded the sympathetic as the part of the organism most immediately involved. Mr. Sedgwick seeks to establish the analogy of the symptoms of cholera collapse with those presented by circumstances accompanying a shock to the sympathetic nervous system, as in the case of a blow applied to the epigastrium, or in that of perforating ulcer of the stomach. He analyses the symptoms accompanying cholera collapse, and dwells more particularly upon the suppression of urine, upon the question of the formation of urea and its vicarious discharge by other organs than the kidneys, the altered condition of the blood, and the relation of the serous discharge to collapse. It is unnecessary to add, that the result of the analysis is entirely favourable to the author's views.

The labour bestowed upon the argument would alone inspire us with respect for the author; and though we are unable to appreciate some of the transcendentalism in which he indulges towards the end of the book,

we regard it as a useful contribution to medical literature. We think that those interested in the important question of the intimate nature of this disease, will find much that is suggestive in our author's ideas; much that is arranged in a new form; and much, probably, that will excite in them a spirit of controversy. For these various reasons, we advise all cholera students to peruse Mr. Sedgwick's volume.

ART. IX.—*A Practical Treatise on Vesicular Hydatids of the Uterus.* By WILLIAM HENRY ASHLEY, M.D., F.R.C.S. E.—*London*, 1856. 8vo. pp. 108.

DR. ASHLEY'S essay embraces the pathological chemistry, etiology, pathogeny, semeiology, prognosis, and treatment of uterine hydatids. It also presents us with a very carefully-compiled history of the opinions of many authors on the subject. The monograph may be read with advantage by those already acquainted with the disease; while to a practitioner who has not yet had any experience in such cases, it will afford considerable assistance.

ART. X.—1. *The Microscope, and its Application to Vegetable Anatomy and Physiology.* By Dr. HERMANN SCHACHT; edited by FREDERICK CURREY, M.A. Second Edition, considerably enlarged.—*London*, 1855. pp. 202.

2. *The Microscope; its History. Construction, and Applications.* By JABEZ HOGG, M.R.C.S., Assistant-Surgeon to the Royal Ophthalmic Hospital, Charing Cross. Second Edition. *London*, 1855. pp. 458.

THE first of these two books professes to be a work on the microscope by a distinguished foreign anatomist, whereas the only part of it which treats of the principal subject is not by Schacht, but by Mr. F. Currey. The enlargements by which this edition is distinguished from its predecessor are the following:—four chapters have been added at the beginning of the book. The first contains Elementary Instruction in Optics: the second is on the English Microscopes; the third on Subsidiary Apparatus; the fourth on the Preservation of Objects. The three last are substituted for chapters on similar subjects which form part of the original work. That the first production of an unknown author should appear as part of a translation, and should thus shelter itself under a distinguished name, is surely objectionable; nor do we think the evil at all removed, even though it may be mitigated, by the fact that it is done with the author's concurrence.

"There is no doubt," says Mr. Currey, "of the superiority of English instruments over those described by Dr. Schacht." This opinion, though opposed to the judgment of anatomists, is shared by our author with other microscopical amateurs; nor is it difficult to understand why this should be the case, if we reflect that while the anatomist regards the microscope merely as an instrument of research, and considers that best which best answers his purpose; the amateur inquires only whether it comes up to a certain standard of perfection, which to the other is of little value. Every one knows that English opticians have arrived at a

marvellous degree of proficiency in the construction of objectives capable of transmitting very divergent pencils of light, or, in other words, of large angular aperture. With such objectives the markings on the shells of certain species of Diatomaceæ can be resolved with a distinctness of definition which no French or German instrument is capable of affording. Much ingenuity has also been displayed in the complicated adaptations by the use of which the excellency of the objectives can alone be made available. Well may the English microscopist value himself on his instrument; well may he leave the coarser investigations to such men as Thuret and Quatrefages, who use the instruments of Nachet; to Tulasne, to Hofmeister, and scores of others great in the mysteries of nature, but uninitiated in those of large angular apertures. To the anatomist, the very perfections of our finest instruments diminish their utility. The eighty guinea microscope of Mr. Ross, to borrow an illustration from Dr. Carpenter, is no more suited to serve his purpose, than is the racer to serve those of the roadster or carriage horse. He has no time to go through a complicated course of adjustment, and even if he had, the result obtained would not be so satisfactory. However carefully the objective of large aperture is used, several disadvantages remain. The outlines of cells and other elements of tissue are so extremely faint as to be difficult to recognise; those points of the object only are distinguishable which are exactly in focus, the parts which are immediately above or below them being wholly invisible. We have not space for the further discussion of this question. After all, there is nothing which can be advanced in relation to it of such force as the indisputable fact, that while no research of any importance in anatomy has been made with the objectives under consideration, results of such immense value have been gained to science by instruments which the English microscopist despises, and considers unworthy even of description.

The second book at the head of this notice, though larger in bulk, is of less pretension than the other, and of more merit. It is intended for the uninitiated, to whom we cordially recommend it as a useful and trustworthy guide. It well deserves the popularity to which it has already attained.

Art. XI.—*A Manual for Midwives and Monthly Nurses. Dublin*, 1856. pp. 170.

The propriety of publishing a manual of midwifery for the use of midwives and monthly nurses may appear to many members of the profession rather questionable, so generally is it believed that "a little knowledge," which is all this volume proposes to give, "is a dangerous thing." But we think, on full consideration, it will be admitted that much may be said in its favour. It is obviously to the advantage of the profession, no less than that of the public, that the practice of the accoucheur should be assisted by competent rather than incompetent nurses—that the standard of obstetric knowledge possessed by the several midwives attached to our maternity institutions should be raised; and it is clearly in accordance with the spirit of the times that useful information should be diffused throughout all ranks of the community. Entertaining these views, and persuaded that the profession has nothing to fear from enlightened,

and everything to dread from ignorant, midwives and monthly nurses, we approve of the object of this manual, and are glad to find that it very adequately fulfils the purpose for which it was written. It contains a short description of the pelvis, restricted to an explanation of its more important anatomical and pathological characters—a rather full account of the phenomena of natural labour—good directions for the management of puerperal patients and infants, so far as the nurse's duties are concerned—several useful receipts for articles of diet or drink required in the sick room—and such information respecting the circumstances of difficult, protracted, tedious, and complex labours, as will place the nurse upon her guard, and show when she should seek promptly for the assistance of the accoucheur. The work contains some typographical errors, and a few equivocal idiomatic terms or expressions, which, in a future edition, we would recommend to be corrected and altered. These do not, however, detract from its practical value; but there is one direction given respecting the management of the breasts in the case of persons who do not suckle, which we strongly object to—viz., that "a little milk should be taken away occasionally by the child or by a breast-pump, just enough to relieve the sense of distension." Now, we are satisfied that this is not only an unnecessary, but a mischievous proceeding, and that, for the purpose in view, it is sufficient to restrict the diet, give an occasional cooling aperient, gently rub the breasts with sweet oil when tense, and foment them when painful. This was the practice pursued by William Hunter with almost invariable success at a time when few fashionable people were accustomed to suckle their children; and following the precedent, we have adopted the same practice with equally satisfactory results.

ART. XII.—*Obscure Nervous Diseases Popularly Explained. Being Six Letters, addressed to a Physician, on the many Nervous Affections resulting from Nervous Irritation and other Sources of Reflex Nervous Disturbance.* By J. L. LEVISON.—*London*, 1856. pp. 114.

ALTHOUGH no period of life is equally prone to reflex irritation proceeding from the teeth as early infancy, still, cases occur to every medical man in which, at later dates, anomalous and painful affections are found to result from caries, exostosis, mal-position, or other morbid conditions of these organs. Cases of dyspepsia and facial neuralgia from this cause are familiar to us. Mr. Levison communicates several instances of nervous affections, of a peculiar character, not ordinarily the consequence of dental derangements, which were shown to depend upon that cause. Among the instances given by the author, we may mention a well-marked case of chorea, consisting mainly in convulsive spasms of one side of the neck; another of partial paralysis of one side of the body; another of ptosis; one of sleeplessness, in which cure was obtained by improving the state of the teeth. The subject is one of great interest, and we thank Mr. Levison for the information he affords us. We should be glad to welcome the appearance of a more comprehensive work than the one before us, embracing all the various phases of dental irritation, and addressed, moreover, solely to the medical profession.

PART THIRD.

Original Communications.

ART. I.

On Ulcer of the Stomach. By WILLIAM BRINTON, M.D., Fellow of the Royal College of Physicians, Lecturer on Physiology in St. Thomas's Hospital, Physician to the Royal Free Hospital.

IN a former number of this Review (Jan. 1856), I attempted to sum up the information respecting the Pathology of the Gastric Ulcer, that I had deduced from the personal inspection or the records of about a thousand necropsies, in which this lesion had been found to be present.

In the following pages I propose to view the same malady from another aspect; and to analyse, as briefly as possible, the symptoms by which it is usually announced in the living subject. By doing so, I hope to illustrate many of the points alluded to in my former essay, if not to add some interesting details to what is already known respecting the diagnosis of this disease.

However careful and accurate an observer of natural phenomena any one may be, his statements can have little value unless they are accompanied by specific information as to his opportunities and means of observing, their respective extent and delicacy, and the method by which he uses them. For want of such details, we constantly find remarkable discrepancies in the experience of two or more different observers, even where the subjects and instruments of research are such as involve but few and simple conditions of experiment. And when we turn from the simpler physical sciences to one which, like medicine, is not only more complex, but involves mysterious elements of life and disease that our existing knowledge can hardly be said to appreciate, we find such discrepancies far more numerous and prominent. In short, the conditions of experiment are so multiplied, that all strict comparison is impossible.

This proposition will explain the limited use which I have made, in the following pages, of the hundreds of cases of ulcer of the stomach which I have collected from different sources. As records of disease, based on the observations of an almost equal number of independent authorities, they often afford us positive evidence of the most valuable kind; especially when we consider that, in every one of them, the significance of the symptoms noticed during life as evidence of the malady, has been certified by the subsequent necropsy. But though their more prominent features offer us what are sometimes very instructive contrasts, yet we cannot jus-

tifiably lay much stress on their minor differences. And any negative evidence we might extract from them would be all but useless. In other words, the absence of all mention of this or that particular circumstance, from any one of these records, constitutes no valid grounds for concluding it to have been really absent from the corresponding case;—an inference to this effect being only allowable where the narrative itself either distinctly expresses the fact, or as distinctly implies it.

But in calling attention to the imperfections of these records, I have no wish to exalt the comparative value of my own clinical researches. On the contrary, I would warn the reader, that equally grave defects are inherent to the observations of any single inquirer.

The successful clinical study of this malady (as of most others) requires the observation and comparison of a great number of cases, at short intervals of time. Such a requirement is best supplied by the out-patient practice of a large hospital. For the class of Hospital patients corresponds, in general, to that portion of the community most obnoxious to the gastric ulcer; which seems to fall with disproportionate severity and frequency on those who suffer from the ills implied by penury in this metropolis—excessive toil, insufficient and unwholesome food, foul air, mental anxiety, and those habits of intemperance which are the effect as well as the cause of such misery. But the disease generally has so chronic and sub-acute a character, that the sufferer rarely seeks and obtains admission as an In-patient, except where the hæmorrhage or peritonitis that occurs in its course immediately threatens life.

But Out-patient practice, however conscientiously conducted, offers some features which must not be forgotten when we make it a means of medical research. With opportunities for the most sedulous and minute study of symptoms in a large number of cases, it is often a very treacherous index of their course and termination. The severer examples of disease we may draft into the wards of the Hospital, and thus assure ourselves respecting the progress of the symptoms; and in the event of death, of their exact significance as revealed by the necropsy. But in other instances (which in this particular malady form the majority) the patient proceeds gradually towards recovery from the very beginning of the medical treatment; and in doing so, he often ceases to attend, without any previous intimation, at a variable stage of the cure, such as seems to him to justify his dispensing with further medical advice. The physician is thus left in complete uncertainty as to whether the symptoms disappear or return; and in the latter case, what modifications they offer by comparison with those previously present, and in what way they terminate.

When these facts are taken into consideration, and especially when we remember that, in a large proportion of gastric ulcers, the diagnosis remains somewhat uncertain during a long period of their existence, it becomes evident with what reserve and caution we must receive the most accurate observation of symptoms only. Adding to this uncertainty the bias that even accurate and honest observers often seem to acquire by the study of a special malady, my readers will probably not think me too scrupulous if I regard the majority of cases not confirmed by necropsy as of little service for the study of symptoms, except by careful comparison with others in which this verification has been afforded us.

The statements in the following pages may therefore be regarded as derived chiefly from two sources:—1. The records of some hundreds of cases, affording in many instances little more than an outline of the chief symptoms present, but always verified by careful necropsy. 2. The personal study of about one hundred and fifty cases, affording minute details respecting symptoms, but only verified by necropsy in a small proportion of that number.*

The statements obtained from these two sources exhibit a close agreement with each other. Their discrepancies are, indeed, chiefly negative: in other words, are for the most part explicable by the omissions inevitable in many of these brief records. But whether they are exclusively so, I must for the present decline to decide; and venture to indicate, as the most interesting question for future researches to answer respecting this malady—"How far can the symptoms of gastric ulcer vary from their ordinary formula; or, what anomalies of these symptoms, as carefully observed and recorded during life, are compatible with the presence of this lesion, as proved by an examination after death?"†

In what we may regard as typical cases, the history of the ulcer of the stomach is made up of the following succession of symptoms:—The malady is announced by disturbances of gastric digestion; at first, mere uneasiness and pain in the epigastrium; then nausea and vomiting, or regurgitation, that expel the food previously taken, or a tasteless or acid watery secretion. At this stage of the disease, it is sometimes cut short by the occurrence of perforation, with its sequel of fatal peritonitis. Failing such an accident, the dyspeptic symptoms are next complicated by hæmorrhage from the stomach; sometimes a sudden and dangerous gush, oftener a slow and intermittent drain, of blood. The anæmia produced by this hæmorrhage is generally associated with a cachexia which seems to be essentially independent of it; being chiefly the result of the inanition necessarily implied by frequent vomiting of the food, or by large destruction of the gastric mucous membrane, and consequent impairment of its function. In young females, another symptom is often present, in the form of more or less complete amenorrhœa, which may be associated with either of these two states of anæmia or cachexia; in other words, may be connected with ulceration, with hæmorrhage, or with both. The gradual acquisition of all these symptoms conducts the disease, in a variable period of time, to a climax, from whence we may next briefly trace it towards its termination. Retaining the liabilities to death by perforation, by hæmorrhage, by vomiting, and by exhaustion, which the above organic results of ulceration severally imply, the lesion often ends by one of these modes of dying, or by two or more of them in combination. In other cases, a spontaneous subsidence of these symptoms, in something like the inverse order of their occurrence, announces a recovery; or a similar amendment is only effected by careful medical treatment, such as quite entitles us to dignify it by the term of a cure. In less numerous instances, these symptoms continue with what is (for obvious reasons) rarely more

* The reader who is desirous of referring to examples of these cases themselves, will find several recorded in the Association Journal for the past and present year.

† Further allusions to the same point will be found in a subsequent part of this Essay (p. 179).

than a moderate intensity, during a variable period of life; in the course of which their uniformity is from time to time varied by considerable fluctuations of severity. The remissions which form one extreme of such fluctuations often sometimes merge into intermissions so complete, that we are left in doubt whether the process of ulceration has been merely reduced to a stand-still, or has broken out afresh after the cicatrization of the lesion. In any case, the protraction of these symptoms during many years of life gradually complicates the impairment of nutrition they produce, with that naturally resulting from the approach of old age; mingled with which they then constitute an indirect or conditionating cause of death, the influence of which it seems scarcely possible to estimate with any exactness.

But the symptoms just enumerated vary so remarkably in different cases, that each of them demands a separate study.

Pain, which is usually the first in the order of occurrence, is also the most frequent and characteristic of them all. Indeed, we may doubt whether it is ever absent from the whole progress of any case. For though there seem to have been instances of gastric ulcer, fatal by perforation, in which no pain was complained of prior to the attack that marked this event, still it is obvious that we have no right to presume the absence of so common a symptom as pain in the region of the stomach, merely because a patient has failed to speak of it at the time, or has not referred to it during a brief and agonizing illness. But since, in one or two cases of open ulcer in my own practice, the pain has completely intermitted for several days at a time, shortly before the occurrence of death by exhaustion, we may regard it as just possible that this symptom might be absent during the few days that would sometimes include the whole course of the disease, in cases of rapid perforation.

The character of the pain is peculiar. Rarely or never does the sufferer describe it as* lancinating, stabbing, or stitching. In the earliest stage of the disease, it is little more than a feeling of weight,† sometimes a tightness, giving the patient an impression as though the food experienced a stoppage in his epigastric region. Retaining these dull and continuous characters, it then gradually becomes intensified into a burning sensation, and at last into a gnawing pain, that produces a kind of sickening depression, which is quite distinct from the nausea often associated with it.

The date of its access is also characteristic. In a vast majority of cases it comes on from two to ten minutes after the ingestion of food, and remains during the one or two hours which correspond to the period of gastric digestion, at the close of which act it gradually subsides and disappears. And when, as is often the case, it is accompanied by vomiting, it almost invariably ceases as soon as this act has emptied the stomach of its contents. In some instances, however, the pain follows deglutition immediately, instead of being preceded by the usual interval of a few minutes. In these cases there is a presumption that the cardiac extremity of the stomach is the site of the lesion: a conjecture which is of course strengthened by embarrassment in the act of swallowing, such as

* The character would often distinguish the pain that attends scirrhus of the stomach.
† These epithets are all derived from the descriptions given by patients themselves.

suggests its close proximity to the œsophagus.* In some instances, the pain imitates that of an ordinary form of dyspepsia, in only coming on half an hour, an hour, or more, after eating.† Lastly, in what are generally either large lesions or protracted cases—often both—the pain loses the above character, becoming continuous during the intervals of the meals, and lasting days or even weeks without any intermission; or it even occurs chiefly on an empty stomach, and is alleviated by the ingestion of food.

The situation of the pain forms another of its characteristics. The place of its earliest appearance and greatest intensity, and to which it often remains strictly limited, corresponds to the centre of the epigastrium, or to the median line of the belly immediately below the free extremity of the ensiform cartilage. The portion of the epigastric region to which the pain is referred, forms a circular area of rarely more than two inches diameter,—sometimes, indeed, a mere spot of less than half this size.

But there are certain exceptions to the above rule. One of these, which is generally offered in the female, is apparent rather than real, and is due to that change in the situation of the cartilages of the ribs which is effected by the compression of stays, and which materially deepens the epigastric region in the vertical direction. In other instances, the pain is behind the ensiform cartilage instead of below it, or occupies the boundary of the epigastric and umbilical regions, instead of its usual site in the middle of the former. Finally, the pain sometimes lies a little to the right or left of the median line; or extends from a point of greatest intensity here towards either hypochondrium; or, in still rarer instances, is chiefly referred to the latter situation.

In some instances, the pain in the epigastrium is associated with a feeling of violent pulsation or throbbing; in other cases, the same sensation is felt, independently of the paroxysm of pain, which it may even replace. It appears to be analogous to the throbbing of an abscess, and cannot be recognised by any external examination.

The dorsal pain, first described by Cruveilhier, constitutes almost as important a symptom of the gastric ulcer. As far as my experience goes, it generally comes on a few weeks or months later than the epigastric pain, and from this time forth is quite as constant and characteristic, if not as distressing. It is almost always felt as a gnawing pain, which, ranging in its vertical position from the spine of the eighth or ninth dorsal to that of the first or second lumbar vertebra, is usually "interscapular" as well as "rachidian."

Like the epigastric pain, it has a fixed seat, generally remaining at or near the spot of its first appearance during the whole progress of the disease. Like it, also, there are lateral as well as vertical deviations from its ordinary situation. But I do not think I have ever seen these remove it to a greater distance than one or two inches from the median line,—indeed, scarcely ever more than a single inch. Its worst attacks generally alternate—rarely coincide—with those of the epigastric pain.

* One or two instances of this kind have occurred in my own practice. One is also detailed in the Wien Med. Wochenschrift for 1854, No. 51.

† An instance of this kind was lately brought before the Pathological Society by me; and is mentioned in the Lancet for Nov. 24, 1855.

How far the vertical deviation of the epigastric or spinal pain entitles us to conjecture a corresponding situation for the gastric ulcer, I am unable to decide. But one or two cases on record indicate such a connexion:—pain in the umbilical region being, for example, associated with an ulcer of the greater curvature. But with respect to the horizontal deviations of these pains, there is good reason for asserting, that where they are marked, they justify us in inferring a similar situation for the ulcer. The records that I have collected furnish me with about twenty instances of this kind: out of which about fifteen exemplify the concurrence of pain in the left hypochondrium with an ulcer of the cardiac extremity of the stomach; and four or five illustrate the same connexion between the right hypochondrium and the pyloric extremity of the organ. My own practice has also afforded me three or four cases, in which a similar deviation led me to predict the cardiac or pyloric situation of the ulcer during life. But I have found that the very local character of the dorsal pain often makes it a better test than the comparatively more diffuse epigastric pain. While I need scarcely add that the coincidence of the two in respect to this deviation is a far stronger testimony than either, unsupported, can afford; and that even this agreement requires to be confirmed by the presumption which may be derived from the other characters of this symptom.

Among the latter, we may first allude to the effect of pressure in increasing the pain. This is indeed a very important test, being one which, to speak with logical accuracy, converts what may be, for aught we know, a subjective sensitive phenomenon, into an objective one, that constitutes a far more trustworthy indication of local disease. To use the expression which generally suggests itself to the sufferer, there is a soreness as well as a pain: the least pressure in the epigastrium is sometimes unbearable; the patient, if a female, is even content to forego the fancied advantages of her stays, rather than endure the pain the central piece of whalebone in these ingenious aids to disease often produces. In the majority of instances, the soreness is exactly limited to the part of the epigastric region already specified. As it is produced by the more or less direct application of pressure to the diseased structures, it is not to be excited by pressure on the unyielding spine. But, in general, one and the same pressure on the epigastrium will excite both epigastric and spinal pain: sometimes even the latter, chiefly or exclusively.

Of course it is essential to use such a graduated pressure as shall not involve parts more distant than the stomach: a pressure, in short, scarcely exceeding that with which we manipulate the belly in cases of suspected peritonitis or colic. I say this because it would otherwise be possible to make strange errors. Thus I have known cases of mere emphysema and bronchitis, in which deep epigastric pressure caused considerable distress (easily mistakable for soreness), apparently from embarrassing the heart, which had gradually been forced down into the upper part of this region. It is not altogether superfluous to add another caution with respect to the above test. Not only must it be applied with great care and delicacy in the first examination of a supposed case of gastric ulcer, but, as a rule, we can scarcely be too reluctant to repeat it, even to verify an expected amendment. At any rate, its effects are sometimes so injurious, that

it is necessary strictly to prohibit the patient from all manipulation of the epigastric region, as well as from all pressure producible by dress (such as stays in the female) or calling (as is the case with shoemakers).

Whether the pain of a gastric ulcer is always increased by pressure, it seems impossible to decide. There is only one necropsy* on record—and this probably not of a true or spontaneous ulcer—in which it is distinctly specified that pressure was altogether devoid of such an effect. But I have once or twice met with cases which so nearly approached the symptoms of this lesion in all other respects, that I have been obliged to suspect its presence. The varying degrees in which pressure affects the patient in different cases, somewhat confirm such a suspicion, and indicate that even this characteristic of the ulcer is occasionally (though very rarely) absent.

A curious instance lately fell under my notice, in which pressure on the base of the ensiform cartilage relieved the patient from the sense of stoppage, and the dull epigastric pain, which came on soon after eating. Even here, however, there was a circular area, about an inch below the apex of this cartilage, very moderate pressure in which brought on severe epigastric and spinal pain. And after the artificial diminution of the epigastric pain procured by such pressure, the dorsal pain soon became much more violent.

The effect of posture on the pain in different cases is more variable. As a rule, a severe paroxysm is relieved by the recumbent posture, no matter what may be the situation of the ulcer in the stomach. But the varieties of the recumbent posture—or, to speak technically, the *decubitus*—will often have no influence whatever in increasing or diminishing the pain. But in other instances it will afford a valuable confirmation to our diagnosis, and may sometimes even entitle us to conjecture the exact seat of the lesion.

The facts upon which this statement is based are the following:—Of the cases witnessed by myself, in which the symptoms have led me to diagnose an ulcer of the stomach, I have found about two-thirds exhibit a marked influence of posture on the pain. During a paroxysm some were obliged to lie prone, some supine; some on the right side, some on the left, some were even obliged to sit up. I say obliged to do so, including in this phrase equally those cases which were distinctly relieved by the selection of a particular attitude, and those which experienced a great increase of pain by adopting any other. In some of them, however, the painful posture could be borne for a minute or two, until the gradual increase in the severity of the pain forced their abandonment. In like manner, the less painful attitudes had generally been adopted, to the complete exclusion of that habitual decubitus which most persons naturally assume during sleep. The remaining third of my cases offered no peculiarity of decubitus; though in many of them the pain was relieved by rest in the recumbent posture.

The fewer cases in which the effect of posture upon the symptoms during life was compared with the appearances seen after death, have

* Archives Générales de Médecine, 1823, vol. xx. p. 212. A soldier, aged twenty-three, died after twelve days' gastritis. Two ulcers were found.

afforded me more specific information to the same effect.* That is, the first (and probably the larger) group shows a pretty close correspondence between the posture adopted and the site of the ulcer—the prone decubitus being associated with an ulcer of the posterior surface of the stomach; the supine with one of the anterior surface; the decubitus on the right or left side, with a lesion of the cardiac or pyloric end of the organ respectively. But, on the other hand, in some of the very cases where we might have best expected such a connexion, I have found it fail altogether; a large chronic ulcer, exclusively limited to the pyloric pouch, having been associated with no change of an habitual decubitus on the right side; and an ulcer of the posterior surface, or of the small curvature, having been relieved by even the supine variety of the recumbent attitude.†

Such a correspondence in certain cases, and its absence in others, naturally remind one of what may be called objective and subjective sensations in the normal action of nerves, and suggest the analogous distinction of objective and subjective pains in their abnormal states of activity. But such a distinction fades before any philosophical inquiry. Even in the commonest forms of irritation of a nerve, most of the minute mechanical conditions remain unknown to us: and yet until we can specify and determine these, we cannot assert that a given pain is not so far objective, as that it results from a local lesion of the nerve. In like manner, a careful consideration would probably conduct us to the rather startling propositions, that all pain is subjective; that nothing but an elaborate organization at its periphery and centre enables any nerve to give an objective sensation; and that, even then, its objectivity is, strictly speaking, but very partial and imperfect.

The partially subjective character of the pain in gastric ulcer receives a good illustration from the manner in which it is often affected by mental changes. Amongst these we may specially allude to the depressing passions of sudden fear, anxiety, or anger, as frequently bringing on a paroxysm of pain, the severity and duration of which exceed those of the attacks produced by distension of the stomach with food. Here, however, the situation and character of the pain generally remain unchanged.

The effect of movement upon the pain closely corresponds to that of posture. As a rule, all violent bodily exertion is likely to be followed by an attack. While even the moderate exertion implied in walking, sustained so as to produce fatigue, generally brings about the same effect. And there can be little doubt that the relief generally afforded by the recumbent attitude is in great part due to the perfect rest it implies. In some instances which have fallen under my notice, the movements of locomotion have given rise to a peculiar sensation of dragging in the

* Priority in observing this interesting connexion belongs to Dr. Osborne, of Dublin, whose other clinical remarks on the gastric ulcer (Dublin Journal of Medicine, vol. xxvii. p. 361) will well repay the reader.

† Even in these cases, however, we may probably distinguish that the decubitus fails to guide our conjectures, rather than absolutely guides them wrong. For the fact that the habitual decubitus remains unaltered, deprives us of all grounds for any inference. Again, in the absence of specific cause to the contrary, we might well expect that the efficacy of the recumbent posture in relieving the pain would be shared by all the varieties of this posture.

right hypochondrium, such as induced me to suspect adhesion of the stomach to the liver. In one of these instances I have been able to verify this conjecture by a necropsy.

With this effect of adhesion I may mention another, that well illustrates the accuracy of an old observation respecting the symptomatology of the liver. In two or three cases, the adhesive inflammation uniting the ulcerated stomach to the surface of the liver, has been accompanied by that pain in the right shoulder which has long been regarded as characteristic of (superficial?) hepatic inflammation.

Lastly, the pain is affected in a special manner by various kinds of food. As already mentioned, its worst access or paroxysm generally has a close (though not exact) correspondence with that period of gastric digestion during which the organ is most distended with food. It is also increased by the ingestion of hard or indigestible substances; and mitigated by a pulpy milk diet. There are also many articles of food which have an irritating effect quite independently of their consistence. Amongst liquids, few are more generally unbearable than ordinary tea and beer. Finally, all hot substances are usually productive of great pain.

But exceptions to all these rules are occasionally seen. The pain is sometimes unconnected with the ingestion of food; sometimes relieved by it. And I have known even brandy, or hot water, to be taken by a patient with this object. While careful inquiry has satisfied me that beer is sometimes (especially in the aged) very well borne by the stomach, and is advantageous to the organism generally.

The *vomiting*, which forms the next symptom of gastric ulcer, is far from exhibiting such specific characters as those just affirmed of the pain which generally precedes it. It usually occurs when the paroxysm of pain has reached its greatest height; forming in this respect the crisis of the attack. Though generally preceded by a few efforts or retchings, it is rarely of a violent character; indeed, the distension of the stomach which prevails at this time, suffices to render it a very easy and painless variety of sickness. Once begun, it rarely ends without completely evacuating the stomach of its alimentary contents:—an act of expulsion which usually affords complete relief to the pain, but sometimes leaves a slight burning sensation, that only disappears after an interval of two or three minutes.

The chief varieties of the vomiting relate to the following details. Firstly, as regards the date of the malady marked by its access. Though generally preceded by the characteristic pain during several weeks, it sometimes comes on much earlier; occasionally almost as soon as the pain itself. In respect to the nature of the substances vomited, these vary chiefly with the precise date of this act. Soon after the ingestion of food they are of course alimentary; at a later period they have an acid character, which is often an intensely sour taste to the patient himself; and still later, are sometimes mixed with bile. Lastly, in those rarer instances in which the act of vomiting comes on quite independently of the ingestion of food:—for example, shortly after rising from a night's sleep—it expels a glairy alkaline fluid, that consists chiefly of the saliva swallowed prior to the attack. In the latter instance, the vomiting (which is often periodic) is frequently connected with habitual drunkenness; especially with the collapse that follows a debauch.

As regards the frequency with which this symptom is present in cases of gastric ulcer, I have no exact and trustworthy estimate to offer. My own experience would induce me to believe that it occurs in the majority of instances, and is rarely or never absent from the whole course of the malady, except in the rapidly perforating ulcer of the young female. The records I have collected afford one example of its absence in an ulcer that remained active during four years. In other cases it has been only represented by slight nausea. In others it has been limited to a single attack, or to the very close of the disease. Such evidence is indeed confirmed by the well-known effect of a strict regimen in alleviating this symptom, which in many cases only comes on after a full meal, and is at once suppressed by reducing the food to a minimum of the most unirritating substances. Among other circumstances that favour the access of vomiting, there is but one which seems to have any very close and constant relation to its frequency and intensity—namely, the size of the ulcer. This is, however, often connected with a long duration of the lesion, and with its intimate adhesion to neighbouring organs: two characters which might independently favour the occurrence of vomiting. The danger of this symptom it is difficult to exaggerate. There can be no doubt that it is the immediate cause of a considerable proportion of the deaths from gastric ulcer; indeed, my own experience entitles me to predict that more exact information will hereafter exhibit the mortality thus produced as one which far exceeds that brought about by hæmorrhage, and approaches the large per-centage due to perforation.*

It is not difficult to explain the dangerous character of this vomiting. By expelling the greater part of the food shortly after its reception into the stomach, it starves the patient with a rapidity that will be determined chiefly by the quickness of its access, and the completeness with which it empties the organ. And in addition to the effects of inanition, it adds the fatigue implied by such violent and abnormal action of the nervous and muscular systems.

The next symptom of the gastric ulcer is *hæmorrhage.*† Since the process of ulceration itself implies a solution of continuity‡ in the coats of some of the vessels of the stomach, nothing short of a simultaneous

* Compare the author's Essay in the January number of this Review, p. 180.

† I have purposely avoided the use of the word *hæmatemesis* in speaking of hæmorrhage as a symptom of gastric ulcer. One chief reason for my doing so has been the fact (till now too little noticed in the history of this malady), that blood effused into the stomach often escapes being vomited thence, and can only be detected in the fæces. And as it would seem pedantic to burthen our professional vocabulary with the addition of some such word as *hæmatokopræsis* to express the latter (and even more frequent) of these two results of hæmorrhage into the stomach, I prefer to lay the chief stress on the pathological occurrence, rather than on its obvious symptomatic consequences. There are still more valid objections to the use of the term *melæna*, which, though bequeathed us by Hippocrates, implies both vomiting and purging, and expressly connotes a peculiarity of the matters thus expelled from the alimentary canal—namely, their black colour—which is neither constant nor essential.

‡ With respect to the occurrence of "hæmorrhage by exhalation," through the coats of the vessels, apart from any solution of their continuity, little need be said. "We now know that this doctrine is incorrect; that the walls of even the finest capillaries have no pores of appreciable magnitude, such as would be necessary for the transit of blood-corpuscles; and hence that the extravasation of these structures is a proof that some bloodvessel has been ruptured. That amongst the myriads of these minute tubes present we often fail to detect the exact seat of the lesion, need of course little surprise us."—[Art., Stomach and Intestine, by the author, in the Cyclopædia of Anatomy.]

obliteration of these tubes can prevent some effusion of their contents. And hence it is not very surprising to find that the myriads of such tubes concerned in every lesion are rarely occluded with that quickness, precision, and universality, which would be requisite to suppress all hæmorrhage from their interior.

How far the hæmorrhage which occurs in ulcer of the stomach may be attributed to mere congestion, it is impossible to determine. But from the analogy of this lesion to ulcers seated elsewhere, we may fairly presume that the same degree of congestion which generally attends ulceration might constitute an efficient immediate cause of the bleeding. Still, as the hæmorrhage almost invariably occurs soon after a meal, and is often distinctly traceable to the ingestion of an unusual quantity of food, we are left in doubt whether the influence of this inflammatory congestion is not far surpassed by that afflux of blood which attends the act of gastric digestion, as well as by the mechanical disturbance which distension of the stomach would necessarily inflict on the diseased vessels that occupy the ulcer itself.

In my former essay I have alluded to the varieties of hæmorrhage in respect to their source: and have classified them as coming from the minute vessels of the gastric coats; from the chief branches or trunks of the arteries which run in the sub-serous areolar tissue external to the stomach, before being distributed to these coats; and lastly, from the vessels that occupy the parenchyma of the adjacent liver, spleen, and pancreas, and become involved in the ulceration after the stomach has contracted adhesions to one or other of these organs. I have also noticed the relation of these varieties to the depth—and therefore, other things being equal, to the date and duration—of the ulcer, as well as to the amount and appearances of the blood effused.

There are no data for determining the frequency of those scanty hæmorrhages which are poured out, in the earliest stages of the ulcerative process, from the minute vessels of the mucous membrane, and its sub-mucous areolar tissue. But it is certain that they occur in a large majority of cases. And it is probable that they are present in numberless instances, in which no symptom reveals them. For since a small quantity of blood does not excite vomiting, it depends entirely upon a casual coincidence of these two symptoms—hæmorrhage and vomiting—whether the former is revealed by the latter. And unless the attention of the patient be particularly directed to the examination of his stools, a moderate quantity of blood may also leave the intestinal canal by this natural channel, without ever being detected.

In all cases of this kind, the blood undergoes the ordinary changes which attend its exposure to the action of the fluids of the stomach and intestine. The moderate quantity of blood generally poured out not only becomes mingled with the various ingesta and secretions which may chance to be present, but gradually undergoes a kind of digestive process, that has the effect of greatly modifying its colour and consistence. Wherever the extravasated blood has been sufficiently exposed to this action, it will be found to have acquired a dark, grumous, or even black colour, and a peculiar tarry or almost pultaceous consistence. A small quantity of blood thus altered by digestion sometimes even simulates the colour and appearance of inspissated bile.

Hence the following precautions are often necessary in respect to this important symptom of gastric ulcer. We must never presume its absence because the patient has failed to notice it. Our inquiries must be directed equally to the matters vomited and to the stools. As regards the former, we must question the patient, not only as to what he may have recognised as blood, but as to the character of all the substances he has vomited. And the matters habitually rejected from the stomach should be submitted to a strict and repeated microscopic examination; care being taken to select such specimens as are either free from all admixture of food, or at any rate from animal food containing blood-corpuscles. Precautions of this kind will often show that a comparatively clear fluid deposits a sediment containing blood-corpuscles in considerable quantity; and perhaps ranges in other specimens from the same patient, through a brownish ropy mucus, to a grumous fluid having the ordinary "coffee-grounds" appearance of blood thus altered by digestion. A similar examination will sometimes be useful in the case of the blackened fæcal evacuations to which gastric hæmorrhage gives rise. Dilution with water will generally distinguish inspissated bile. But if not, the microscope will at once set the question at rest. The ingestion of the salts of iron is a source of error that may of course be easily detected by inquiry; though I have known the inky vomiting which has accidentally followed the administration of this drug immediately after tea, excite considerable alarm in the mind of a patient and his medical attendant.

The proportionate frequency of those larger hæmorrhages which are due to the vessels external to the stomach becoming involved in the ulceration, is just as uncertain. But from my own experience I should conjecture that they occur in not more than one-third of the gastric ulcers which come before us in ordinary practice. And I have elsewhere adduced reasons for supposing that they are fatal in from three to five per cent. of the whole number of these lesions which the most sedulous examination can detect in the dead body.

The symptoms of such hæmorrhages illustrate and confirm the proposition implied above—namely, that the blood poured out from the ulcer into the stomach scarcely exerts any specific action as an emetic or a purgative, but seems to excite vomiting or diarrhœa chiefly by its quantity; in other words, by the mechanical stimulus which its distension of these segments of the intestinal tube implies. Soon after a meal, the patient begins to experience an unusual fulness and weight in the region of the stomach; attended (sometimes even preceded) by feelings of syncope. Nausea rapidly supervenes, and ends in the vomiting of a large quantity of blood; which may either be partially coagulated, or, if rapidly effused and rejected, may retain a colour and fluidity that testify to the arterial character of its source. In other cases (and I am disposed to conjecture, chiefly in those rarer instances where the hæmorrhage, besides being less considerable in quantity, occurs independently of the meal time) the blood is effused in considerable quantity without exciting any vomiting whatever; and is passed at once, through the pylorus, into the intestine, which it leaves more or less rapidly with the stools. Lastly, in very exceptional cases, the rapidity of the hæmorrhage is so great that it distends the stomach, and more or less the intestine, with a single gush; and the

patient faints and dies before any expulsive act can take place, or diminish the enormous clot which the necropsy reveals as the cause of his sudden decease.

The state of the bowels in this malady seems devoid of all connexion with any special features of the lesion. Constipation is, however, the rule in the great majority of cases. And diarrhœa is so much the exception, that we may doubt whether its frequency is much greater than might be expected, supposing it quite independent of the ulcer. But, as already intimated, copious hæmorrhage from the lesion generally gives rise to looseness of the bowels, though without producing any modification of the ordinary epigastric pain. A significant contrast to these facts is afforded by ulcers situated in the first portion of the duodenum (or the immediate neighbourhood of the stomach), which give rise to diarrhœa with much greater frequency than the gastric ulcer. There can be little doubt that this difference is due to that simple law of the peristalsis of the alimentary canal, which connects the movements of the most distant parts of the intestine, while it confers a comparative isolation on those of the stomach.

Amenorrhœa is present as a symptom of the gastric ulcer in so many of the female cases of this malady, as to require a special consideration.

There are no data which would entitle me to make any definite estimate of the frequency with which the presence of this symptom coincides with the existence of the ulcer. But I have found reason to conclude that, on the whole, regular menstruation is far more common than is generally supposed. This fact is quite in consonance with what my inquiries have revealed respecting the total numbers of males and of females past the menstrual epoch, and not arrived at puberty, in whom the lesion has been detected by careful necropsy.

Further, even in the female during this epoch, a careful inquiry seems to indicate that this symptom associates itself with different groups of the lesion, in very different degrees.

It is in the chronic ulcer of middle-aged women that the catamenia are least affected. Many of the cases in which the ulcer has lasted for ten or fifteen years are recorded to have menstruated regularly; some even profusely. Indeed, in some the malady has lasted throughout the whole menstrual epoch of life, without exercising any appreciable influence on that function.

The coincidence of amenorrhœa with copious hæmorrhage from the ulcer is certainly more frequent. But in most of these cases, the relation between the two symptoms seems to be a very natural and obvious one. The amenorrhœa not only follows the hæmorrhage, but is caused by it; just as it would be ensured by any other serious hæmorrhage, or by that drain of nutritious fluids which pregnancy or lactation imply. In other instances, the amenorrhœa precedes the hæmorrhage. But since hæmorrhage is not more frequent in these cases than in cases of chronic ulcer in general, there is no ground for asserting the efficiency of suppressed menstruation as an independent cause of the bleeding. In like manner, there is rarely any connexion between the date of the hæmorrhage and the menstrual period. And finally, whatever has been said by authors respecting the liability of the gastric ulcer to give rise to a periodical hæ-

morrhage that forms a vicarious menstruation, I do not know a single well-authenticated instance of the kind on record.*

There is, indeed, but one group of gastric ulcers with which amenorrhœa seems to have any frequent or direct relation—viz., the perforating ulcers of the young female.†

In speaking of symptoms collected for the first time after the death of the patient (as has happened in many of the scattered cases of this kind which I have collected from different sources), there is so little hope of accuracy, that I do not think it worth while to state exact numbers. It may suffice to say that the majority of these cases exhibit scantiness or absence of the menses as one of the most prominent features of their history; that in many of them the amenorrhœa was accompanied with a state of pallor and anæmia, which was (somewhat rashly) termed chlorosis; that some, however, menstruated regularly and copiously; a few profusely; while a few had never arrived at puberty.

With the latter of these statements we may connect an allusion to a still more frequent condition of the same kind. Many of the so-called cases of amenorrhœa and chlorosis are instances of delay in the appearance of the menses, rather than of their suppression or interruption.

The age of many of the female subjects of these perforating ulcers corresponds to what we should expect from such facts. It is one which closely approaches to the average epoch of puberty, and the year or two immediately following; but which does not exhibit those fluctuations above and below this average age which would be requisite to assign it to the exact access of puberty itself. Still, the coincidence between the amenorrhœa and the ulcer is an unquestionable fact. And the first question concerning this fact suggests itself in the form of an alternative:—Does the amenorrhœa cause the ulcer, or the ulcer the amenorrhœa?

The first of these two questions I think we must answer in the negative: not only because the ulcer is present in the male sex, and in the neuter monster,‡ at the same age, as well as in the female at all other ages, but because, even in the female at this epoch of life, the exceptions

* The only instance I know of which approaches the characters of a vicarious menstruation, is one mentioned by Cruveilhier (Anatomie Pathologique, fol. 1835, vol. i.), apparently on the sole testimony of the patient some years after. A periodic hæmatemesis replaced the menses "when these failed to appear, which happened often enough." It was accompanied by the expulsion of membranous tubes from the bowels; was unaccompanied by pain; and did not prevent the patient from working in the open air. Without more explicit information as to the date of the hæmorrhage with respect to the expected menstruation, the length of the menstrual periods, and of the longest exclusively vicarious flux (its frequency alluded to seeming rather to militate against its duration), this case is hardly to be relied on. With it, however, I may mention another case, where there was no hæmorrhage, but in which it is vaguely stated that the pain in the abdomen was increased before and after menstruation. (New Medical and Physical Journal, for 1811, p. 89.) A similar instance in my own practice seemed, on close examination, to show, that the pain of the menstrual epoch was quite distinct from that of the ulcer, being abdominal rather than epigastric in its site, and dysmenorrhœal in its nature. (Association Journal for Jan. 12, 1856.) But in two or three other cases there has been an increase of epigastric pain at the beginning of the menstrual flux. (Ibid, April 5, 1856.)

† This important fact we owe to Dr. E. Crisp, whose collection of cases, leading to this result, I have noticed elsewhere (British and Foreign Medico-Chirurgical Review, Jan. 1856, p. 171). The meritorious industry of this gentleman has been so little recognised by some English authors on the gastric ulcer, that I am anxious to offer him this acknowledgment. One or two inaccuracies which I have detected in his citations scarcely deserve notice, except in so far as they justify me in assigning to the connexion between the lesion and the amenorrhœa an explanation somewhat differing from his.

‡ Compare the January number of this Review, p. 171.

to the presence of the supposed cause are too numerous to be compatible with such a causation. Indeed, to the various cases of regular menstruation alluded to, we might plausibly add a large proportion of those in which menstruation had delayed its appearance, as well as all those in which puberty was absent. For surely many of the former would scarcely be instances of amenorrhœa, just as all the latter are certainly disqualified for this epithet.

In favour of an affirmative answer to the second question, or of the view that it is the ulcer which causes the amenorrhœa, we may point out that, in most of these cases, the dyspeptic symptoms which correspond to the establishment of the lesion have themselves preceded the deficiency or cessation of the menses; and that such an explanation, as it would receive no contradiction from the mere age of these cases, would find its parallel in the case of other grave constitutional disorders, which scarcely any pathologist would doubt to be the cause of the amenorrhœa by which they are frequently accompanied. A good illustration of this kind of suppression of the menses may be found in the tuberculous cachexia which often selects this epoch of female life as the period of its fatal attack; and which, though often associated with chlorotic symptoms, can generally be distinguished from true chlorotic amenorrhœa by a careful physical examination, aided by an accurate inquiry into the family history of the patient.

A careful observation of the details of that constitutional state which accompanies the amenorrhœa of these gastric ulcers, affords some confirmation of the above view. That state it is customary to speak of as "chlorotic." But I have never yet seen an instance that would suffice to establish the pathological identity of the cachexia present in this group of gastric ulcers with that of true chlorosis; nor do I know of any authentic records of such a case. The differences of the two states are, indeed, essential. The cachexia that attends the ulcer, even when best marked, is devoid of every characteristic symptom of severe chlorosis. The pallor, even where extreme, offers no trace of that greenish hue which the very name of chlorosis (χλωρὸς, *green*) connotes. The dyspnœa on exertion, and the soft bellows-sound, are much less distinct. And lastly, there is little or no œdema of the subcutaneous areolar tissue.*

As the age of these subjects of the gastric ulcer advances, it is not uncommon for the amenorrhœa to cease, all the other symptoms of the lesion remaining unaffected. In rarer instances, the so-called chlorosis also diminishes and disappears. But the most satisfactory proof that the amenorrhœa is not in any sense the cause of the chlorotic symptom, is afforded by some still rarer (though well authenticated) examples, in which the ulcer has been attended by a marked degree of this cachexia, without any interruption whatever to regular and copious menstruation.

But it is obvious that the above view does not at all explain the connexion between amenorrhœa and perforation; much less the fact that the ulcer is more liable to affect the menstrual function at this age of female life than at any other. Nor do I think that there are sufficient materials for such an explanation at present accessible to the pathologist. It would,

* This paragraph is quoted (almost *verbatim*) from some remarks on a case which offers a good example of the peculiarities of this group of gastric ulcers, and which will be found reported by the author in the Association Medical Journal for Jan. 12th, 1856.

however, be easy to suggest that such a periodic hæmorrhage as that of the menstrual flux might well be more easily affected during the struggles of the constitution to establish and maintain it, than when the organism had become accustomed to its recurrence, or strengthened so as to be more indifferent to the loss of blood it implies.

A much less vague and conjectural influence appears to be exercised by this epoch of life on the characters of the ulcer itself. That its great liability to perforation is not due to the ulcerative process sharing in the vigour and activity of youth (as has been suggested by an excellent authority), must, I think, be evident, when we consider the true physiological meaning of vigour on the one hand, and ulceration on the other. Vigour of ulceration is indeed weakness of health; nay more, vigour of constitution, supposing an ulcer already present, would oppose its progress, and limit or diminish its ravages, would ward off perforation by thickening its margins, depositing lymph on its surface, and gluing the peritoneal surface of the stomach to some adjacent organ.

But it is more satisfactory to state a conclusive fact than to point out what seems an abuse of terms. And since there can be no reason for denying to the male sex the vigour and activity such a theory ascribes to youth in general, the fact that the increased tendency to perforation at this age is limited to the female, may spare us all further reasoning on the subject. We need only add, that all the peculiarities of the symptoms and appearances seen in this group of ulcers refer the above tendency not so much to any special activity of the ulcerative process, as to the absence of that inflammatory reaction by which its destructive advance is often checked, and its worst effects warded off.

The cachexia generally associated with the ulcer at other ages of life appears to have precisely the same import as that already suggested for the chlorotic state which represents it in the young female. Like the latter, it seems to be essentially not so much a symptom as a congeries of symptoms: a state that expresses the injury inflicted on the organism by a variety of causes. The wearing effect of long and frequent paroxysms of pain, the fatigue and inanition implied by frequent vomiting, the drain of frequent or copious hæmorrhage, and the loss of digestive power involved in the destruction of the stomach, and finally, the mere age of the patient—are circumstances every one of which is likely to share in producing the cachexia that is present.

As already intimated, it is probable that this cachexia, which is best marked in ulcers of tolerably long standing, and therefore in middle-aged or elderly people, corresponds to the chlorotic symptoms and the amenorrhœa above noticed as generally present in connexion with the gastric ulcers of the young female. And my own experience entitles me to presume that it is rarely altogether absent; that, contrary to what is generally stated on this point, a person suffering from a gastric ulcer scarcely ever wears an appearance such as any observant practitioner would mistake for that of a person in health. Nay more, I might add, that the physiognomy of the disease is so peculiar, that I have sometimes been forewarned of its presence by the mere sight of the patient's features in a crowded Out-patient room at the Hospital. With many characters that would often leave us in complete uncertainty as to whether the

cachectic aspect was due to ordinary chlorosis, to tuberculosis, or (in later life) to the cancerous diathesis, the sharp lines that long and constant pain, and partial starvation, have worn in a patient's face, sometimes afford what is almost a *differentia*, characteristic of this disease. At any rate, this peculiar expression of countenance is, on the whole, a safer indication than mere anæmia, emaciation, or exhaustion can alone afford.

The *perforation* which sometimes occurs in the course of gastric ulcer is notified by symptoms so intense and characteristic, as to require but a very brief description. After more or less distinct indications of the ulcer have existed during a variable period of time, the patient is suddenly attacked by violent pain in the epigastrium, which rapidly spreads over the belly. Its diffusion is accompanied by the appearance of all the ordinary symptoms of peritonitis; the wall of the belly becomes extremely tender to pressure; the patient assumes an attitude which relaxes the muscles of this part; there is an absence of the usual respiratory movement here, which is rapidly followed by great tumefaction of the abdomen, and tympanitic distension of the bowels. If a strict physical examination of the belly be insisted on, the peritoneal cavity will be found to contain fluid—usually those contents of the stomach which have streamed through the aperture in its coats, increased by the subsequent addition of an inflammatory effusion. The continuance of these symptoms generally destroys the life of the sufferer in from twenty-four to thirty-six hours; but death is often preceded by a period of comparatively painless collapse.

Rarely does the train of symptoms that follows perforation offer any marked deviation from the above type.

In many instances, however, a remarkable paroxysm of pain precedes the occurrence of perforation. This intense pain—the duration of which varies from a few minutes to several hours—is, I believe, generally due to a leakage of the gastric contents through that thin film of rotten tissue, to which, at this period, the coats of the stomach are reduced. In consonance with such an explanation, a more chronic pain of similar character has sometimes been found associated with a complete matting together of the stomach and all the neighbouring viscera by a large quantity of lymph, without any visible perforation of the coats of the stomach, or any escape of its contents.

The symptoms of those various modifications* of the process of perforation which were alluded to in treating of the pathology of the gastric ulcer, offer few peculiarities worthy of notice. Partial perforation, allowing of a subsequent repetition (or rather extension) of the accident, or leading to abscess, would of course be distinguished by symptoms which, though differing from each other in every particular instance, would yet offer the general features of being more local, more chronic, and less intense, than those of ordinary perforation fatal by general peritonitis.

There are other circumstances attending the accident which might almost be enumerated among its symptoms. As stated elsewhere, it almost always occurs after a full meal; and is often distinctly traceable to mechanical violence, such as coughing, sneezing, concussion or constriction of the belly. The sensations of the patient frequently verify the nature of the accident, by distinctly appreciating that something has given way in

* See p. 174 et seq., of the last volume.

the belly, and thus caused a gush of fluid that has lit up the agonizing pain which has followed it.

Ampliation, or enlargement of the stomach, as a result of the constriction produced by the cicatrix of a gastric ulcer, constitutes a variety of the malady which, of course, brings with it a special train of symptoms. Into these, however, my present limits forbid me to enter. It is enough to point out that their connexion with the ulcer is both infrequent* and indirect; that they are present in instances of ampliation from so many other causes, that it is only by a history revealing those general symptoms of the ulcer already described, that we could refer any particular case to this origin.

The *Ætiology* of the gastric ulcer has hitherto been rather a subject for conjecture than for any successful inductive inquiry. And the author cannot flatter himself that his own researches will place it in the latter category. But since the exposure of error is one step towards the detection of truth, he ventures briefly to sum up whatever fragments of information he has been able to glean in this respect.

There can be no doubt as to the physiological circumstances which predispose to this disease. Old age, privation, fatigue, mental anxiety, and intemperance are such frequent coincidents of its occurrence, that we are fully entitled to regard them as its more or less immediate causes in a large proportion (I think we might say a majority) of cases. Of these, the influence of advancing age seems, from my inquiries, to be that which is most distinct and indisputable, and which rests on the broadest numerical basis of facts. But that careful clinical study of the malady which my Hospital opportunities have afforded me, leaves me just as little doubt with respect to the influence of poverty and intemperance.

We have found reason to qualify the ordinary notion with respect to the influence of the period of female puberty, and its attendant disturbances of health, in the production of the gastric ulcer. At least, we have seen that this particular epoch predisposes, not so much to the occurrence of ulcer, as to a peculiar character and termination of the ulcer; that it is a want of reaction, resulting in a tendency† to perforation, rather than a proneness to ulceration, which our numerical data would entitle us to assert. And even should this opinion be hereafter modified by a larger series of cases than I have been able to bring together, there will still be such a preponderance in the total formed by the gastric ulcers of the male cases, and of the middle-aged and old female, as to exclude the above group from any general significance with respect to the ætiology of the disease.

* Infrequent, as not occurring much oftener than once in 100 cases; indirect, as implying in most instances a previous cessation of the ulcerative process, or healing of the ulcer.

† In my previous Essay I omitted to add any estimate of the proportionate frequency of perforation at different ages in a given number of ulcers. From a calculation founded on the tables there adduced (p. 170), we may infer that the general proportion of one perforation to 6½ ulcers, or 13·4 per cent., is made up of the following proportions in the two sexes, at the undermentioned ages:

	At ages up to			
	30	50	70	90
Per-centage of ulcers fatal by perforation in both sexes	40	16⅔	10	4⅔
In the male only	28	23	17	7
In the female only	59	10	3	2

The specific diseases with which the ulcer has been seen to occur, seem to have far less influence than the above physiological conditions. Tubercle, pleurisy and pneumonia, syphilis, ague, and fever, are those which are most frequently revealed by the necropsy or the previous history of the subject of gastric ulcer. But the per-centage of tubercle in cases of gastric ulcer does not seem to exceed its average in all persons indifferently. And the same statement will probably apply to all the remaining disorders. It would seem, too, from my own inquiries, that these cases of gastric ulcer do not exhibit that family history of phthisis which we should expect, on the supposition of there being any real connexion between the two diseases, and which we certainly find, under similar circumstances,* in the ordinary form of tubercular disease.

Some have sought to explain the peculiarities of the gastric ulcer by the circumstances of stomach-digestion. Ulceration, say they, having once taken place, the ulcer is prevented from healing, and even increased, by those great and sudden alterations in size which the organ undergoes at different periods of digestion; by the chemical and mechanical irritation of the food; and by the solvent action of the gastric juice upon the languid tissues that form the periphery and base of the ulcer, or upon the scarcely-organized lymph that has been poured out in this situation.

But in respect to the ætiology of the lesion, such an explanation is useless. For it is of little use to explain what happens after ulceration has once taken place, when the very point to which we want to arrive is—Why ulceration takes place at all; and especially, why it singles out this particular organ? It may be said, however, that the extension or continuance of the gastric ulcer form its chief characteristics: that the cicatrices of ulcers are frequently found in the intestine; and that the main peculiarities of the ulcer of the stomach, as contrasted with the ulcer of any other part of the alimentary canal, consist in its long duration, its gradual enlargement, and its reluctance to cicatrize.

I should be sorry to throw any undue discredit upon an explanation possessing so much practical truth and value as this certainly does; but it is the duty of those engaged in clinical researches to subject to a strict scrutiny every view which is not already established on an irrefragable basis. Now, there are not only good grounds for presuming that we have no series of facts (such as the careful records of systematic necropsies would afford) sufficient to establish the propositions these statements imply, but all the information as yet at our disposal is such as to inculcate the greatest caution in receiving them.

The ulcer of the stomach appears to cicatrize, probably without any medical treatment directed to this end, in about fifty per cent. of its total numbers. It does not seem to perforate oftener than about once in eight cases. No doubt, the class of intestinal ulcers, as a whole, offers a larger per-centage of cicatrices, and a smaller of perforations, than the above numbers. But the truth of the above propositions assumes an amount of this difference, such as we have at present no right to assert.

* Careful inquiry among the same class of Out-patients has shown me a remarkably hereditary character of the pulmonary tubercle to which they are so liable. (Compare the Author On Life Assurance, 1856, p. 12: also the Report of the Brompton Hospital for 1849.)

Further, when we examine into the two classes of lesions which we group under the terms of "gastric ulcer" and "intestinal ulcer," we shall find reason to attribute still less value to the above limited numerical differences. For we have seen that the majority of ulcers of the stomach cannot, in the present state of our knowledge, be traced to any specific constitutional disease. While we know that a large majority of those intestinal ulcers we are now contrasting with them, merely form the local expressions of a general malady, by the nature, date, and duration of which they are themselves dictated and regulated. The typhoid fever reaches its term, and the exulcerated agminate follicles generally heal over; the attack of dysentery subsides, and its ravages in the large intestine are more or less repaired; the phthisical cachexia continues, and the ulcers it has produced also remain. Are there no grounds for suspecting that the ulcer of the stomach may be due to some chronic vice in the organism, which dictates its occurrence, duration, and cessation, like the typhoid, dysenteric, or tubercular states of the constitution in these cases? At any rate, is there not a sufficient difference between the two classes of gastric and intestinal ulcer to forbid the unqualified reception of any pathological theory which regards them as alike in all save their mechanical and chemical circumstances? Besides, it seems very easy to overrate the influence of the circumstances above alluded to as tending to retard the process of cicatrization in the ulcer of the stomach. Wounds of this viscus heal with great facility, not only in the domestic mammalia usually selected for the purposes of physiological experiment, but in the healthy human subject. Indeed, in both man and animals, all fistulæ of the stomach seem to progress naturally towards closure and cicatrization, although placed under conditions which appear to be much less favourable to such a result than those of ordinary wounds or ulcers. While, as already mentioned, the ulcer itself heals with a greater frequency than can be explained by any theory which makes the circumstances of the digestive process so principal or special a cause. Lastly, every organ is adapted to its circumstances, and is generally organized to resist any unusual exposure that these may imply.

The details by which the process of ulceration is commenced would probably throw some light on its causes. But unfortunately, these still remain almost unknown to us. Direct observation is almost impossible. Analogy has little value. And the scanty and imperfect conjectures that both of these sources afford, are scarcely even consistent with each other, much less susceptible of a common application to any single view of the ætiology of the gastric ulcer.

In respect to the former, however, we may notice that several necropsies have shown ulcers of the stomach associated with more or less circumscribed patches of marked congestion, ecchymosis, or extravasation, which have been regarded as the beginnings of similar lesions in the neighbourhood of these ulcers. A similar (though quite distinct) class of appearances, constituting the "hæmorrhagic erosion" of Rokitansky, is believed by him to represent the mode by which the ulcer commences. But, to say nothing of the rarity of these appearances in conjunction with the gastric ulcer, I do not think any one conversant with the irregularities that often affect the distribution of blood after death in the digestive canal, would

like to assert the first class of phenomena to be distinctive of commencing ulceration. And the last is not only partially open to the same objection, but appears to be quite a distinct disease from the ulcer, lasting an indefinite time without showing the slightest disposition to merge into it, and exhibiting a somewhat different train of symptoms.

In still more exceptional cases, we find another class of phenomena, concerning which we may repeat the doubts already expressed respecting the above hæmorrhages. A distinctly oval or circular depression has been found in the neighbourhood of an ulcer of the stomach; or the mucous membrane has exhibited an appreciable softening of the same size and shape—once or twice even in conjunction with a reddish or darkish tinge of discoloration. Here, again, we ask, what proof have we that these depressions or softenings would have become ulcers, or even that they existed during life? Analogy, however, though it does not answer this question with a definite affirmative, at least affords us a strong presumption. That ulceration of the duodenum, which often follows severe burns, and which I have carefully avoided hitherto introducing into these discussions, because its situation, cause, and appearances alike seem to me essentially distinct from the ulcer of the stomach, has been all but seen to begin in this way.*

Such facts and analogies perhaps entitle us to hazard a conjecture, with which we will end these somewhat vague and unsatisfactory guesses at truth. Not only is there every reason to deny the existence of any specific disease that can lay claim to the title of "*the ulcer of the stomach,*" but all the varieties that affect the form, rapidity, situation, numbers, and terminations of this lesion, seem to find their parallel in the causation of the malady, both as regards the organism generally, and those first departures from the normal state which inaugurate the local mischief. In short, we have no more right to talk of *the* ulcer of the stomach than of *the* ulcer of the leg; no more reason to assume an invariable commencement of the gastric ulcer by hæmorrhage, or by softening, or by a submucous deposit of lymph, than we have to restrict the beginning of what is evidently a similar process of destructive absorption in the limb, to an ecchymosis, a pimple, a superficial abscess, a burn, or a varicose vein. And just as our every-day experience assures us that external or cutaneous ulcers may begin by either of these lesions—may have, that is, either of them as its immediate and conditionating cause, and may yet retain a general identity of that ulcerative process to which they are chiefly due—so not only do these very facts afford us fair grounds for supposing a similar diversity in the case of the ulceration that affects the stomach, but all that we have been able to glean respecting it confirms this analogy. The variety of diseases with which it appears to be connected, the equally numerous and diverse physiological conditions that favour its occurrence, can only thus be explained. Ague, fever, or the vascular disturbances of female puberty, might, perhaps, be supposed to facilitate or cause ecchymosis. But to convert this effusion into an ulcer would require a process of destructive absorption that no mere extravasation would explain. While the influence of old age, or privation, or fatigue, which throws little

* Transactions of the Pathological Society, vol. i. p. 258. Those who know Mr. Prescott Hewett's merits as a morbid anatomist will excuse me if I regard any observation of his as very unlikely to overstate the distinctness of the appearances present, or to confound the effects of disease with the sequelæ of death.

or no light on the precise local change that ushers in the ulceration, exactly concurs with the efficacy of these circumstances in the production or promotion of ulceration elsewhere. In like manner, to grant that the circumstances of digestion often retard the healing of a gastric ulcer—and the marked effect of treatment specially directed to these circumstances proves no less—is to concede nothing more than what, *mutatis mutandis*, we may verify for an ulcer of the leg, in which the same result is equally under the influence of such physical circumstances as posture, pressure, and the like. Nay more, the analogy not only applies to these details, but appears to illustrate the remarkable fact elicited in my former Essay. Here I found reason to conclude that the epoch which immediately follows the access of puberty in the female imparts to the gastric ulcer a peculiar character, best defined as a more or less complete absence of the inflammatory reaction that generally engages the base and margins of the lesion. And the large experience of Mr. Critchett has observed a similar character in the ordinary cutaneous ulcer at the same period of life; associated, too, with an analogous cachexia, and an equal disturbance of the menstrual flux.*

Diagnosis generally.—From what has been already stated with respect to the great varieties to which each of the symptoms of gastric ulcer is liable, we might, on merely arithmetical grounds, deduce the infinite modifications that would result from their combination. Of these it is no exaggeration to say, that they make each case of ulcer of the stomach unlike every other. And they especially suggest two questions respecting the diagnosis of the malady. (1.) What is the minimum of evidence that will justify us in affirming the existence of an ulcer of the stomach during life? (2.) What are the diseases with which it is most likely to be confounded?

A specific answer to the first of these two questions it is impossible to give. But I am inclined to think that nothing less than all the chief symptoms enumerated entitle us to pronounce a decided opinion. In other words, unless the pain possess the characters attributed to it, unless this pain be accompanied by vomiting, and unless there be evidence of hæmorrhage having occurred in the course of the malady, there is no sufficient basis for a definite diagnosis of the existence of a gastric ulcer. The date, duration, and frequency of the pain chiefly indicate some morbid condition of the mucous membrane of the stomach. The vomiting adds, that this disease implies great irritation of the nervous centres connected with the organ. And it is reserved for the hæmorrhage to show that the disease is such as to involve an absolute breach of continuity in the structure of the stomach.

But I have not the slightest doubt that an absolute enforcement of this rule of diagnosis would lead us to overlook a vast number of cases; and might thus be the occasion of grievous errors in practice. In point of fact, beyond the limits of secure diagnosis, there are a large number of cases in which we may justifiably entertain strong suspicions that the symptoms are due to this lesion.

* Critchett on Ulcers of the Lower Extremity, p. 107 et seq. London, 1849. The resemblance of the two lesions is completed by the facts, that in both the ulcer precedes the amenorrhœa, and is aggravated at the menstrual periods. (Compare pp. 170, 171 of this Essay.)

Here (as usual) I am desirous to be understood as speaking chiefly of my own clinical researches. But though I dare not lay much stress on the negative evidence derivable from the symptoms recorded in many hundreds of cases of perforation or hæmorrhage scattered through various journals,—for in a large number of these there may not have been such rigid and minute investigations of symptoms during the life of the patient as to justify us in denying the presence of all indications of disease save those mentioned,—still it is probable that many of them afford strictly accurate records of the dyspeptic ailments that have preceded the fatal attack. And hence it is very possible that of the numerous cases mentioned in which more or fewer of the above symptoms are not recorded, some are instances in which they were really absent.

But much more trustworthy evidence of such irregularities in the train of symptoms that characterize the gastric ulcer, is constantly being brought under my notice in Hospital practice. As might be expected, a moderate hæmorrhage readily escapes the notice of both the patient and his medical attendant. And even where the former habitually inspects the stools, or the physician calls in the aid of the microscope to an examination of any suspicious egesta, the irregularity of its occurrence may baffle all attempts to verify it for months together. In like manner, the vomiting seems to be sometimes, though much less frequently, absent from the history of the malady, during a great part of its course, or merges into a trifling regurgitation after meals, such as we hardly dare consider its representative.

It is of course impossible to define the precise degree of suspicion that ought to attach to a case in which the evidence of gastric ulcer was rendered imperfect by the absence of either of these symptoms, or of both simultaneously. But weak as such a suspicion often is, I am disposed to think that in every instance in which we find long or aggravated dyspepsia seriously affecting the general health, and associated with pain and tenderness in the epigastrium, and pain in the interscapular region, increased immediately after the ingestion of food,—there we ought at least to keep steadily before us the possibility of a gastric ulcer; as a possibility which, even if it falls far short of a definite diagnosis of this lesion, is yet sufficiently important to dictate the whole plan of treatment. And these remarks will especially apply to such symptoms when they occur in connexion with amenorrhœa in young females who have lately attained the epoch of puberty. Here the absence of hæmorrhage, and the little attention such persons often give to their dyspeptic symptoms, sometimes conspire to obscure the diagnosis: even where a careful inquiry into the history of the malady, and a sedulous examination of the epigastric region, afford us reason to suppose that the patient is in imminent danger of death by perforation of the stomach.

The above observations render it unnecessary to dilate upon the means by which we should generally distinguish between dyspepsia and gastric ulcer. In a great majority of cases, there is little difficulty in deciding which of the two maladies is present. But in some cases the distinction is by no means easy. And there are good reasons for conjecturing, that of all the Protean forms which dyspepsia may assume, that called the morbid sensibility of the stomach is the one which is most likely to

include cases of ulcer; or, in other words, if really independent of this lesion, is most likely to be mistaken for it.

To distinguish between ulcer and cancer of the stomach is generally just as easy: occasionally, just as difficult or impossible. As a rule, the symptoms of the cancerous lesion only appear at a comparatively late period of the malady, when the pylorus forms a hard moveable tumour, that can be easily felt from the exterior of the belly. Prior to this, the pain is either absent, or if present, has a lancinating character, and a time of appearance that belongs rather to the later stage of gastric digestion, than to the few minutes that immediately follow deglutition. The vomiting of the same stage of the malady often exhibits an analogous difference. The matters thus rejected will occasionally, to the practised microscopist, afford valid evidence of the presence of malignant disease in the cancer-cells they contain. The hæmorrhage is rarely excessive, and (ulceration being of comparatively late occurrence in gastric cancer) is often limited to the last few weeks of life. And lastly, the comparative duration of the two maladies, the age of the patient, or his cachectic aspect, often aid the diagnosis.

But not only do hardly any of these characters possess much independent value, but even several of them in combination may leave the question undecided. The gastric ulcer may come on as rapidly, and destroy life as quickly, as cancer. A large proportion of its subjects are middle aged or old. Many of these, too, have a cachectic aspect, such as is often easily mistaken for that of cancer; sometimes quite undistinguishable from it. The hæmorrhage of the ulcer may affect the moderate amount, and the "coffee-grounds" appearance, it ordinarily offers in cancer. The pain may be intense; and the access of its paroxysms may be habitually delayed until much more than the ordinary date after ingestion. Finally, the lymph by which an ulcer adheres to the liver may give rise to the production of a tumour, which is capable of being felt through the wall of the belly. And unlikely as it may seem that these separate contingencies should all combine to obscure the diagnosis of any single case, such instances do from time to time occur. Indeed, the reader may be referred to a late number of the 'Association Journal'* for an instance of this kind in my own practice; where though, from being engaged in the express study of these gastric diseases, I naturally gave the utmost attention to every feature of such a doubtful case, yet it was only at the necropsy that I could satisfy myself whether the ulceration I had diagnosed some months before was or was not malignant.

Art. II.

On the Proximate Cause of Functional Action. By J. Hinton.

The actions which take place in the animal body naturally divide themselves into two classes—the nutritive and the functional; or those which are concerned respectively in the formation of the organs and their use. In some instances, it may be difficult to draw the exact line at which nutrition ends and function begins, but for the most part the distinction is clearly defined, and theoretically the separation of the two forms of

* Association Journal for 1855, p. 1107.

action is always easy. There are three forms of function: nervous action, muscular contraction, and secretion. Taken in a large sense, these divisions appear to include all the active functions known to exist in the human body.

In the following remarks, no explanation will be attempted of the phenomena of nutrition: accepted for the present as ultimate facts, they form rather the basis upon which it will be sought to found a consistent theory of the cause of functional activity.

Little doubt can be entertained that the force which is operative in the vital processes is but a peculiar manifestation of the same force (or forces, if there be more than one) with which we are familiar under other names, as regulating the phenomena of inorganic nature. But although thus, in its origin, one with the other physical forces, the peculiarity of the conditions under which it exists in living bodies imparts to it specific properties, to designate which the term *vital* is employed. One of the most characteristic of these peculiar modes of action of the vital force, is the opposition which it presents to the operation of those forms of force which are termed *chemical*—an opposition not of essential nature, but of special direction. The vital force (or, as from this point of view it might be called, the vital affinity, for the sake of bringing out more clearly at once the relation and the contrast) controls and holds in abeyance the chemical tendencies of the matter in which it subsists.*

From the state of chemical tension thus arising, it results that there exists in all living matter a constant tendency to change. No sooner are the conditions requisite for the manifestation of vital properties withdrawn, than chemical affinity resumes its sway, and decay commences. Even during life the same process is continually going on. The tissues waste, and are renewed, and waste again.

A certain connexion between this waste or disintegration of the tissues and the functional activity of the body in which it takes place, is universally admitted. Yet the relation which subsists between them is by no means satisfactorily established. For the most part, the activity is held to precede and cause the waste.

"Discharge of function, *consequent degeneration*, absorption, and replacement by new structures."†

"In the history of a cell there are three stages,—that of its growth, of its decay, and the intermediate one of its functional activity, which is dependent upon the first, *and which causes the third*."‡

"We may look upon the death of such cells (the muscular tissue), whose term of life might otherwise have been considerably prolonged, *as the result* of the expenditure of their peculiar modification of force under the guise of mechanical power."§

In this representation it appears to me that the relation of cause and effect is inverted—that the existence of a controlled and subjugated tendency to chemical change in living bodies is the origin of all the capacity for functional action which they display, and that the disintegra-

* There can be no difficulty in conceiving forces essentially the same acting thus circumstantially in opposition. Innumerable instances will occur to the mind in which heat, for example, opposes chemical affinity, or gravitation itself raises or suspends a weight.

† Mr. Paget: Lectures on Surgical Pathology, p. 131.

‡ Dr. Bucknill: British and Foreign Medico-Chirurgical Review, No. 29, p. 226.

§ Dr. Carpenter: Human Physiology, p. 109.

tion of their tissues is not a "result" or "condition" of their activity, but rather the moving spring and source of that activity itself.

The life of the body being one, its functional power must be one also. Widely as they may differ in their immediate form and object, the functions, when regarded in relation to their origin, may not be isolated from each other. They are common products of a single power, which requires to be investigated at once in all its modes of action. Hence, probably, the want of success which has attended the various attempts that have been made to trace the physical causes of separate functions. But, on the other hand, much of the obscurity which attaches to the ideas of life and the vital force appears to have arisen from the attempt to include under one denomination, and to refer to one mode and development of force, phenomena of diverse, and indeed opposite, characters.

Broadly as the line of demarcation is drawn by nature between those processes by which the living organism is built up and maintained, and those which involve the death and disintegration of the tissues in which they occur, the prevailing tendency of physiological speculation has been to include both series of actions under one name, and to refer them to the immediate operation of a common power. They have been termed indiscriminately *vital actions*, and adduced without distinction as instances of the direct operation of the vital force.

Thus Liebig says: "The active or available vital force in certain living parts is the cause of the mechanical phenomena in the animal organism."*

And Dr. Carpenter thus expresses himself: "The contraction of any muscle upon the application of a stimulus must be attributed to an exercise of *vital force* engendered by previous acts of nutrition."†

And again, speaking of muscular and nervous action, he says: "We are entitled to affirm that each is a peculiar modus operandi of the same force as that which is concerned in cell-formation."‡

According to this view, the vital force is made the direct agent in actions essentially different. Hence arises the impossibility of defining it; for while the words are so used it is surely in vain to seek to attach to them any signification more definite than that of a general expression for all the changes which take place in a living body. Any term similarly used would become equally obscure and unsettled. By thus including in one category actions so opposed as function and nutrition, the phenomena of life are placed in an attitude of irreconcilable variance with those which pertain to all other branches of physical science. The fatal error has been to overlook the fact that two forces (or modes of force) are at work in the living body. It has not been perceived that the *chemical* affinities of the animal organs constitute a source of power co-equal with, and precisely measured by, the power of the vital force. The work of two agents has been assigned to one. If now the omission be supplied, and the vital and chemical forces be recognised as the two forces of organized matter—the former as the resistance, the latter as the resisted force, and therefore the force available for action—a large part of the obscurity

* Organic Chemistry of Physiology and Pathology, p. 221.
† Human Physiology, third edition, p. 476.
‡ Philosophical Transactions, part 2, p. 737. 1851.

which envelopes the theory of vital action is at once removed. An uniformity of principle is seen to prevail between the laws of the organic and inorganic worlds, and the facts hitherto so intractable arrange themselves without difficulty in accordance with some of our most familiar conceptions.

Bearing in mind that no explanation is offered of the nutritive processes in the living body, it will be seen that upon the theory propounded there is a perfect analogy between the animal body and a self-acting machine.

In both there exists a mechanism adapted to the performance of certain defined actions, and a reservoir of power or force by which that mechanism is kept in operation. In both, the source of this power is essentially the same. In living bodies one tendency of matter, its chemical affinity, is held in check; in any machine that is to manifest a capacity for action, art must bring into a like condition of restraint some tendency of matter, either the same or similar.

In the simple instance of a clock, the gravitation of the weights, controlled by an adapted mechanism, is the power which effectuates its functions—the revolution of the hands, the striking of the hour. In the watch, the restrained elasticity of the spring holds the same relation. The steam-engine owes its power to the repressed expansiveness of the vapour. There is no instance, indeed, of an artificial accumulation of force or capacity for action that does not depend upon this principle. Matter restrained from the fulfilment of any of its natural tendencies affords power; the removal of the restraining force, permitting the play of the tendencies so controlled, produces action; which action may be made to subserve any purpose by suitable modification of the resistance, and the employment of an adapted mechanism.

In this respect the organic and inorganic worlds obey a common law. Organization gives capacity for action only by virtue of the resistance it presents to the chemical forces; these chemical forces, acting under definite limits, and in connexion with various structures, being the true sources of all functional activity. A living body is a divinely-made machine, constructed, indeed, with a marvellous delicacy, perfection, and complexity, and depending upon a power, the vital modification of force, which it is wholly beyond our skill to imitate or comprehend, but still involving in its *working* no other principles than those which we every day apply, and see to regulate the entire course of nature.

For the inorganic world furnishes abundant instances of the same balancing of forces resulting in a similar activity or capacity for action. The term *irritability*, in so far as it denotes a capacity for responding to stimuli, confined hitherto to organized structures, might with perfect accuracy receive a more extended application. It exists in whatever form of matter there is found the same powerful tendency to change of state with which it is associated in living bodies. Thus, in the chloride or iodide of nitrogen the slightest touch induces an explosion. In the case of gunpowder, the tendency to change in which is less energetic, the chemical affinities of the materials are brought into action by the momentary application of intense heat. In the same way a solution of certain salts, when the cohesive force is barely counterbalanced by the sol-

vent power of the water, will assume the crystalline form upon the gentlest touch, or the mere passage of a vibration. The slightest scratch causes unannealed glass to break.

In these instances—and very many more might be adduced—it is surely correct to say that action ensues on the application of a stimulus; and in them all it is obvious that the action is immediately due to pre-existing and restrained tendencies to change of state. The stimulus is only in a secondary sense the cause of the phenomenon, and evidently determines it by removing the condition which forbade the previous operation of those tendencies. In all such cases the modus operandi is the same as that of the mechanisms previously referred to, and they are precisely analogous to the simpler contrivances in which a suspended weight is made to fall upon the disturbance of its equilibrium by slight causes.

If the doctrine of the correlation of the physical forces be applied to material actions or changes of this class, it becomes at once apparent that the correlated force is neither the resistance nor the stimulus, but the controlled or latent tendency to change.

Thus, e. g., the application of a certain amount of heat to a magnet suspends its attractive power. If, therefore, to a magnet sustaining a mass of iron sufficient heat be applied, there results an action—the fall, namely, of the iron to the earth, the cause of this action being the gravitation which the magnetic force had previously been exerted in controlling. It might be said that the gravitation is converted into motion; it would never be proposed to attribute the motion to a conversion either of the magnetic force or of the heat into mechanical force. But in respect to the animal functions, this very error has been committed; for in the illustration above cited the magnetic attraction represents the vital affinity or force, the gravitation the repressed chemical affinities of the living tissues, the heat the stimulus, and the fall of the weight the function.

Many arguments may be adduced to show, that while the Correlation Theory affords a consistent and beautiful expression of the relation which exists between the forces of the external world and the developments of the vital force in the growth and nutrition of the body, it is entirely misapplied when it is proposed as an explanation of the connexion of the vital force with functional activity.

In the first place, this view entirely ignores the balanced state of the forces in the animal economy, and the accumulation of power arising from the repressed chemical affinity, which it regards merely as operating, after the vital force has discharged the function, in reducing to simpler compounds the devitalized tissue. Surely this is utterly opposed to all we know of the economy of force which prevails throughout nature, and pre-eminently in the living body, in which no power, how subordinate soever, or apparently trivial, is ever wasted.

It is unquestionable, that in this state of equilibrium of the chemical and vital forces there exists an arrangement by which great results might be accomplished. Everything is prepared for the exhibition of a large amount of power by the mere permission of the play of chemical affinity. Would it not be a gratuitous squandering of resources that such a capability for action should be turned to no account?

2ndly. To suppose a conversion of the vital force into functional action,

is to set aside an actual and sufficient cause in favour of one that is entirely hypothetical. The state of chemical tension in the animal body, and the co-existence of chemical change with functional activity, are admitted facts: that this chemical action in the tissues gives rise to the external manifestations of function, is an inference as simple as that the chemical change among the particles of gunpowder is the cause of its explosion. How, then, are we justified in assuming the existence of another process, hard to conceive, and impossible to demonstrate?

3rdly. The theory in question, while it rejects a cause so natural and obvious, in reality involves the idea of an effect without any adequate cause at all. No intelligible relation of cause and effect can be shown between the stimuli which excite the functions and the conversion of force which they are supposed to cause, or for which they "supply the condition." No proportion is maintained between the amount of the stimulus and the amount of force converted. In what way, for instance, can gentle pressure on the thumbs of the frog, during the season of coitus, produce a conversion of the vital force of nearly all the muscles of the body into an energetic contractile action?

4thly. Waiving all theoretical objections to the view of the correlation of vital force and functional activity, it may be remarked that the facts do not agree with the principles of that doctrine. The "material substratum" is wanting. In the conversion of the vital force of a muscle into mechanical force, for example, there is no change of the matter in which the force subsists. The conversion supposed is precisely such as would occur if a heated body were suddenly and without adequate cause to lose its heat, and manifest electricity instead, or shoot into spontaneous motion. The view propounded by Liebig—viz., that the vital force which is converted into mechanical force in muscular contraction is not that of the muscle itself, but may be derived from any other part of the organism, and conveyed to it by the nerves—would be more accordant with the terms of the theory, but we know experimentally that it is not correct.

5thly. The vacillating language used in reference to this part of the subject, by those who have most successfully applied the doctrine of correlation to vital phenomena, betrays the unsoundness of their position.

"Muscular contraction," says Dr. Carpenter, "may be regarded as proceeding from the expenditure or metamorphosis of the cell force, which ceases to exist as a vital power, in giving rise to mechanical agency." But speaking of the external stimuli of muscles, he adds: "These agencies are concerned in occasioning that metamorphosis of living organized tissue into chemical compounds, whereon the development of the muscular force seems to be immediately dependent."*

Are not two different origins here assigned to muscular contraction? Again, Dr. Carpenter observes (p. 747), "We are, then, to regard the *nervous*, electrical, and other stimuli under whose influence the muscular force is called forth, less as the immediate sources of that force than as furnishing the conditions under which the vital force, acting through the muscle, is converted into the mechanical force developed in its contraction."

* Philosophical Transactions, part ii. p. 746. 1850.

But at p. 745, we read: "The *nervous* force appears convertible into motion through the medium of the muscular apparatus."

With regard to the nervous force, Dr. Carpenter writes as follows: "We find only one kind of tissue serving for the generation and transmission of nervous power, this alone affording the material substratum through which the *vital force* can manifest itself as nervous agency." And again: "Nerve force *which has its origin in cell formation* may excite or modify the process of cell formation in other parts." (p. 743.) But, on the following page, he argues, that "all the facts that have been adduced in support of the identity (of the nervous force and electricity) will be found readily explicable on the idea of their correlation or mutual convertibility."

Can the nerve force be both a manifestation of vital force and a result of the conversion of electricity? Can it have its origin at once in cell formation *and* in a galvanic current? And yet, further, are there not the same reasons for holding that the electrical stimulus only furnishes the conditions under which the vital force is converted into the nervous force, as exist in respect to muscular contraction?

Even Liebig's perspicuity fails him upon this subject. In his observations On the Phenomena of Motion in Living Bodies, he writes thus: "All experience proves that there is in the organism only one *source* of mechanical power; and this source is the conversion of living parts into lifeless amorphous compounds."*

But at p. 220, "As an immediate *effect* of the manifestation of mechanical force, we see that a part of the muscular structure loses its vital properties, its character of life."

Is not the same change thus made both cause and effect?

The last writer on this topic, Dr. Reynolds, in an able article On the Objects and Scientific Position of Physiology,† is not more definite in his language. Compare the following passages:—"The partial disintegration of the tissues (of the muscular and nervous systems) is one condition or *source* of their action." (p. 112.) "We have therefore to regard these animal properties (sensibility and muscular contraction) as functions of the *vital force* inherent in the cell, and as constituting two of its special endowments." (p. 118.)

In the passages above cited—and many more of the same character might be adduced—two contradictory ideas appear to have been struggling in the writer's mind, and alternately giving the colour to his language. One is, that motion, or nervous action, as the case may be, is a direct expression of the vital force; and the other, that it is the result of the chemical disintegration of the muscular or nervous tissues. Owing to this cause, the words used virtually assert that the retrograde metamorphosis of the tissues, or their conversion into lifeless compounds, is a result or manifestation of the vital force, which is in its very terms a contradiction.

To these considerations it may be added, that to affirm the function to be the result of the accompanying disintegration, is to adopt the negative side of the argument. It enables us to reject altogether *sensibility* and *contractility*, as separate properties of the nervous and muscular tissues,

* Op. cit., p. 242. † British and Foreign Medico-Chirurgical Review, No. 31.

apart from their known tendency to chemical change. And no principle in science is better grounded than that nothing may be assumed to exist without a proved necessity.

The substance of what has been advanced may be briefly stated thus. Dynamically considered, the changes which take place in the inorganic world are divisible into two classes—those which directly result from the application of some new force to the matter in which they occur, and those which ensue from pre-existing tendencies to change when some force previously operative is neutralized or overcome. The former class of material changes are characterized by an absolute proportion between the force applied and the resulting action; the latter are distinguished by their spontaneity, or the disproportion (often extreme) between the apparent cause and the result.

The endowments of living beings embrace both these forms of action. The first is seen in the processes of nutrition, development, and growth, the forces engaged in which are truly correlated, as Dr. Carpenter has most ably shown, to other forms of force. The changes in which function consists exemplify the second, being effected by the chemical affinities of the elements of the tissues, when the vital resistance is in definite manner and degree diminished.

Treating the question thus on abstract grounds, it can hardly be denied that the view of the vital functions above propounded possesses great simplicity, and by virtue of its wide analogies, a certain amount of *à priori* probability. It aids in reducing to the smallest number "the assumptions which, being granted, the order of nature as it exists would be the result." But it cannot on such grounds claim acceptance, unless it be capable of an unstrained application to all the phenomena which come within its scope. It would almost appear, indeed, to be so natural an interpretation of the facts of animal existence, that had it been the true one, it could hardly have been overlooked or rejected, and that the class of functional actions must have presented characters which, indicating the direct agency of the vital force, forbade them to be grouped under so simple an expression. I shall proceed, therefore, to an examination of some of the leading facts connected with the animal functions, and inquire:—

I. How far the actions of the nervous system may be interpreted upon the principle suggested. From such an inquiry it is of course necessary to exclude altogether the phenomena of thought and volition, viewed in their psychological relations. Of the mysterious process by which a material change in the brain awakens a perception or kindles a thought, we are entirely ignorant; nor can we form any conception of the mode in which the spiritual will communicates its behests to its obedient instrument. Whatever theory be adopted of nervous action, these relations must remain equally inscrutable. Confining our attention, therefore, to those operations of the nervous system which are strictly physical in their character, it may be remarked that all the stimuli which excite them are adapted to bring into activity the repressed chemical affinities of the organic elements. Thus the nervous force is called into action by mechanical irritation, or motion in whatsoever form applied, by changes of temperature, by chemical reagents, electricity, light, or sound, and by the sapid and odorous properties of matter. It is hardly possible to perceive in these various

agents any property in common to which their influence upon the nervous system can with reason be referred, except the power they all (so far as they are known to us) possess of disturbing an unstable chemical equilibrium. They cannot all supply a force which is converted into the nervous force. They have no visible adaptation to cause such a conversion of the vital force. No analogy warrants the assumption that they can immediately produce a state of active polarity. But acting upon a tissue in which the affinities of the component elements are so delicately balanced, and the inherent tendency to chemical change so strong, it can hardly be otherwise than that they should overthrow that balance, and bring into play the latent and coerced attractions.

"In compounds in which the free manifestation of chemical force has been impeded by other forces," says Liebig, speaking of inorganic substances,* "a blow, or mechanical friction, or the contact of a substance the particles of which are in a state of transformation, or any external cause whose activity is added to the stronger attraction of the elementary particles in another direction, may suffice to give the preponderance to this stronger attraction, and to alter the form and structure of the compound."

That such an actual change of the composition of the nervous tissue does ensue from the action of the stimulus, is proved by the fact that the same stimulus will not reproduce the effect until after the lapse of a certain interval. This should not be the case if the stimulus merely induced a polar state, or itself assumed the form of the nervous force. The necessity of time for the renewal of the irritability is evidence of an altered composition.

Instances have been adduced from the inorganic world of the production of action in substances prone to change by slight mechanical irritation, which may be referred to as the analogues of the sense of touch. The senses of sight and hearing are susceptible of illustration by similar analogies.

To prepare a plate or paper for photographic purposes, it is only requisite to apply to it a suitable chemical compound, the elements of which tend to assume other relations, and of affinities so weak as to be overcome and neutralized by light. Thus prepared, the paper is called sensitive, and it would appear to furnish a very exact illustration of the process by which vision is effected.

The retina consists of matter prone to change. Its elements break up and enter into new relations immediately the vital force or affinity which holds them in their existing combinations ceases, or becomes impaired. What hypothesis can be more simple than that the luminous rays of the spectrum should have the power, to a certain extent, of neutralizing this delicate affinity, and thus causing, or, to speak more correctly, permitting, a definite chemical change to take place in the retina; just as the actinic rays, overcoming the affinities of the photographic salts, cause or permit a new arrangement of their elements?

The sense of hearing also admits of explanation by the application of the same principle. In the texture of the auditory nerve it appears that the chemical and vital forces are so balanced that the sonorous vibrations overthrow the equilibrium, and bring into activity, as in the case of light,

* Op. cit., p. 207.

the chemical affinity. An illustration of the nature of the action is furnished (if we may compare great things with small) by the fact mentioned by Rogers, that masses of ice and snow of considerable magnitude may be precipitated from the Alpine ridges by the sound of the human voice. The gravitation of the masses, and the resisting forces which maintained them in their places, being in such exact equilibrium, that even so slight a motion of the atmosphere suffices to give the preponderance to the former. This illustration, remote though it may seem, is valuable, as bringing clearly before the mind the essential character of the process which constitutes the animal function. For the stimulus in this case, the aerial vibration, palpably induces the resulting action, not by any direct agency, nor by a conversion of one form of force into another, but solely by disturbing the equilibrium of the counteracting forces, and neutralizing the resistance which opposed the force of gravity.

Such a change of composition in the nervous substance must tend directly, in conformity with all our knowledge of physical laws, to produce a polar state or force, corresponding in every respect with that which we term the "nervous force." The close analogy which exists between the nervous force and electricity, strongly confirms this view of its origin and nature. For we recognise chemical change, and especially the decomposition of compound bodies, by means of stronger affinities acting on their elements, as an invariable source of the electric force; and Mr. Grove has demonstrated its existence as a result of the changes which take place in the photographic process. In the living body, it would appear that the decompositions (if they may be so called) in which the exercise of function consists, give rise to a force—not electric, indeed—but of a peculiar though analogous character, inasmuch as the changes in which it has its origin, though analogous to those which take place in inorganic matter, are yet of a distinct and peculiar order. Thus regarded, the nervous force, in its relation to functional activity, may be defined to be a polar condition, or other molecular change in a nerve, akin to that which exists in a body conveying a current of electricity, and arising from a chemical change either in itself or in any of the tissues with which it is in relation. This change being the result of the chemical affinities of the elements of the tissues, which come into play when the vital resistance is diminished by any force which, so disturbing the equilibrium, is called a stimulus. I have said the nervous force may be thus defined in its relation to functional activity, because there appears to be much evidence that the changes which constitute the development and nutrition of the tissues also give rise to a force which, traversing the nerves, contributes materially to the energy of the vital processes, and more especially, perhaps, to the sympathetic development of various portions of the body, and the general condition of vigour which is denominated tone. This question, however, does not fall within the scope of the present paper, which relates only to those actions in the living body that are attended with a retrograde change of structure.

The nervous force, therefore, having its origin in chemical or anti-vital changes, must possess an especial adaptation for exciting changes of a similar character. Hence it is pre-eminently the excitor of function, causing in any organ to which it may be conveyed the same subordination

of the vital to the chemical affinity from which it sprang. To take another illustration from the eye. Light impinging on the retina determines therein a chemical change, which developes in the optic nerve the nervous force. This force causes in the brain an action of the same order as that in the retina. Hence again originates a nervous force, which, conveyed to the iris, causes yet a third time a chemical change, which is the source of its contraction.

That the nervous force, as excited by stimuli, is opposed to the vital affinity, and tends to the induction of changes resulting in the disintegration of the tissues, is rendered probable, not only by its relation to the functional activity of the organs, which is always connected with such disintegration, but also by various facts which show ulcerative or other destructive action to be the result of abnormal stimulation of a nerve, or even of the excessive application of the normal stimuli. An interesting case of this nature is mentioned by Mr. Paget, in which obstinate ulceration of the palm of the hand was caused by pressure on the median nerve, and which healed immediately the pressure was removed. Another case is mentioned by Mr. Simon, of ulceration accompanying neuralgia of the knee. Nor can such destructive effects be attributed rather to the withdrawal than to the derangement of the nervous force; for although ulceration may occur as the consequence of the division of a nerve, there is ample evidence that it is not due to the mere loss of nervous stimulus, but either to the "irritation" consequent on the division, or to the absence of necessary protection to the organ implicated; and that the abnormal stimulus is often the cause of the ulcerative process in these cases, appears highly probable from a case related by Mr. Simon, of disease entirely destroying the fifth nerve, in which the cornea of the affected eye had ulcerated and healed again.

The view of the nervous force which refers its origin to retrograde metamorphosis receives confirmation from various facts which, upon any other hypothesis, are difficult of explanation. Such are—

1. The increased proneness to functional activity which (with certain limitations) always coexists with diminished vital power, and is implied by the expression that *irritability is proportionate to debility*.

2. The phenomena of certain diseases: as tetanus arising from the disorganizing changes caused by a wound, in a debilitated constitution; or those cases of epilepsy in which the cause of the convulsion appears to be merely the mechanical irritation of spiculæ of bone pressing upon the nervous tissue, and the more permanent convulsive action connected with that retrograde change in the brain which is denominated red softening. And lastly, the fact that the mere destruction of the central ganglia, as by crushing or other mechanical violence, induces a vehement exhibition of nervous energy.

II. An examination of the conditions which determine muscular contraction will show them to be in perfect conformity with the principles laid down. The proposition affirmed being that the motor power of a muscle is simply an expression of the state of chemical tension in which it exists, and that its contraction is the immediate result of a change of composition ensuing whenever the vital state which maintains such tension is, within certain limits, thrown into abeyance.

When placed beneath the microscope, the ultimate muscular fibre is seen to contract first at any spot where it has been broken or otherwise subjected to injury. The slightest mechanical irritation, even the presence of the least particle of matter, determines a local contraction, as also do chemical reagents and water. The contact of the atmosphere, which we know, from the history of subcutaneous wounds, to have a lowering influence on the vitality of the internal tissues, excites irregular contractions on the surface of an exposed muscle.

In cases of protracted phthisis, or other diseases attended with exhaustion of the vital power and emaciation, contraction of the muscles arises with increased facility, and may be visibly excited by a light blow upon the muscles of the thorax.

And during vigorous life, the stimuli which best excite the action of the muscles are precisely those which most powerfully evoke their inherent tendency to change of composition. The nervous force has been shown to stand in a special relation to such change. Electricity, which as a muscular stimulus ranks second to it in power, stands first among the physical forces as a promoter of chemical change, and manifests its opposition to the vital force by the instant death which accompanies its excessive action; by the coldness, pallor, and depression of vital energy which follow its local application in a powerful form; and the more speedy putrefaction of muscles which have been electrified immediately before or after death.*

The phenomena of post-mortem contraction of the muscles are, perhaps, not strictly comparable with those of their living action. It may be doubted whether they are facts of the same order; but so far as the former are available for illustration of the latter, they entirely support the view that contraction depends upon a diminution of the vital resistance, allowing to a limited extent the play of chemical affinity.

The simplicity and adequacy of this theory are well exemplified by its bearing upon the dynamical problem involved in the motion of the heart. Of the various extraneous forces to which the maintenance of its action has been assigned, all have been rejected by Dr. Carpenter, who prefers to regard—

"An alternation of contraction and relaxation as the characteristic and constant manifestation of its vital activity. . . . . Just as the Leyden jar," he adds, "may be so charged with electricity as to discharge itself spontaneously, so it is easy to conceive that a muscle may be so charged with motility or motor force as to execute spontaneous contractions."†

A few considerations will show that this hypothesis cannot be accepted as a correct representation of the action to which it relates.

For, in the first place, the motion of the heart or any muscle (as Dr. Carpenter himself represents the case) is not a *manifestation* of the vital force, but a *conversion* of it. And such conversion cannot occur without a preceding change in the conditions under which the force exists. To suppose it to take place spontaneously, is to suppose a material change to originate itself; an effect without a cause.

* The varying effects of electricity upon the muscles according to the direction of the current and other circumstances, are perhaps not yet entirely explicable upon any general principle. It is believed that they are not more difficult of explanation upon the view maintained above, than upon any other hypothesis.

† Human Physiology, p. 476.

Again, the words "motility or motor force" are most unhappily wanting in precision; and whether they be held to mean actual motion, or capacity for motion, the idea seems to be alike inapplicable.

The illustration, also, adduced by Dr. Carpenter does not assist his argument. In the Leyden jar, electricity received from without is accumulated by resistance, and transmitted when the resistance ceases, either being neutralized by the use of the discharger, or overpowered by the excessive accumulation of the resisted electricity. That is, as if a real momentum of motion were imparted to the muscle, and stored up within it by resistance, until it had accumulated to a sufficient intensity. But the heart, on Dr. Carpenter's view, is in no such case: no account is taken of any force resisted; the entire process is a continuous development of one force, suddenly altering its character and mode of operation.

The deficient element is the force which determines this sudden change from a form of action which builds up the living tissue to one that disintegrates and destroys it. The chain is broken at that point; but the recognition of the two forces which inhere in every part of the animal body, at once supplies the wanting link. The heart, like every living muscle, is charged with force, not motor or contractile, but chemical. The chemical affinity of its elements, resisted by vital or nutritive action, accumulates within it, creating a state of tension and proneness to action, precisely such as exists in the Leyden jar. The comparison is just, though incorrectly used. Muscular contraction from a stimulus is the analogue of the electrical discharge by means of metallic contact, in which the resistance is removed; and the spontaneous contraction of the heart is parallel to the spontaneous discharge which ensues when the resistance is too weak.

An adequate account of the facts appears to be conveyed by the following statement. In the muscular structure or nervous ganglia* of the heart, the chemical and vital forces are so balanced, that they assume a state of alternating activity. It might be said that the vital force exists in large quantity, but of low intensity. Hence, when, by the process of nutrition, the chemical affinity has been accumulated to a certain amount, it overpowers the vital resistance, and that chemical change which is the cause of contraction ensues. And the same series of changes continually recurs, because the vital state is constantly renewed. It is possible that the maturity of the cells which constitute the muscular fibre, being accompanied by a failure of their vital power, may give the occasion for the ascendency of the chemical force; but the phenomena of voluntary muscular contraction, and the fact that the heart's action is often more rapid in proportion to the debility of the vital power, seem opposed to such a view. The action may be roughly compared to the alternate formation and decomposition of the ammoniuret of mercury in the course of an intermittent electric current.

In the foregoing remarks, it has been assumed that the vital force is characterized by a varying intensity of action. In proof of this law, it is sufficient to refer to the normal succession of the sleeping and waking states. The heart may be said to wake and sleep with each recurrence of its beat.

* There are many circumstances which favour the idea that the action of the heart is dependent upon the ganglia contained in its substance.

With regard to the mode in which chemical change of the muscular tissue effects its contraction, nothing certain is known. There is no difficulty, however, in the conception of such a causal relation, since the production of mechanical force by means of chemical action is one of the most familiar of facts, and the muscular structure may, without any violence, be regarded as a mechanism adapted for the development of mechanical effects from slight changes of composition.

III. With regard to the process of secretion, there is ample evidence that it depends upon a modified exertion of the *chemical* affinities. The following facts may be referred to:

1. The lower composition of the secreted fluids. In the case of the great mass of the secretions, including those of a nutritive character (as the milk), this less vitalized constitution is evident, and the seminal fluid, there is reason to believe, is no exception. To what, e.g., but an exercise of chemical affinity can the formation of sugar by the liver be referred?

2. The dependence of the secretive action upon the same stimuli and general conditions as the other functions, and especially upon the nervous force.

3. Its promotion by the local application, or presence in the blood, of medicinal or other substances, the influence of which cannot increase, but must tend to diminish the vital resistance of the organs. It is not unlikely that in some instances the secretive action is normally maintained by the decomposing influence upon the gland tissue of substances, themselves in a state of decomposition, circulating in the blood.

4. An over-stimulation of secretion leads directly to destructive and anti-vital changes. Thus, as Mr. Paget has observed, the first stage of inflammation appears to be merely an increase of secretion. Salivation runs on to ulceration. One effect of destructive agents applied to the surface of the body, as a burn, severe pressure, or chemical irritants, is to induce secretion.

5. Professor Graham has rendered it probable that the passage of osmotic currents through animal membranes is dependent upon slight decomposing changes taking place in them.

6. Secretion may continue after death, being then analogous to the post-mortem contraction of the muscles.

The production of electricity and of light must be enumerated among the animal functions, but it will be sufficient merely to allude to them. There is no cause to which they can be referred with more probability than to chemical changes in the electrical and luminous organs. And the generation of electricity is known to be determined, like the other functions, by any stimuli which tend to overthrow the chemico-vital equilibrium, either in the organs themselves, or those portions of the nervous system which supply them.

The view of the vital functions advocated above has many important bearings upon special branches of physiology and pathology, which cannot now be enlarged upon. The great advantage which seems to result from it is the simplification it effects in the conception of the vital force itself. One whole division of what under other views is considered as vital action, being thus transferred to the domain of chemical agency, the idea

of the vital force stands out clear and distinct before the mind as the peculiar molecular action which forms and nourishes the living body. That is its nature; that its entire scope. Thus, by resistance, it accumulates chemical force, and furnishes the conditions under which THE FUNCTIONS—motion, nervous action, secretion—exhibit themselves as the results of chemical affinity.

And the idea of the animal body, the fundamental conception or plan on which it has been framed, appears to be simply that on which we ourselves act when we wish to construct a machine. We use one modification of force as a resistance to another, privileged herein with the power of imitating, at an infinite distance indeed, the sublimest of the material works of the great Creator of all things.

And further still, this view of life opens to us yet another indication of the unity of principle that binds creation into one. On earth we see the antagonism of two forms of force yielding a well-nigh boundless variety of beautiful, useful, and happy action in the successive grades of animal existence. In the heavens, the antagonism of two forms of force develops the regular motions of the planets, and constitutes the law which ordinates the universe.*

Art. III.

Algeria: its Climate and Merits as a Resort for the Invalid. By Arthur Mitchell, A.M., M.D.

(*Concluded from No.* 33, *p.* 226.)

"As every country possesses its characteristic vegetable and its characteristic animal kingdoms, so also it possesses its pathological, it has its diseases peculiar to itself, and enjoys an immunity from others."† Or, in the words of our quaint Sir Thomas Browne, "Death hath not only particular stars in heaven, but malevolent places on earth, which single out our infirmities, and strike at our weaker parts."

It is not my intention, however, to enter into the investigation of the whole *pathogenic* features of the climate and soil of Algeria. To one class of diseases my attention has been especially directed—those of the respiratory organs. And it is my present object to adduce and analyse the testimony which supports the belief that pulmonary phthisis is rare in North Africa. Nor is this belief of recent origin, for according to Brunache‡ the consumptive patients of Celsus were sent by him to Egypt and the Mediterranean shores of Africa. Only from the date of the French occupation, however, have statistics been brought to its support.

* I have perhaps failed to indicate with sufficient clearness that the production of functional action by chemical change depends upon the mode in which such change takes place. It is not every decomposition in the living body that necessarily results in a function, but such changes only, and changes of such intensity, as are adapted to act upon the functional mechanism. In a steam-engine it is not every possible expansion of the steam that causes a revolution of the wheels, but only an expansion which takes place in a sufficient and yet limited degree, and in a special direction. In the animal, passive decay of the tissues, as of an unused muscle, and excessive decay, as in some forms of disease, do not cause, but abolish, function.

† Boudin: Traité des Fièvre Intermit., p. 69.

‡ Géog. Méd., par Boudin, p. 21. 1846.

The medical men of the army seem to have been early impressed with it, for nearly twenty years ago (in 1836, only six years after the invasion of the French) the subject was brought prominently before the Academy of Medicine of Paris by Dr. Costellat, who proposed to found an establishment at Algiers for phthisical patients.* On the ground of the statistical element being deficient, the discussion of the Academy terminated with this vote: "It is doubtful if the climate of Algiers can favour the cure of consumption."

This discussion, and the announcement by M. Boudin, in 1840, of his theory of antagonism between tubercular disease and marsh fever, directed the attention of all the French physicians who visited Africa still more particularly to the subject. Hence an amount of evidence has been steadily accumulating in the medical journals to which these men communicated their various experiences. Sometimes their opinions are supported by figures, at other times not; but in all cases they have their measure of value.

From these sources, then, and from the more extended works of Armand, Bertherand, Foley, Martin, Boudin, &c., as well as from private sources, and from the registers of the military hospitals, to which access was given me, I have collected the information which I shall bring to bear on the elucidation of the question.

I have embodied almost all the statistical portion of this evidence in the following table (p. 196), which will form the basis of my conclusions.

This table actually embraces the statistics of about 150,000 cases of disease treated, and upwards of 20,000 deaths; but it will be observed that sometimes the number of cases treated is given, with the proportion of phthisis included, without giving the number of deaths; and very frequently the reverse. If the deaths to those treated were always in the same proportion, this table might be said to represent about 380,000 treated and 27,000 dead. But I shall make no such assumption. Each aspect of the question must rest on the evidence, which is complete as regards itself. The basis is wide enough in all cases to admit of this. Nor in reality will the value of the conclusions be thus weakened, though otherwise a seeming strength might be given them by the introduction of large but fictitious figures.

What proportion, then, do deaths from phthisis bear to the deaths from all causes? This query has to be answered from different points of view. We have an indigenous and a foreign population, and the influence of race has to be ascertained. We have a coast district and an interior, and the influence of residence has to be inquired into. Fortunately, the following table supplies materials for such a reply.

And first, over all classes of the population, and without respect to place of residence—civilian and military; European, Arab, and negro; in hospital and in their own houses; on the coast and in the interior—the table shows that† 20,955 deaths from all causes included 759 deaths from phthisis—being about 1 in 27·6, or 3·6 per cent.

Again, over the European civil population of Algiers, in hospital and their own houses, out of 9262 deaths from all causes, we have 441 from phthisis—equal to 1 in 21, or 4·8 per cent.‡

* Médecine des Arabes, p. 524. E. L. Bertherand.

† From all the data in the table except No. 19.

‡ Nos. 1 & 2 of table.

Authority.	No. of cases treated.	No. of cases of pulmonary phthisis included in these.	No. of cases of diseases of the respiratory organs included in these.	No. of deaths from all causes.	No. of deaths from phthisis included in these.	No. of deaths from diseases of the respiratory organs included.
1. Algiers. Civil Hospital. For the people of the town and plains. 11 years: 1837 to 1817. Furnished to me by Dr. Foley, from his private records	—	—	—	4343	241	553
2. Algiers. European population in their own houses. 5 years: 1843 to 1847. Collected by the Government. Furnished to me by Dr. Foley	—	—	—	4819	200	733
3. Algiers. Mussulman population. 5 years: 1843 to 1847. Collected by Government. Furnished to me by Dr. Foley	—	—	—	3666	130	515
4. Algiers. Mussulman population. 4 years: 1838 to 1841. 'Méd. des Arabes,' p. 525. E. L. Bertherand	—	—	—	3177	78	—
5. Algiers. Military Hospital. 1852, 3, 4. Extracted from the Registers by Dr. Mitchell, with the permission of Dr. Bertherand	19,738	—	—	1044	42	81
6. Algiers. Hôp. Salpêtrière. 1845. Dr. C. Broussais. 'Mém. de Méd. Mil.,' tom. v. p. 60	1047	12	—	63	4	—
7. Algiers. Invalided soldiers returned to France: 1852, 3, 4. Extracted from the records of the Hôpital du Dey, by Dr. Mitchell	1513	15	—	—	—	—
8. Bone. Grat. Consult. 1843 to 1852. Dr. Moreau. Obtained through Dr. Bertherand	50,712	72	3030	—	—	—
9. Oran. M. le Dr. Marseilham. 1838, 9. 'Mém. de Méd. Mil.,' tom. v. p. 52	5578	—	778	544	—	74
10. Orleansville. Civil and Military Hospital. 1852. Dr. Barby. 'Mém. de Méd. Mil.,' tom. xii. p. 149	—	—	—	1376	22	—
11. Tlemcen. M. le Dr. Catteloup. 1842 to 1853. 'Mém. de Méd. Mil.,' tom. xii. 187	12,851	16	—	1008	12	—
12. Tlemcen. 1842. Dr. Cambay. 'Mém. de Méd. Mil.,' tom. lvii.	2698	17	—	198	3	—
13. M. le Dr. Godelier. 'Mém. de Méd. Mil.,' tom. lix. p. 37.	40,341	62	—	—	—	—
14. Garrison of Sidibel.-Abbés. Dr. Froissart. 1843, 4, 5, 6. 'Mém. de Méd. Mil.,' tom. lxiii. p. 103	—	—	—	229	2	—
15. Blidah. Dr. Finot. Civil and Military Hospital. 1840, 1, 2. 'Mém. de Méd. Mil.,' tom. lvi. p. 119 ...	9678	5	—	798	10	—
16. Blidah. Civil and Military Hospital. Dr. Laveran. 1840. 'Mém. de Méd. Mil.,' tom. lii....	1465	9	—	110	7	—
17. Blidah. June to Dec. 1851. Extracted from Hospital Registers by Dr. Mitchell, given to him by Dr. Laveran	933	4	44	44	1	6
18. Medeah. M. le Dr. Rietschell. 1841. 'Mém. de Méd. Mil.,' tom. lv.	777	—	33	33	—	—
19. Milianah. Dr. Bruguière. 1841. 'Mém. de Méd. Mil.,' tom. lvi.	807	9	—	—	—	—
20. Constantine. Civil and Military Hospital. Nov. 1839 to June 1840. Dr. Deleau. 'Mém. de Méd. Mil.,' tom. lii. p. 236	107	—	—	8	2	—
21. Constantine. European population. Dr. Deleau. 'Mém. de Méd. Mil.,' tom. lii. p. 236. 1838, 39, 40	—	—	—	89	5	—

And of the Mussulman population of Algiers, 6843 deaths embraced 208 from phthisis—giving 1 in 32·9, or 3 per cent.*

* Nos. 3 & 4 of table.

While of the military population in hospital at Algiers, 46 deaths from consumption correspond to 1107 from all causes—having the proportion of 1 in 24·1, or 4·1 per cent.*

Turning now to the influence of locality, we have, out of 17,112 deaths from all causes on the shores of the Mediterranean, 695 arising from pulmonary phthisis—or 1 in 24·6, or 4 per cent.†

While from the statistics of the interior, which unfortunately do not reach a high figure, although they embrace Tlemcen, Blidah, Orleansville, Medeah, Milianah, Constantine, &c., out of 3843 deaths from all causes, we have only 64 from phthisis, or 1 in 60.‡

As regards, then, the relation of deaths from this to deaths from all other diseases, when we compare Algeria with Great Britain and France, we are left in no doubt or difficulty about the conclusion. The line of difference is wide and apparent. Phthisis is the scourge of the latter countries, and but an exceptional visitor in the former.

Of course the value of the conclusion is to be estimated by the value attached to the data on which it rests. When these are still more extended, the relation may be changed a little in either way. I say *a little*, because I think the basis already so broad as to permit no inference that is glaringly erroneous to spring from it. The statistics stretch over seventeen years, and embrace various localities and various races; they were taken from hospital and house practice; they were collected by various observers, and seldom, if ever, with any direct bearing on this question; and they are not an aggregate of those in favour of a particular view, but comprise every figure which I could find published either in serial or monograph literature, or which I could obtain through personal friendship from the hospital registers or the records of private practitioners. The same inference springing from such extensive and diversified materials, however analysed, whether made to speak in parts or as a whole, whether applied to one class or another, to one district or another, must have considerable value.

Let me, however, before further discussing the question, answer the inquiry, what proportion of those who present themselves for treatment in out-door practice, or are admitted into hospital, is made up of phthisical patients?

Out of 123,022 persons treated, 221 cases of phthisis presented themselves, or 1 in 552;§ and out of 49,494 persons treated there were 81 deaths from phthisis, or 1 in 611.‖

This appears to give increased force to the conclusion, even after allowing for errors of diagnosis which are more likely to enter into this than into the statement of deaths where the general symptoms must have been clear enough without necessitating much skill in physical diagnosis.

It would appear that this immunity is enjoyed in a higher degree by the Mussulman than the European population; and of the latter, by the military in a higher degree than the civil portion. And again, that those who live in the towns of the interior are still further removed from the chances of death by this disease than those who live on the coast. I was not prepared for this last conclusion. Indeed, I had anticipated the

* Nos. 5 & 6 of table. † Nos. 1 to 6. ‡ Nos. 10 to 21.
§ Nos. 6 to 8, 11 to 13, and 15 to 19. ‖ Nos. 5, 6, 11, 12, 15 to 18, and 20.

reverse, and I think I derived my impression from the medical men whom I met in Algiers—the general opinion being that Bone offers the greatest immunity, after it, Algiers, and least of all, Constantine.* Of all the deductions this rests upon the narrowest basis, but it must remain where it is till further evidence proves or disproves it.

The proportions at which I have arrived of course differ from those found by individual observers, because I have drawn them as far as possible from the sum of all their observations.

Thus Dr. C. Broussais, in a memoir communicated to the Academy of Medicine, gives the proportions as 1 phthisical out of 650 treated, and out of 102 dead.† His documents comprised a mass of 40,000 sick, and I have involved them in the general table.‡

The same author observes that "this disease without any doubt is much less frequent in our African possessions than in France, and the difference is so great that it evidently depends on the climate—no other secondary circumstances being capable of producing it." And he further states his belief that its progress is less rapid, and the chances of cure greater, in Algeria than in France.§

Again, Messrs. Bonafond and Guyon, from the statistics of six years of the mortality of the civil population, give 1 death from phthisis to every 40 from mixed diseases.

And again, according to M. Catteloup, the phthisics are to the entries as 1 to 803, and to the deaths as 1 to 84.||

While at Bone, M. Moreau found 1 death from phthisis in 42, and his letter to the Academy of Medicine terminates with the following conclusions:—1st. That phthisis is extremely rare among the inhabitants of the country; 2nd. That Europeans are rarely attacked by it; 3rd. That the progress of the disease is arrested in Europeans already the subject of it; and 4th. That it is far from being there a constantly fatal disease.¶

Strong as these are, the conclusions at which M. Odrultz arrives surpass them. Among others he gives the following:**—1st. The climate of Algiers is opposed to the generation as well as to the evolution of tubercle in the lungs; 2nd. This morbid production is observed but very exceptionally among the indigenous population; 3rd. Europeans who do not bring the germ of the disease to Algiers almost never become phthisical; 4th. Those who do bring not only a predisposition, but actually crude tubercle, in greater or less quantity, in the lung, are often cured; or, in the worst case, the progress is extremely slow; 5th. When the tubercle is softened, the climate is no longer favourable.

The averages of all these observers are above mine; but, on the other hand, M. Antonini thinks that about 1 in 20 of the deaths of the military population is caused by phthisis.††

This proportion is below that which results from the data of the table, but I shall not be surprised if eventually this proves to be a very correct appreciation of the proportion of deaths from phthisis over the whole population.

* Martin: Manual d'Hygiène, p. 169. † Boudin: Géog. Méd., p. 25.
‡ No. 13 of table. § Mém. de Méd. Mil., tom. ix. p. 124. || Ibid., tom. xii. p. 187.
¶ Boudin: Géog. Méd., p. 26. ** Annuaire Thérapeutique de M. Bouchardat. 1850.
†† Martin: Manual d'Hygiène, p. 167.

Dr. Martin, after telling us that he believes the medical men of Africa are unanimous about the disease being rare and altogether exceptional among the indigenous population,* gives as his own opinion that it is rare also among Europeans, with whom it "progresses so slowly as to give nature time to organize her means of defence, and consequently of cure. But, moreover, in modifying the constitution it causes it to lose the tuberculous tendency." And, in fine, nothing is more rare among Europeans than tuberculosis developed in the country.†

Drs. Armand and Laveran speak more guardedly on the subject, but the former says, "that no one can contest that among the soldiers, phthisis is less frequent in Algeria than in France;"‡ and the latter, who is an able stethoscopist, furnishes statistics proving the same.

Writing of the same subject, Boudin says:

"The rarity of diseases of the chest is such in Algiers that it has frequently happened to me to visit many hundred sick without having once occasion to apply the stethoscope. Out of a total of 12,853 sick whom I treated in the army of Africa, or in the Lazaret of Marseilles, I only encountered 31 consumptives; and of these 25 had, beyond doubt, been tuberculous before having left for the Morea or Algeria."§

I have still the testimony of another medical man to offer, that of Dr. Bertherand—the present head of the staff at the military hospital of Algiers. He communicated this in writing to me, and I shall give it in his own words:

"I have already had the opportunity," he says, "in several private conversations, of expressing to Dr. Mitchell my opinion of the happy influence which the climate of Algiers exercises both on the development and progress of pulmonary affections.

"A sojourn of more than five years in the military hospitals, in the camps, in European and indigenous towns, in Algiers, Blidah, Constantine, Setif, &c., originated and has every day strengthened the conviction—1st. That phthisis is a disease rare in Algeria; 2nd. That the climate of Algiers arrests, or at least manifestly retards, the progress of commencing tuberculisation; 3rd. That the season of great heat hurries on the work of destruction of advanced tuberculisation.

"I leave to the general statistics with which Dr. Mitchell occupies himself at present with so scrupulous a perseverance, the rigorous demonstration of my first two propositions.

"My intention in this note is to detail more minutely the particular facts of my own private practice.

"From my notes and records of cases, I find that the total number of organic pulmonary affections which I have had occasion to observe since landing in Africa, does not surpass 15.

"Of these, 10 were men and 5 women; 11 were adults, 2 below puberty, and 2 above fifty years of age; 12 were affections existing before arrival in Algeria; 5 are dead, and 10 are still living; 2 died after returning to France in November, 2 in Algiers in May, and 1 in Algiers of cholera in March. Of the 10 alive, 1 has been fifteen years in Africa, 2 have been thirteen, 6 between four and seven years, and 1 for a single year."

I cannot conclude this *résumé* of opinions on this question without citing that of my friend, Dr. Foley, the author of a statistical work 'On the Colonization of Algeria,' and the head of the medical staff of the Civil Hospital of Algiers. His experience has been lengthened and

* Martin: op. cit., p. 164.
† Ibid., p. 171.
‡ Armand: L'Algérie Médicale, p. 376.
§ Traité des Fièvre Intermit., chap. vi.

extensive, and his habits of observation are careful and minute. He frequently communicated to me in conversation his views on this subject, and they always clearly and decidedly went to say that this disease is exceedingly rare in Algiers, both among the European and indigenous population, and that when it is brought to the country, its progress is arrested, and ameliorations of a most marked character take place.

It will be observed that he furnished me with several most important statistical facts. These he extracted from valuable records in reference to the general pathology of the country, which with great labour he has collected, and which I trust he will soon publish.

Negroes in Algeria, to whom this is a northern latitude, fall frequently victims to this disease.* That they do so in Europe is a notorious fact. According to Brunache, in every autopsy he made at Marseilles on a Negro, he found tubercle. Clot-Bey tells us that in Egypt the same thing occurs. At Algiers, Dr. Martin says, "one rarely meets a case of phthisis, except among the negroes;" and Dr. Foley frequently attested the same in conversation.

The negro dies of consumption more and more frequently as you draw him from his own country. Thus, of the negro soldiers at Sierra Leone, their own country, 1·000 men gave 6·3 deaths annually from phthisis, while at Gibraltar, 1·000 gave 43·; at Honduras, 8·1; at Jamaica, 10·3.† And in America, the mortality from phthisis is twice as great among the negro as among the white population.†

Its exceptional appearance among the Arabs is all but universally admitted. In the foregoing quotations this is frequently expressed, and the table is strongly affirmative.

In addition to this, Dr. Grellois, writing in 1846, says "that he has not seen a single case of phthisis among the Arabs, nor has a single case figured in the Hospital of Ghelma since the 1st of January, 1844."‡

In an old Arabic work on medicine, which M. Pharaon and Dr. Bertherand are at present translating, they tell me that no disease having cough for a symptom is even mentioned.

The Grand Mufti of Algiers frequently told me that the disease was almost unknown among his countrymen.

That it does exist, however, statistics show. Both Bertherand and Armand notice it in their works. The Arabs call it *Meurdh dhaf*,§ or "the disease of weakness," and they believe it to be contagious. The syphilitic cachexy, so prevalent, may be a predisposing cause.|| The actual cautery on the thorax, irritating plasters, Moorish baths, baths of hot sand, exposure to resinous vapours, vegetable infusions with honey, &c., are the remedies most in use among them.¶

Armand** gives an interesting conversation which he held with an Arab doctor, leading to the inference that the disease is *very* common. But this is unmistakeably an exaggeration on the part of the worthy Tebib, and Armand is himself evidently of this opinion—stating that among the Arabs he has much more frequently met with hepatic than pulmonary phthisis. Condensing the colloquy, it runs somewhat thus:

* Méd. des Arabes, p. 524.
† Boudin: Path. Comp. pp. 19, 20.
‡ Mém. de Méd. Mil., vol. lx.
§ Bertherand: Méd. des Arabes, p. 523.
|| Ibid.
¶ Ibid.
** L'Algérie Médicale.

Speaking of diseases of the chest—"Thou knowest," said Ben Chaoua, "that they are common here."

"Thou knowest," said Armand, in turn, "that there are *colds* in the season of the rain, which are not serious diseases."

"Yes," was the reply, "but these colds, without being grave, are in summer sometimes very obstinate."

"It is very true." [He spoke, no doubt, of bronchitis.]

"But there is another disease of the chest," continued Armand, "where the patient cannot cough, or at any rate the cough is small and dry, and very painful in consequence of a stitch in the side."

"There," was the rejoinder, "you have a more serious disease, with fever—I know it."

"There is still another," said Armand, "where there is great fever, no stitch, cough, great oppression, and expectoration mixed with blood."

"I know the disease," replied Ben Chaoua; "when it is not well cured, the patient dies in the long run, always coughing."

"In fine," said Armand, "there is yet another chest disease, which shows itself especially in the young and feeble, the pale, the ill-fed, ill-lodged, and ill-clothed, and more certainly if their parents have had the same disease. They begin by coughing a little, then more, sometimes with blood in the expectoration, fever takes place, and night-sweats, the spit becomes thick and puriform, and sometimes comes up as if vomited, and they die off by a decline, giving up the last breath suddenly, sometimes in speaking, sometimes in eating."

"Oh! this disease is very common among the Arabs," said the Tebib.

"Very common?"

"Yes, more so than fever or dysentery."

After having satisfied himself that M. Jacob, the interpreter, had faithfully done his task, and that Ben Chaoua was not talking of some other ailment, M. Armand asked him how he treated such cases.

"Oh! you French medical men torment the sick by bleeding, blistering, and plastering, and you leave them to die of hunger."

"Very much obliged," interpolated Armand.

"The empirical Arabs do just the reverse."

"And what may that be?"

"When a young man," said Chaoua, "perceives that he has this disease, he gives up every occupation; eats everything he fancies, using strong spices and drinking strong liquors; he takes exercise on foot and on horseback; he gives himself up to women;" &c.

"That is to say, he follows a life which would kill a robust man. He ought to finish his existence soon by such a course."

"There are those who are cured by it," was the sententious reply.

"You think so?" queried Armand.

"I am certain of it."

The conversation is more curious than valuable. It does not give the opinion of a Rhazes or an Avicenna, but the little known Ben Chaoua. And an uneducated Tebib and an unprofessional interpreter are bad elements. Nevertheless, it will be observed with interest that the principle of the "sustaining treatment" of the present day—the cod-liver oil, beef-steaks, porter, and open air—exists there, although abused, it is true.

We are naturally led here to the comparison of the mortality from phthisis in Algeria with that in other countries, and it cannot be done more briefly or clearly than in a tabular form.

The Mortality from Phthisis compared with the Mortality from all Causes in different localities.

		Proportion.	
		Deaths from phthisis.	Deaths from all causes.
Algeria	All classes of population	1 ...	27·6
	Europeans—civil	1 ...	21·0
	Europeans—military	1 ...	24·1
	Mussulmans	1 ...	32·9
London*		1 ...	8·1
England and Wales†		1 ...	5·3
Paris‡		1 ...	5·0
French Army§		1 ...	5·0
Marseilles‖		1 ...	4·0
Genoa¶		1 ...	6·9
Nice**		1 ...	7·0
Naples††		1 ...	8·0
Gibraltar, Malta, and the Ionian Isles‡‡		1 ...	3·8
New York§§		1 ...	7·2
Boston§§		1 ...	6·6
Baltimore§§		1 ...	5·4
Charleston§§		1 ...	6·9

Phthisis, then, must become greatly more prevalent in Algeria before it stands on a parallel with Europe or America.

It may be alleged that, in consequence of the great prevalence of other diseases, it is possible that tubercle may exist in the lungs of the many who fall under fever or dysentery, but that they are cut off before it manifests itself or calls attention.

Many facts in my possession go to the removal of this objection.

Out of 1104 autopsies performed at Tlemcen by Dr. Catteloup, in which tubercle was sought for in the lungs, it was found in 88, being in the proportion of 1 to 13. These had died of the following diseases:

Dysentery	71
Marsh cachexy	4
Chronic bronchitis	2
Hepatic affection	2
Cholera	5
Different diseases	4
Total	88

Again, M. Broussais mentions that only 3 cases of tubercle were found in the autopsies of all those who had died of acute disease or of any other disease, except phthisis; while in Paris, and in France generally, under like circumstances, he says that the occurrence is frequent.

* Col. Sykes: Statistics of Nice, p. 36.
† Craigie: Practice of Physic, p. 1000.
‡ Broussais: Mém. de Méd. Mil., tom. lx. 124; and Andral and Boudet.
§ Benoiston de Chateauneuf.
‖ Boudin: Géog. Méd., tom. xxxiv.
¶ Sykes: op. cit., p. 36.
** Andral: op. cit.
†† Andral: Traité de Path. Intern., tom. i. p. 449.
‡‡ Godelier: Mém. de Méd. Mil., vol. lix.
§§ Swett on Diseases of the Chest.

And Dr. Leclerc, who was stationed at Cherchell for six years as *chef de l'hôpital*, stated to me that he had while there some 500 or 600 necropsies; and among these the presence of any tuberculous deposit in the lungs was not established in above 5 or 6, or about 1 per cent. From particular circumstances, he said, the inmates of this hospital were entirely composed of those who had been resident in Africa for a considerable time, and in whose cases, therefore, the climate had had time enough to manifest its influences. In France, with a similar medical charge—that is, with the same class of men—he said that he found tubercle in nearly one-third of the autopsies:

"I cannot but believe," he added, "that many of these men arrived in Africa with latent or nascent tubercle in the lung, although apparently, perhaps, in good health: from which it would appear, that not only does the climate of Africa prevent the development or arrest the progress of the disease, but that there ensues a more radical change—the actual disappearance or removal of the deposit."

It is worthy of remark, that Dr. Leclerc himself labours under chest affection, and has now selected Algiers as his residence, from his high opinion of the climate.

The reasoning of Dr. Leclerc must be correct, if it be true, as asserts M. Boudet, that five out of seven of the population of Paris have tubercle in some form and to some extent in their lungs.

M. Boudin tells us that five persons returned to France on account of dysentery, who, before their sojourn in Algeria, had presented the characteristic symptoms of phthisis; and who, on their return, appeared to him to be entirely cured of this affection. Two of these five died eventually of disease of the large bowel; and the necropsy confirmed the absence of tubercle in the lung as well as in other organs.*

Brunache cites three instances of persons who, with decided symptoms of tubercle in the lung, went to Algiers, and enjoyed good health while there; but in whom, on their return to France, chest symptoms reappeared, and death ultimately ensued from consumption.

Another objection which may be brought against the validity of the statistics is this—that, among the military at least, a large proportion of those who are sent home invalided consist of phthisical patients.

To remove this, I examined the hospital registers for three years (1852-3-4), and with the following result:—Out of 1513 who obtained *congés*, there were 15 cases of consumption; and the 1499 consisted of those suffering from the effects of fevers, dysentery, accidents, &c., &c. This gives just one phthisical per cent. of the invalids returned to France.

It is possible, however, that the whole mortality of Algeria is so high, that although phthisis may bear a small proportion to the whole number of deaths, it may still bear heavily on the effective of the population.

There is no doubt that the rate of mortality over the whole population of Algeria is higher than on the continent of Europe or in Great Britain. It is difficult, if not impossible, however, to give the exact proportion, as it has been so variously estimated by the friends and adversaries of colonization. Statistics of course exist, and the result I shall now give.

The general mortality of Europeans in Algiers, from the 'Tableaux de

* Boudin: Géog. Méd., p. 39.

la Situation des Établissement Français' (official documents), has given an average since the conquest of about forty-four to forty-five deaths out of 1000 individuals.* According to Martin and Foley, many circumstances make this a high statement: amongst others, the including of the still-born, who constitute in Algiers one out of every eleven births.

For the town of Algiers itself, from the same official documents, an average of eighteen years gives 43·5 out of 1000.† But the deaths in Algiers represent a larger population than that of the town, since its large hospitals receive the sick from the villages in the adjoining plains. If this and other sources of error be deducted, according to Martin and Foley, it is reduced from 45 to 31·5 out of 1000 of the population.‡

Among the military population, 1848 showed the deaths to the effective as 36 to 1000. But here again, as colonists and natives were admitted in considerable numbers into the hospitals which furnish the statistics for the above, and ought therefore to be deducted, according to the same observers the mortality will fall from 36 to 24.

I am well aware that M. Boudin, M. Desjobert, and the opponents of colonization in Algeria, keep it at the figure I first stated; but from a perusal of the works on both sides, I feel inclined to think that they somewhat over-estimate it, and that the smaller proportion is nearest to the truth.

Comparing, then, the general mortality here with that in Europe, we have—

For Algeria, European civil population . . .	31·5 (or 45) in 1000
„ Ditto European military population . .	24·0 (or 36) in 1000
„ France, civil§	24·0 „
„ Ditto, military§	19·0 „
„ Prussia‖	30·6 „

There is clearly, therefore, no such excess of the general mortality as to account for the small proportion which deaths from phthisis bear to the deaths from all causes.

Diseases of the Respiratory Organs generally.

Though phthisis is rare, it does not necessarily follow that other diseases of the respiratory organs are also rare. This is another and an important aspect of the question. The original table leads to the solution of this also.

I. Of the European population of Algiers in hospital and in their own houses, 9262 deaths include 1286 from diseases of the respiratory organs, or 1 in 7·1, or 14 per cent.¶

II. Again, of 57,223 cases treated in hospital, dispensing, and private practice, 3852 laboured under some form or other of chest affection, which gives the proportion of 1 in 15·1, or 6·6 per cent.**

III. And again, 26,249 cases treated, gave 161 deaths from disease of the respiratory organs, or 1 in 163.††

* Foley et Martin: Histoire Statistique de la Colonisation, p. 323. † Ibid. ‡ Ibid.
§ Desjobert: Documents Stat. sur l'Algérie, p. 2.
‖ Stat. de la Pop. de l'Europe, p. 61. ¶ Nos. 1 & 2 of table.
** Nos. 8 & 9 of table. †† Nos. 5, 9, & 17 of table.

Before commenting on these results, let us ascertain what particular diseases of the respiratory organs compose these numbers.

And, first, of those who died of miscellaneous diseases of the chest, we find that out of a total of 1882,*

Phthisis	gave	613
Chronic and acute pneumonia	„	431
Chronic and acute bronchitis	„	396
Pleuritis	„	144
Pleuro-pneumonia	„	55
Empyema, emphysema, and hæmoptysis	„	5
Thoracic affections not named	„	238
Total		1882

This total corresponds to 13,872 deaths from all causes.

Secondly, of those who were treated for miscellaneous diseases of the respiratory organs, an aggregate of 3030 is divided thus: †

Phthisis	72
Pneumonia	61
Bronchitis	1987
Pleuritis	359
Croup and laryngeal disease	20
Hooping cough	235
Influenza	62
Hæmoptysis, angina, asthma, asphyxia	234
	3030

Corresponding to 50,712 treated for all diseases.

Are these proportions greater or smaller than in Europe? They are much smaller, as will be seen from the subjoined table.

Proportion of Deaths from Diseases of the Respiratory Organs to Deaths from all causes in different places.

In Algeria	14·0	per cent.
‡In Paris	33·0	„
§In London	31·5	„
§In Nice	25·1	„
§In Genoa	31·0	„
§In Turin	38·2	„

And according to the statistics of M. Trébuchet, one-third of the whole mortality in Paris is made up from the three diseases—phthisis, pneumonia, and bronchitis (catarrh). Thus from ten years' observations,

Phthisis	gave	an average of	4261	deaths	annually.
Catarrh	„	„	2222		„
Pneumonia	„	„	2634		„
			9117		

Or one-third of the whole average mortality.‖

* Nos. 1, 2, 3, 5, & 17. † No. 8.
‡ Trébuchet: Annales d'Hygiène, vol. xlvi. § Sykes: op. cit.
‖ Trébuchet: Annales d'Hygiène, vol. xlvi.

A remark of Dr. Martin, in reference to the progress of one of these diseases, is worthy of notice. He says that he has never seen a case of pneumonia originating under his own observation in Algeria, pass into phthisis; and he concludes from this, that there is some antipathy between the climate of Algiers and the generation of tubercle.*

Influence of Season on Deaths from Phthisis in Algeria.

The influence of season on the mortality by phthisis is not without interest, and to some extent I am able to indicate it. Six hundred and thirty-six deaths by this disease were thus divided over the year:

January	65	145	July	51	164
February	33		August	57	
March	47		September	56	
April	56	160	October	61	167
May	53		November	55	
June	51		December	51	

The general impression, I think, is, that August, September, and October are the months most fatal to phthisical patients; but the above table does not point to any month or months as being especially fatal, although the first three months of the year are certainly, on the whole, the least fatal.

In France it is otherwise. According to M. Trébuchet's statistics for Paris, between 1839 and 1849, March, April, and May are decidedly the most fatal, and September, October, and November the least so.†

The general mortality in Algiers, however, is greatly diminished during the first three months of the year; and twice as many are treated for all classes of disease during the last half of the year as during the first (83,379 and 39,913).‡

As far as *surgical practice bears upon this question*, I learned from Dr. Bertherand, that almost no surgical case presents itself arising from a scrofulous habit of body. "*White swelling*," he said, "I may say, never appears amongst the soldiers in Africa—certainly not one-tenth of the number of such cases we meet in similar hospitals in France." And to the note giving me his experience of phthisis, he appends the following: "Wounds of the chest by fire-arms are recovered from in Africa in a proportion truly surprising. The cause has been too exclusively assigned, I think, to the smallness of the balls used by the Arabs; and it has been too much lost sight of, that the gravity of these wounds does not altogether depend on the injury, greater or less in extent, which the projectile produces in the lung, but more especially on the splinters of bone, &c., which it takes with it. Be this as it may, however, the fact itself cannot be denied." He then details numerous instances of recovery under his own care, and concludes with this query: "May not a part of these happy terminations be, with justice, attributed to this—that the injured portion of the parenchyma is acted on here by an atmosphere less irritating than in other latitudes; and by inference, may we not draw from

* Martin: op. cit., p. 171. † Annal. d'Hygiène, vol xlvi.
‡ Finot: Mém. de Méd. Mil., vol. lvi.

the fact, some favourable considerations in the question of the organic pathology of the lung?"

I have throughout the whole of the discussion of this subject rather stated the opinions of others than delivered any of my own. It has been my wish simply to expose the evidence, and I have endeavoured to do this as fully as possible.

I have offered no probable explanations. It may be a question of climate or of soil, or of both, or of neither. With this I have not dealt. My object was to ascertain on what the belief rested, to what extent it existed, and how far it was supported by statistics.

It would appear, then, that although on some points of the pathology of Algiers, difference of opinion may exist among the medical men who have had to do with disease there, on one they are all agreed,—and that is the one we have discussed. Some hold it more moderately than others, but all believe in the truth of the general statement, and if *quod ab omnibus creditum est, verum est,* we must receive it too.

My own summing of the evidence would be this:—

1. That the foregoing figures and expressed medical opinions authorize us in concluding that phthisis is a disease considerably rarer in Algeria than in Europe or North America.

2. That on the same testimony we are also warranted in concluding that other diseases of the respiratory organs are less frequent in Algeria.

3. That from the extent and character of the evidence adduced, it is probable that these general inferences will stand the test of future inquiry.

I confine myself to these, and therefore within narrower bounds than the French physicians have usually done. Yet it is true that there are certain points of this evidence which support the view that in Algiers the development of tubercle is prevented to some extent in those predisposed to it, and that in those in whom it already exists—while in an early stage—the progress of the disease is arrested, and such a complete disappearance of the general symptoms induced, as to be called *a cure.* Such a conclusion is much more uncertain than are those I have drawn, which must be considered the only legitimate ones. But though it does rest on a less satisfactory basis, one would almost wish to believe that it contained a measure of truth, and that thus the unfortunate who seeks Algiers labouring under that ailment, "wherein," as Sir Thomas Browne hath it, "with us it is as dangerous to be sentenced by his physician as by a judge," may not "fruitlessly be put in hope of advantage by change of climate." We may add with the same author: "He is happily seated who lives in places whose air, earth, and water promote not the infirmities of his weaker parts, or is early removed into regions that correct them."

Art. IV.

Clinical Observations on Hare-lip. By Hermann Friedberg, M.D., Lecturer on Surgery and Forensic Medicine in the University of Berlin.

In the ensuing pages I propose presenting to the reader a few cases of hare-lip which occurred in my surgical wards, and offered circumstances of peculiar interest. I shall first relate a few cases in detail, and then only advert to the leading features of others, in order to illustrate the proceedings which I have adopted in operating for this malformation, and also for the purpose of weaving in or adding some physiological and surgical remarks. Thus, to the first case I have appended observations on the treatment of the inter-maxillary bone in operating for hare-lip, and on the spontaneous cure of fissured palate after this operation. After Case 2, I have examined the relation which the hare-lip bore to the co-existing malformation of the basis. I have taken the opportunity of at the same time investigating the abnormal state of the eyes presented in this case. The formation of the red margin of the lip has also appeared to me to merit a careful consideration. In going over this ground, I have first had regard to the ordinary method of operation, and have then adverted to that adopted by Pétrequin, Mirault, and Malgaigne, for the purpose of obviating the angular inflection of the red margin of the lips, which is so apt to result from the ordinary proceeding. I have next illustrated by several cases my own mode of procedure, which consists in forming an arched projection with a broad base under the point of junction of the lateral parts of the lips, and to form the edge of the lip by transplanting the red margin. Lastly, I have given a summary of the principles which should guide us in the choice of a method for the formation of a red border of the lips.

Case I.—Hare-lip on the right side, involving the palate; absence of the right half of the upper lip; the nose disfigured by the lower half being pushed over to the left side. Operation for the lip performed fourteen hours after birth. Repair of the right half of the upper lip by transposition of a flap excised out of the cheek; the margin of the lip formed by making an arched button or projection with a broad basis, below the point of juncture of the lateral parts of the lip; detachment of the inferior half of the nose from its insertion, for the purpose of straightening it; cure; gradual retrocession of the inter-maxillary bone, and spontaneous closure of the palatal fissure.

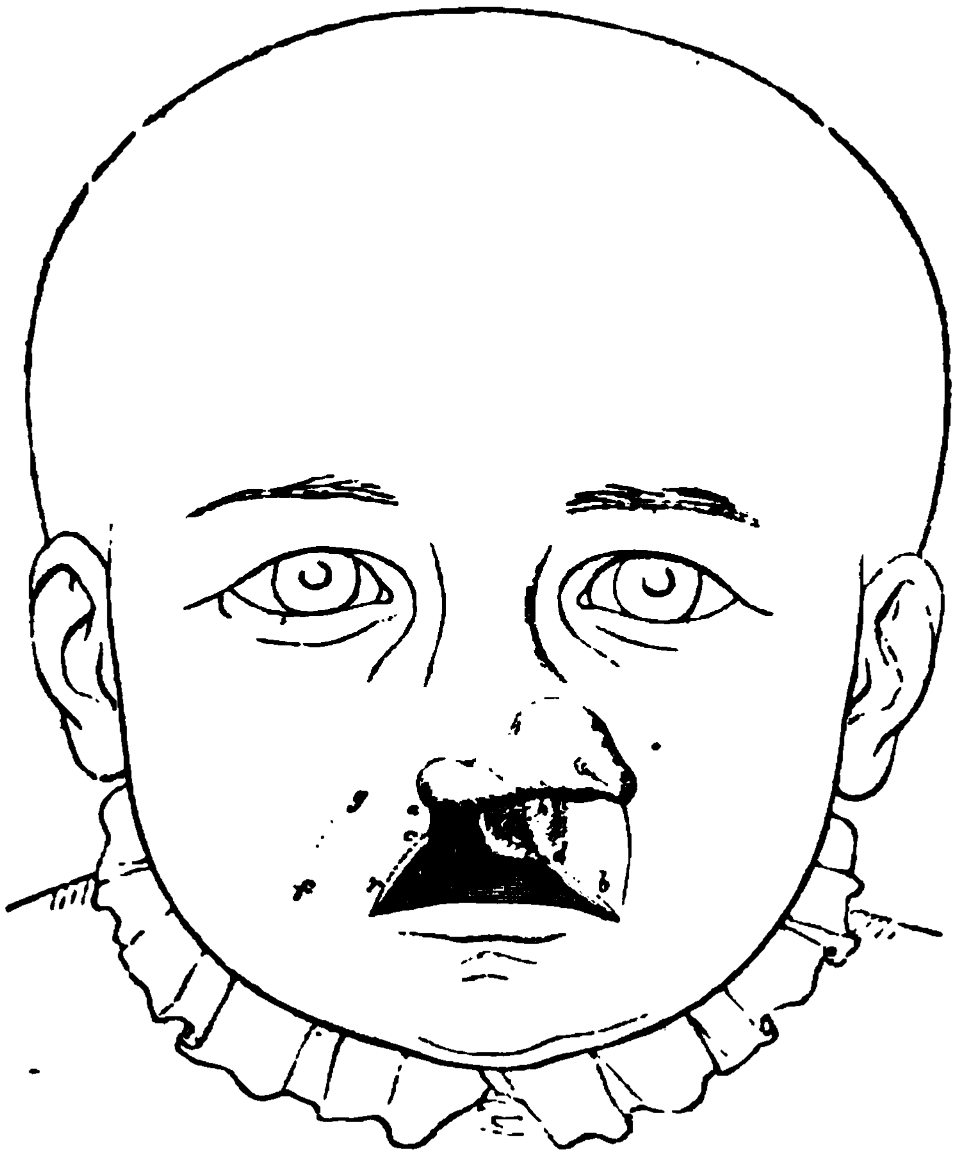
Heinrich W——, the third child of healthy parents, was born with a hare-lip. His mother had enjoyed perfect health during her pregnancy; the two older children were born without malformation. Twelve hours after birth, on the 7th March, 1853, the child was brought to my clinique, and presented the appearance represented in fig. A.* (p. 509.)

The boy was plump, and well-formed, with the exception of the anomaly to be shortly mentioned. The upper part of the nose was

* The original drawings of the woodcuts have been executed by the well-known artist, Mr. Andorff, of Berlin. The dotted lines in fig. A indicate the incisions to be made for the operation of hare-lip.

rather narrow; the inferior portion was remarkably broad, and malformed; the tip of the nose was turned upwards, and pushed over to the

Fig. A.



left side; the left nostril appeared contracted, the left ala pushed upwards. The septum nasi rested in front upon the inter-maxillary bone, and posteriorly on the left margin of the palatal fissure shortly to be mentioned. The right ala nasi was remarkably drawn over to the right; its point of insertion passed imperceptibly into the right cheek, and lay close to the upper end of the right edge of the labial fissure. The right supra-maxillary portion of the lip was entirely deficient, so that no trace of the sulcus naso-labialis was perceptible on this side, whilst it was distinctly visible on the left side. The only indication of the upper lip on the right side consisted in a narrow red labial streak, which extended from the angle of the mouth towards the external termination of the right ala nasi, was lost about three lines below it, and formed a border to the edge of the right cheek, which was turned towards the labial fissure. The labial fissure was very broad, and in the right nostril led into a fissure of the right side of the palate between the inter-maxillary bone and the right superior maxilla. This fissure extended backwards between the maxillary processes of the superior maxillæ. The posterior portion of the palatal fissure lay more to the right than in the middle of the oral vault, and terminated at the horizontal processes of the palatal bone, at a short distance anterior to the velum, which was perfectly normal.

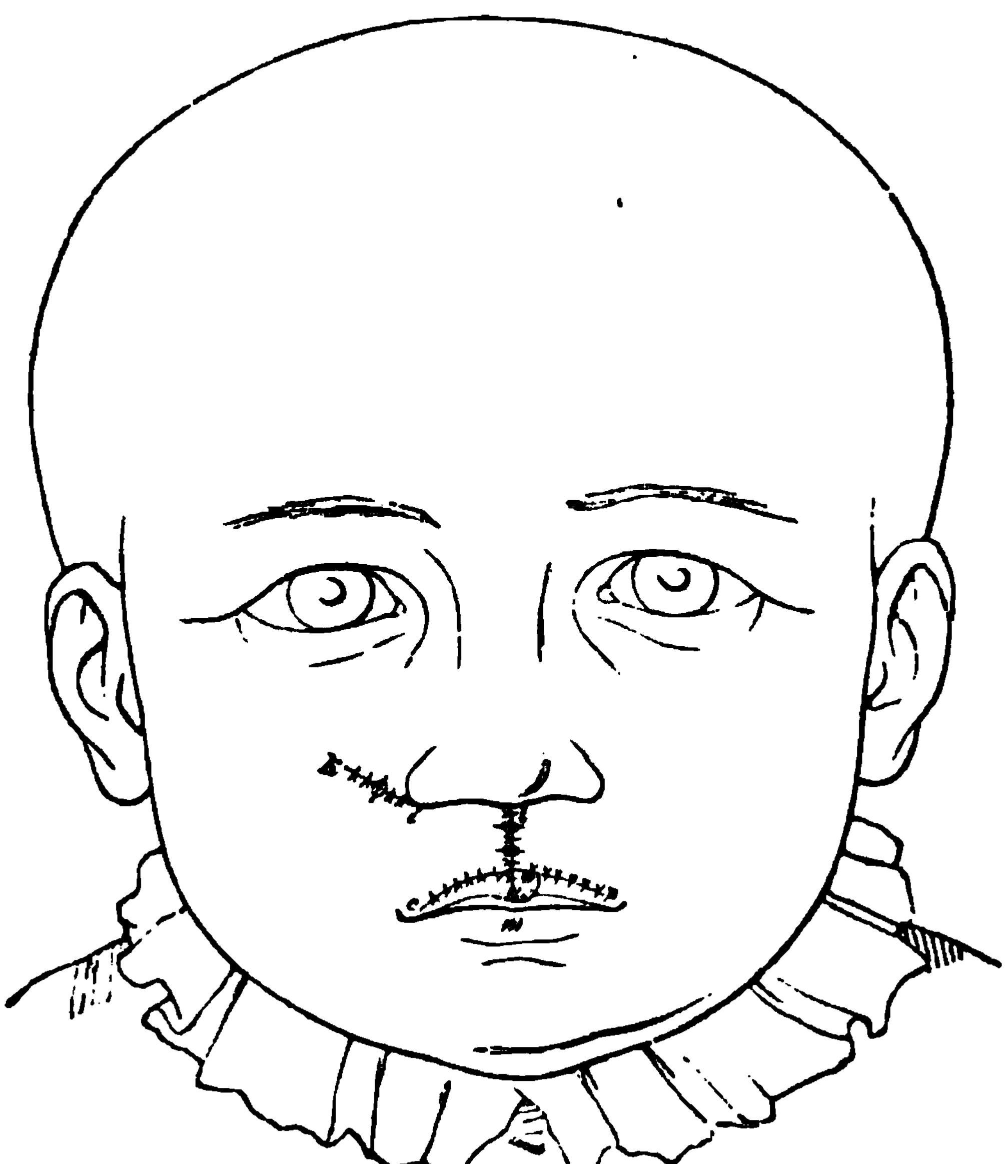
Accordingly, the fissure appeared to depend mainly upon an imperfect development of the right superior maxilla. The left termination of the

latter, only covered with mucous membrane, lay fairly exposed, as the right half of the upper lip was undeveloped. The inter-maxillary bone lay under the short septum narium, and with it was pushed over to the left. At the same time, this bone was turned on its axis to the left side, and projected with its dental margin, so that the latter advanced from the dental margin of the left superior maxilla, which completely adjoined it, to the right, and projected with its right (free) side beyond the surface of the left half of the lip, by about two lines. The mucous investment of the right half of the inter-maxillary bone on the left side passed over into the fissure of the left side of the lip, which consisted of the middle portion and the left lateral half of the upper lip. The middle portion of the lip was very short, completely adherent to the inter-maxillary bone by its posterior surface, and, in accordance with the axial rotations of this bone, directed to the left with the philtrum. In consequence of the union of the inter-maxillary bone with the left superior maxilla, the middle of the upper lip was completely united to the left lateral portion. This entire portion of the lip appeared arrested in its development, and left a large portion exposed which it ought to have covered. Especially below, over the mouth, this portion of the lip was very narrow, because the red margin in the vicinity of the left angle of the mouth diverged from the horizontal position upwards.

I was glad to undertake the treatment so early as twelve hours after birth, as the exposure of the oral mucous membrane to the atmosphere had as yet given rise to no injurious influences, especially inflammation of the mucous membrane of the mouth and respiratory tract. For the same reason I deemed it advisable to hasten the operation, in order to obviate such consequences. Fourteen hours after birth, the infant was operated upon under the influence of chloroform. I was unable in the present instance to apply any of the ordinary operations for hare-lip. Even had I adopted the expedient employed in Case 2, for the purpose of extending the lip, it was impossible to cover the defect by the left portion of the upper lip, as it could not have been extended sufficiently to approximate it to the right edge of the fissure; nor could the latter be approximated, because it was stretched between the angle of the mouth and the external edge of the nostril, and the right lateral portion of the upper lip was absent. But had I been able to draw the left part of the lip as far as the left edge of the fissure, and to unite them, the naso-labial groove would have been pushed over too much to the right, so as to disfigure the child. Owing to the great shortness of the middle portion of the lip, and the limited extent of the entire left portion of the lip, even by extensive detachment I could only hope to approximate it so far as to cover the left half of the region of the upper lip; the right half of the upper lip at all events required to be newly formed. The red labial margin on the right edge of the fissure appeared to me to indicate the method which I ought to pursue. As it appertained to the cheek, I proposed transplanting a flap from there of the size of half an upper lip, which was to be brought over to the left, and its red labial edge directed downwards, so as to represent the normal lip. The transplanted flap was to be united with the left portion of the lip, the red margin of which would serve to provide the left half of the labial edge.

I adhered to this plan in the operation, which was executed as follows:—I made a straight incision (fig. A, *a*, *g*), from the point of insertion of the right ala nasi outwards, through the entire thickness of the cheek. The length of this incision equalled the height of the upper lip that was to be formed. A second straight incision (fig. A, *f*, *g*) was made from the external termination of the former, involving the entire thickness of the cheek. The second incision formed almost a right angle with the former (at *g*), and ran so as to separate the edge of the cicatrix and the red substance of the lip from the right margin of the fissure (*a*, *c*, *r*). This incision equalled half the breadth of the space belonging to the upper lip. These two incisions separated the flap (*f*, *g*, *a*, *r*), which hung from the cheek by its base (*f* and *r*). I now detached with the scalpel the middle portion of the upper lip from the inter-maxillary bone, as also the left half of the lip, with the left nostril and adjoining portion of the cheek, and severed the cartilaginous septum from the floor of the nares. By this means I was enabled to turn the inferior portion of the nose, which was twisted to the left, to the right side, and give it its proper position; on the other hand, the entire left portion of the lip became so moveable as to allow of an easy adjustment to the right half of the lip. The inter-maxillary bone could now be pushed back towards the palatal fissure, though not sufficiently to entirely obviate the projection of its right side,

Fig. B.



I now with the scalpel detached the right cheek at the external and superior side of its upper incision (fig. A, beyond *g*), as well as the integuments of the right half of the nose, to such an extent as to be enabled

to approximate to one another the two edges of the incision into the cheek (fig. A, *f*, *g* and *a*, *g*); they were at once attached to one another by stitches and one figure-of-8 suture (fig. B, *i*, *k*). The red margin of the left portion of the lip (fig. A, *e*, *b*) was next separated from above downwards to the vicinity of the left angle of the mouth, and turned down; the upper part of the left margin of the fissure was merely scarified. The flap from the cheek was now laid into the right half of the space belonging to the upper lip, so that the red margin faced downwards (fig. B, *o*, *m*, *n*); the latter was detached as far as the vicinity of the right angle of the mouth (fig. A, *c*, *r*), and turned down. The superjacent edge of the cicatrix of the right margin of the fissure was removed by an incision (fig. A, *c*, *g*), which also detached the upper angle of the red margin (fig. A, *c*, *g*). The transplantation of the flap from the cheek converted what had been its external edge (fig. A, *f*, *g*) into the upper margin of the right half of the lip and the floor of the right nostril (fig. B, *i*, *l*); what had been the upper edge (fig. A, *a*, *g*), now was directed from the septum narium towards the lower edge of the upper lip, and was united to the right margin of the left portion of the lip (fig. B, *l*, *n*).

By the incisions *a*, *g* and *c*, *d* (fig. A), the two free ends of the dependent red edge of the lips received the following conformation: the upper angle of the left red margin (fig. A, *d*, *e*) projected, while the upper angle of the right red margin (fig. A, *c*, *g*) was slanted off. For this reason the two angles, when their cut surfaces were placed in apposition, fitted well, and could be united by fine stitches. The red margin now represented a continuous arch, with a short fold projecting downwards. This fold I laid under the point of union of the two halves of the lips (fig. B, *n*), then pushed the right and left edges of the red margin of the lip together, so as to form an arched prominence, which was at once attached to the lip. I distributed the remainder of the red margin, and fastened it to the right and left under the lip (fig. B, *o*, *n* and *n*, *p*) in such wise that the arched prominence was less marked (fig. B, *n*, *m*). In order still further to secure the labial halves in apposition, I passed a few narrow strips of court-plaster from one cheek to the other, and covered them with collodion.

The child bore this severe operation well. There was scarcely any hæmorrhage, owing to the external maxillary artery having been compressed during the whole operation by Dr. Wilhelm, who kindly assisted me. The position and form of the nose appeared much improved (fig. B). I proposed to rectify the great width of the right ala nasi subsequently, if it should be necessary, by excising a triangular wedge, of which the base would be turned to the free margin. Soon after the operation, and three times within the next twenty-four hours, the child had attacks of dyspnœa,* which were rapidly removed by warm baths and friction of the thorax with the flat hand. Internally, half a teaspoonful of syrupus croci and syrupus diacodion was administered every half hour, which induced sleep of several hours, during which the infant breathed calmly with an open mouth. On the second day after the operation two sutures were removed, on account of the tension induced; throughout, union took place by first intention, excepting at the point of the right cheek from

* See Chirurgische Klinik, Beobachtungen und Erläuterungen im Gebiete der Chirurgie. Von Dr. Friedberg. Band i. p. 200. 1855.—(On the Nature of the Dyspnœa following the operation for Hare-lip.)

which the right half of the lip was taken; here the edges of the wound (fig. B, *i*, *k*) partly separated, and subsequently united by granulations. But very little tumefaction ensued in the parts, and no peculiar features were presented. From the fourth day after the operation the child took the breast. On the subsequent day I was enabled to remove all the sutures, and replace them by strips of plaster. The floor of the right nostril (fig. B, *i*, *l*) rapidly cicatrised, and the operation could now be termed completely successful.

Two months later I saw the child again, and found it in good condition. The fissure of the hard palate was diminished by one half, the inter-maxillary bone no longer projected, and had united with the alveolar process of the right superior maxilla, with the exception of a minute indentation. In the tenth month after the operation, after the middle lower incisors had made their appearance, the palatal fissure had completely closed. The red margin of the lip exhibited a symmetrical and wavy line on both sides of the arched prominence which occupied the middle; the lip presented an outline closely resembling that of the normal condition.

I avoided the extirpation of the inter-maxillary bone in the case just detailed, although it had an abnormal direction, and projected beyond the level of the labial fissure. I diminished the prominence of the bone by bending it back towards the palatal fissure, so far as its union with the left upper maxilla permitted. In case of this proceeding failing, it was to be feared that the labial union which lay before the projecting part of the inter-maxillary bone might advance a little; but I might equally expect that the pressure exercised by the lip upon the bone would gradually push it back into its proper place. The result has shown that the latter hope was justified. In all the cases of hare-lip which I have hitherto observed, it was possible to push back the projecting inter-maxillary bone by the pressure exerted upon it, with the fingers or by means of a forceps, sufficiently to effect the union of the lips. The branches of the forceps with which the inter-maxillary bone is seized should be covered with leather or linen, so that the mucous membrane may be protected as much as possible.

This proceeding necessarily induces a fracture of the nasal septum. If the inter-maxillary bone is so far detached as to fall down, Mr. Butcher* recommends that it be fastened with wires to the upper maxilla; this procedure could necessarily only succeed if, in detaching the inter-maxillary bone, the vascular connexion remains sufficiently intact to secure its due nutrition.

In the boy whose case I have just detailed, the pressure of the fingers sufficed the more readily to push back the inter-maxillary bone, as the septum narium had previously been divided along the floor of the nares. I have not yet had occasion to adopt the proceeding of M. Blandin, which consists in excising a cuneiform portion of the septum. In no case do I consider extirpation of the inter-maxillary bone to be necessary, solely on account of its too great prominence. If this bone is removed, the alveolar arch of the upper maxilla will eventually prove too small for the lower

* The Dublin Quarterly Journal of Medical Science, p. 43. Feb., 1856.

jaw by four incisors; from the two jaws not fitting, the chin will project much, and mastication will be rendered difficult. If the inter-maxillary bone is distorted, or much retarded in its development, it is better to remove, as was done in Case 2.

Numerous cases have been published in which, after the hare-lip had been operated upon, a spontaneous closure of the fissure in the palate ensued. It appears that the fissure in all these cases only affected the hard palate. In addition to the case above given, I am able to quote another, a girl, who was operated upon in my clinical wards ten hours after birth, in whom the palatal fissure healed spontaneously. In both cases the fissures ceased anteriorly to the ossa palati. The girl just mentioned had a double hare-lip, involving the palate. The fissure on the right side of the inter-maxillary bone was two lines broad, that on the left three lines broad; between the palatal processes of the superior maxilla it was about two lines, further back about three lines in breadth. At the age of thirteen months, the palatal fissure was closed, with the exception of a portion, one line broad, posteriorly. Six months later, the child was re-admitted, on account of some other complaint, and no trace of the opening remained.

The spontaneous cure of fissured palate is partly the result of the progressive growth of the inter- and supra-maxillary bones; it is partly brought about by the pressure exercised upon the bones, when the operation of hare-lip has been successful, by the upper lip and the cheeks. As this influence is more powerful in front than behind, the closure of the fissure takes place earlier anteriorly than posteriorly. Where this influence is not brought to bear, in consequence of the labial junction not having been effected, the fissure of the palate does not close spontaneously. In two cases of this kind I was able to determine that the fissure had increased in width after birth; in these cases the bones in relation with the fissure must have separated still further, owing to the absence of any retentive medium. The circumstance, that we meet with infants who have a fissured palate, with an entire upper lip, indicates a very feeble development of the bones in relation with the fissure, and does not invalidate the view just alluded to. As further evidence of the pressure exerted upon the subjacent bones by the upper lip, I may advert to the fact that if, during the existence of hare-lip, a tooth breaks through in its vicinity, the latter, instead of descending, advances, and pushes before it the edges of the fissure. Hyrtl* remarked, justly, that the pressure exercised upon the alveolar process of the upper jaw by the upper lip, maintains the teeth as they appear, in due order.†

Another main reason why the palatal fissure earlier closes in front than behind, is to be found in the fact that the inter-maxillary bone and the alveolar process of the superior maxilla are developed more rapidly during the first years of life, than the other bones connected with the palatal fissure.

It has been asserted that the union of the lips in labio-palatal hare-lip

* See his Handbuch der topographischen Anatomie und ihrer praktisch medicinisch chirurgischen Anwendungen. Band i. p. 224.

† He alludes to the fact that when a tooth pierces the anterior wall of the alveolar process, the lip may be perforated, so that the tooth appears on the face.

is of no moment with regard to the nutrition of the child, because the obstacle to suction lay in the palatal fissure. I decidedly demur to this view. Children affected with labio-palatal hare-lip are only then unable to suck when there is a palatal fissure on each side of the inter-maxillary bone (*palatum fissum duplex*), when the fissure is broader than the nipple, or when it is combined with fissured lip. If the lip is closed, and the palatal fissure only narrow and confined to one side, the children are able (like Heinrich W.) to take the breast. They seize the nipple as low as possible, press it against the edges of the palatal fissure, and thus force out the milk.

Considering the injurious influence exercised by the fissure of the palate upon nutrition, and at a later period upon articulation, and considering the influence exerted by the union of the lips upon the contraction of the fissure in the bone, it is the more necessary to form a high estimate of the early operation for hare-lip. I cannot agree with Hyrtl* and Dupuytren,† who consider it impolitic to operate soon after birth, as the great mortality among new-born infants might cause the operation to be regarded as the cause of a fatal issue. I have on another occasion combated the fear of operations upon new-born infants.‡

Mr. Butcher§ remarks that the fissure of the palate often closes spontaneously, if the hare-lip is operated upon in infancy. But it does not appear from his treatise, which is so rich in practical suggestions and scientific data, that he has himself observed this circumstance; it is not definitely stated in the eight cases which he has published, that he observed this result. The earliest time at which he operates is the twelfth day after birth; and he is of opinion that the operation ought not to be performed before the end of the first week, in order, as it were, to allow the "functions of the body to be healthily in action." The conclusive grounds upon which he opposes the postponement of the operation, induces us to hope that he may think proper to operate still earlier, since there seems no reason why the functions of the body should be in particular order at the end of the first week.

The question arises as to which functions are involved in the treatment of hare-lip. As soon as the tissues of one cut surface can grow into those of the opposite side, the condition for the union of the cut surfaces is fulfilled. But this condition is realized during fœtal life, as is shown by the cicatrisation of the stump after spontaneous amputation before birth. Dr. West|| has published a case of this kind, in which the leg of a new-born child had been removed an inch and a half below the knee, and the cicatrising process of the stump was far advanced. Mr. Butcher¶ quotes a case in which the stumps of the toes of a new-born infant showed a recent cicatrix; in another case, the stump of a finger exhibited a perfect cicatrix. The same was observed in the case of an infant, by Chaussier,** where the amputated fore-arm lay by the side of the child. A series of

* Loc. cit. p. 226. † Chirurgie Clinique, tom. iv. p. 90.

‡ Chirurgische Klinik, Band i. p. 199.

§ On the Operative Measures necessary in the Treatment of Hare-lip: Dublin Quarterly Journal of Medical Science, No. xli. p. 30.

|| London Medical and Surgical Journal, vol. i. p. 741. 1832. ¶ Loc. cit. p. 27.

** Discours prononcé à l'Hospice de la Maternité. 1812.

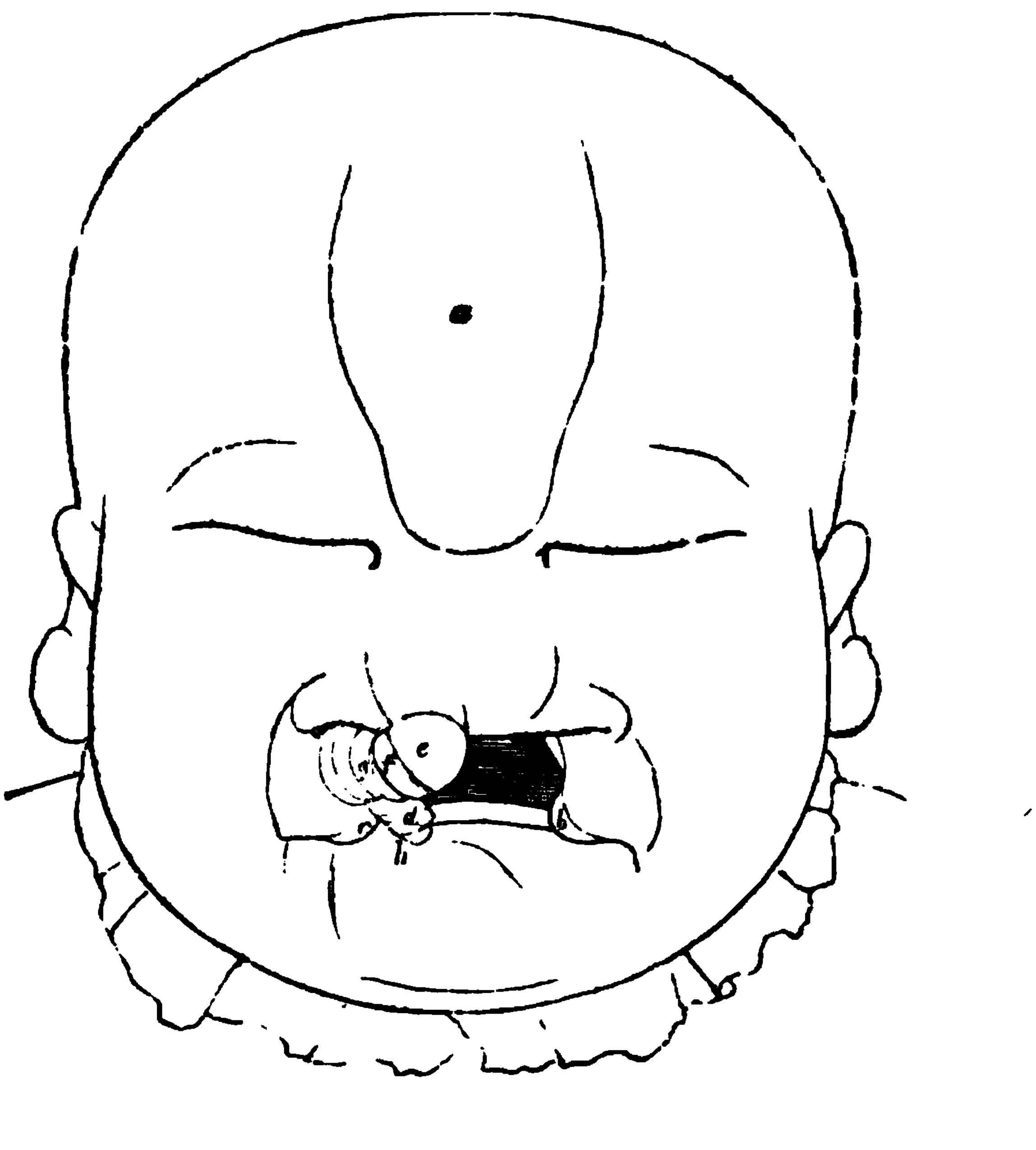
analogous instances has been recorded in Germany. The warmth of the uterine cavity necessarily exerts an influence, but the powers of reproduction are sufficient, even after birth, to secure the healing of wounds, as is shown by the perfect result obtained by operating for hare-lip within a few hours of birth. I have shown, in my 'Clinische Beobachtungen,' that the reaction that follows an injury is the less the sooner the operation is performed after birth.

CASE II.—Double labio-palatal fissure, with microphthalmia, resulting from malformation of the brain. Proposed method of operating for hare-lip. Death. Necropsy.

Mrs. L., whose previous children were well formed and healthy, had a fifth child, a boy, who was born with a hare-lip, and presented several deviations from the normal state. It was brought to my clinique when twenty days old. The child had not at that time opened its eyes, had slept much, and had shown a good appetite. It could not suck, but took the milk that was given it with avidity, though a large portion was rejected by the nostrils, with frequent symptoms of urgent dyspnœa. During these attacks the child was raised; it coughed, and soon was pacified. The amount of nourishment thus taken was evidently insufficient, because the infant became more voracious, and the dyspnœa at the same time increased when it took food. A fortnight after birth it had diarrhœa. Two days later, the child was found to have aphthæ, to become restless and fretful. Vomiting supervened, and the milk that was rejected was not at all, or but slightly, coagulated.

On admission, on the 1st July, 1853, the child presented the following appearance:—

Fig. C.



The child was of normal size and well formed, with exception of the head, which was decidedly too small in proportion to the size of the child, especially the cranium. The circumference of the cranium was 11$\frac{7}{8}$ inches, the antero-posterior diameter was 4, and the transverse diameter 3 inches. The forehead presented a peculiar malformation, as both ossa frontis formed a pointed projection resembling a cockscomb (fig. C, *a*), the anterior edge of which felt like a sharp ridge of bone. The anterior fontanelle did not present any peculiar extent, but was drawn out towards the frontal suture, to a long point. The occiput was normal in shape.

The eyes were remarkably close to one another; the lids showed no trace of convexity, they were so firmly closed that I could only separate them by means of a speculum; considerable resistance was offered to this instrument by the constrictor palpebrarum. The orbits were much too small; the right eyeball very small, and the left one almost imperceptible.

The nose was of normal length, very broad, and prominent below; the bridge of the nose was very narrow and shallow, and visibly depressed under the crest of the forehead. The left ala nasi was broader than the one on the right. A flap of skin, four to five lines long, and two lines broad (fig. C, *e*), hung down from the tip of the nose, and showed a piece of red labial tissue at its inferior extremity; it corresponded to the cutaneous covering of the septum, the philtrum, and mesial portion of the upper lip. Besides these defects, there was a double labio-palatal hare-lip. To the right of the cutaneous fold just spoken of, the remarkably small and narrow inter-maxillary bone projected so as to advance three lines beyond the level of the two lateral portions of the upper lip; it was turned to the right, and approached close to the tip of the nose. The left external incisor (*h*) had already made its appearance at the anterior much-distorted end of this bone, but was badly developed. At both sides of the inter-maxillary bone the palatal fissure was visible, and posteriorly it formed a broad slit in the hard and soft palate, and divided the uvula into two equal parts. The lateral portions of the upper lip were pretty symmetrically formed, and showed a well-developed red labial margin, which to a small extent inverted the edges of the fissure (fig. C, *b* and *c*). The oral mucous membrane presented numerous catarrhal ulcers, and was of a vivid red. The navel was perfectly healed, and the scrotum contained both testicles.

The crest on the os frontis indicated a fusion of the two halves of the bone, and this, taken with the irregularity in the features, induced me to assume a fusion of the two cerebral hemispheres, closely approaching to cyclopia. The smallness of the skull, and the close approximation of the eyes, also suggested a scanty development of the brain.

The inter-maxillary bone, though atrophic, was present; there were also two orbits, though below the normal size; lastly, the child had a nose of sufficient length, with two nares; consequently, we had not to do with a fusion of the two halves of the face and the mesial portion of the face. The nose and the inter-maxillary bones were present. But when complete fusion of the two halves of the cerebrum is associated with microcephalic malformation, we generally find the two halves of the face also

fused together, and the mesial portion defective. On this ground, I concluded that the assumed fusion of the cerebral hemispheres was incomplete.

Long life was scarcely to be expected, nor desired, for a child presenting such malformations as those adverted to. Still, as such a child has an undoubted right to a prolongation of life, and this was much endangered by the labio-palatal fissure, I considered it to be my duty to perform the operation for hare-lip. As the respiratory orifice was so disproportionately large, and the mucous membrane of the mouth so extensively exposed to the atmosphere, I considered the most urgent indication to be the limitation of the respiratory orifice, and the protection of the oral mucous membrane. The inflammation and ulceration of the latter, however, induced me to postpone the operation, as I could not assume that during its continuance the operation would be successful. I ordered the internal administration of chlorate of potassa, the mouth to be brushed carefully with an infusion of sage and mel rosæ, with warm baths of chamomile, and broth.

The following is the plan I proposed to follow in operating:—I intended forming the cutaneous septum narium with the flap of skin dependent from the tip of the nose. After its removal from the intermaxillary bone, the latter was to be extirpated, so as to convert the double, into a single, fissure. I considered the removal of the bone indicated because it was distorted and atrophic; otherwise I prefer retaining it, on account of its bearing incisor teeth. The two lateral parts of the upper lip, with the adjoining portions of the cheeks, were to be so far detached from their base, as to permit the edges of the former to be conveniently approximated to one another. In order to cause the cheeks to follow the more readily, and bring the labial halves together without unnecessary stretching, I proposed separating the cheeks and lips from the nose by an incision carried carefully round the lower boundary of the nose, through the soft parts. If after this separation there had still been much tension on attempting to bring the lids together, I should, as I have often done before, have made a vertical incision in the cheek, of sufficient length and depth, at some distance from the angle of the mouth, in order to secure greater gaping of the wound and yielding of the lip.

The red margin of the fissure was to be detached as far as the angle of the mouth, and to be turned down; the remaining part of the margin I intended simply to remove. Otherwise, the operation was to be completed as in the first case.

The operation was not, however, performed. The boy appeared much debilitated on the following day, breathed feebly, and expired in the course of the forenoon, without evincing any pain or suffering.

The following are the results of the post-mortem, which was performed eighteen hours after death:

The corpse weighed five and a half pounds, and was nineteen inches in length. There was no rigor mortis. After the scalp was removed, the coronal, sagittal, and lambdoidal sutures, as well as the parietal and occipital bones, were found to be normal. The pointed crest on the

frontal bone was caused by a complete fusion of the two halves of this bone, so as to form a projecting ridge. At this point, the frontal bone was found, on being sawn through, to be one and a half line thick. The falx was remarkably narrow, especially in front, near the crista galli; still, it penetrated to a considerable depth, so as to secure a division of the cerebrum into two lobes. The entire brain weighed eleven ounces. At the base of the brain, the course of the arteries was so far abnormal that the basilar artery gave off a communicating branch to the carotid on the right and not on the left side, and that therefore the circle of Willis was imperfect. Anteriorly, the anterior branches of the carotid ánastomosed; but from the union, only one artery was given off on the left side, which did duty as the arteria corporis callosi. The posterior portions of the brain, and the origins of the nerves, were normal. The fovea Tarini was very deep, and was bounded anteriorly by two oval, shallow, small, and ill-defined mammillary bodies. Anteriorly to these lay the small shallow tuber cinereum, with a short infundibulum, and normal pituitary body.

The origins of the optic nerves, in their passage from the hemispheres over the crura cerebri, were very slightly attached. They met before the tuber cinereum in a semicircular arch, and not, as in the normal state, in an acute angle. The chiasma did not project from the cerebral tissue, but was attached to it, and did not present the crucial form. The nerves themselves, which passed off at right angles from the optic arch, were detached. They were very small, especially the one on the left. Before the chiasma, the anterior wall of the third ventricle was not formed, as usually, by the thin lamina of the corpus callosum, which passes up from the chiasma, and which above merges in the anterior portion of the corpus callosum. Instead of the normal formation, there was a commissure of grey and white cerebral substance, of one and a half line in thickness, which passed from one hemisphere to the other. Anterior to these, to the extent of about half ân inch, the gyri of the anterior hemisphere, especially those corresponding to the origin of the olfactory nerves, entirely coalesced, and the olfactory nerves were deficient. Although further on the cerebrum presented a fissure of at least an inch in depth, this coalescence of gyri extended upwards, so that the anterior portion of the corpus callosum was replaced by genuine cerebral gyri, like the vermiform process of the cerebellum. Finally, these gyri posteriorly passed into a genuine corpus callosum, of one and a half inch in length, and of proper width. The posterior end was remarkable from projecting two lines above, and covering the pineal gland. The cerebellum was well developed. No abnormity was discovered at the base of the cranium, except that the alæ minores of the sphenoid bone were too short and obtuse. The orbital portions of the frontal bones were so closely pushed together, that a space of not more than two lines remained for the ethmoid bone, which was chiefly filled up with fibroid tissue. The osseous basis for the above-mentioned cutaneous flap attached to the tip of the nose, which represented the membranous septum of the nose and mesial portion of the upper lip, consisted of the vomer, with an inter-maxillary bone. The latter was only four lines thick, and therefore entirely arrested in its development. In addition to the atrophied external incisor, which had already made its appearance, the

inter-maxillary bone contained two middle incisors, of almost normal size, concealed in their alveoli; there was no trace of the right external incisor. After detaching the integuments of the face, the middle suture of the ossa frontis was found firmly united; the nasal bones, the upper maxilla, and the nasal cartilages, were well formed.

The orbits were remarkably small, especially the left one. The right eyeball was normal externally, but only five lines thick; the left only three lines in diameter, and containing a cataract. Both eyes, when examined internally, presented a peculiar malformation connected with the original development. From the internal inferior margin of the iris (the ordinary seat of coloboma iridis), some dense vascular cellular tissue, which was firmly attached to the sclerotic, passed behind the iris to the middle of the bulb, where it formed a sacculated dilatation, which enclosed the lens as a capsulo-pupillary membrane. The right lens was normal; the left one very small, and cretified. The inner surface of the sclerotic exhibited, from the insertions of the lenticular pedicle to the foramen opticum, a thickened ridge.

No marked alterations were perceived in the remainder of the body; the trachea was well formed, the rings open, the heart sound, the thymus large; the cæcum had not descended sufficiently low, the prolongation of the peritoneum for the right testis was still open, the entire spinal cord down to the cauda equina was normal.

A review of the various anomalies of the formation of the head in the above case, shows the essential condition of the anomalies to have resided in the faulty condition of the cerebrum. This was characterized by the incomplete fusion of the anterior lobes. The imperfect separation of the cerebrum into two hemispheres, may be fairly attributed to imperfect development of the anterior termination of the central nervous system, or of the anterior cerebral lobes. The arrest of development of this lobe, from which, according to Bischoff and Von Baer, the eyes are separately produced, was also the cause of the little eyes being so closely pushed together. In the same way, the arrest of the development of the anterior cerebral lobe would entail a faulty development of the anterior portion of the chorda dorsalis. It is from this division that those portions of the nose and face are developed which, according to Huschke, advance to separate the eyes, which originally are formed from a single rudiment. We may therefore assume that the imperfect formation of the mesial portion of the face, and especially of the inter-maxillary bone, depended upon the imperfect disposition and development of the anterior end of the vertebral canal. Owing to the low degree of the arrest in the anterior cerebral lobe, the two primitive olfactory lobes and the optic lobes might in the first instance be separately developed, but would afterwards be retarded, and appear small and atrophic.

In the above case the immediate cause of the labio-palatal hare-lip may be sought in the defective development of the inter-maxillary bone. As this bone was not developed *pari passu* with the two upper jaws, its junction with them, and the union of the three portions of the upper lip, was not effected. The consequence of this was necessarily a double labio-palatal fissure.

As regards the malformation of the eyes, a peculiarity was observed which appears to offer the very opposite to what is seen in coloboma. The portion of the cutis which normally serves to form the lenticular capsule and the pupillary membrane, is folded into the orbit at its inferior and inner side, where it is surrounded by the cup-shaped cerebral portion of the bulb, so as to complete the globular form. At the point where this junction is effected we see in coloboma a thinning, whilst in the above case this point exhibited a firm vascular cellular ridge. This ridge manifestly originated in the fold of the cutis just spoken of, and, owing to morbid conditions, had persisted as a cordlike union between the lenticular capsule and its original point of entrance from the cutis.

The treatment of the red labial margin in the operation for hare-lip should seek to prevent the indentation which so frequently follows this operation, and proves a disfigurement as well as an impediment to articulation. The usual method of simply scarifying the edges of a hare-lip before uniting them, is likely to favour the formation of the indentation. Dieffenbach, who followed this method in above a thousand cases, met with the indentation at the point of union of the edges so constantly, that he remarks: "The lips are generally drawn up at this point, so that it becomes necessary to repeat the operation after some years."*

I do not venture to determine whether the indentation of the lip is due to the contraction of the cicatricial tissue, or to the fact that the latter does not yield sufficiently so as to follow the growth of the lip; both circumstances may possibly exert an influence in the matter. This secondary deformity of the upper lip has induced several surgeons to depart from the usual procedure, and make an angular cicatrix (Mirault), so as to form a projection at the point of union of the red margin. This may be done by two proceedings; either we follow Pétrequin's method, and excise the edges of the fissure by an elliptical cut—in which case, when brought together, they become elongated, and project below—or else we adopt Malgaigne's plan, who detaches the red margin of the fissure, and after bringing their two cut surfaces together, unites them by a suture. In this case, a red button is formed at the point of union.

I have followed Pétrequin's method in only two cases, but in both an indentation resulted at the point of junction of the labial border, which peculiarly disfigured the projection. I have repeatedly adopted Malgaigne's proceeding, and have still more frequently seen it employed by Professor Langenbeck, when I was senior assistant-surgeon in the surgical clinique of the university. In most cases, the cicatrix which corresponded to the union of the two halves of the lip exercised some traction upon the two sides of the button-like projection, and caused a double retraction of the red margin; in a few cases, an indentation formed in the middle of the button.

E. Mirault, of Angers, sought to prevent the indentation of the upper lip by forming a cuneiform flap at the lower edge of one portion of the lip, which was accurately fitted to the bevelled-off edge of the lower margin of the other portion of the lip. As the edges of the incision have an angular disposition, and the cicatrix, which corresponds to the point of union of the lateral halves of the lip, runs across the base of the wedge,

* Operative Chirurgie, Band i. p. 400.

the indentation cannot prove so considerable as when the edges are simply scarified before being placed in apposition. Mirault's procedure may, therefore, in many cases, secure a satisfactory result, and prevent the indentation of the lips.

It has been asserted, that when an angle is formed by the union of the edges of the wound, an indentation only ensues if union does not take place by first intention, and the cuneiform flap is retracted before union is effected. My own experience leads me to oppose this assertion. Even if the wedge is fixed by union by first intention, not only may an indentation form on either side, but the flap itself may be drawn up in the middle. I have observed this result in several cases of hare-lip operated upon either by Professor Langenbeck or myself. In my 'Chirurgische Klinik,' Band i. p. 201, I have given the case of a child (a drawing of which was taken by M. Andorff), in which Professor Langenbeck performed the operation for simple hare-lip, with an angular disposition of the cut margin, and where, notwithstanding, two indentations remained in the upper lip. They carried the well-preserved edge with them, and terminated in the direction of the nostril in a band of cicatricial tissue. In this case, however, the wound had healed by first intention.

The method which I have adopted in Case I., and which consists in making a border to the lower margin of the united lip, and forming a broad arched projection of the red margin at the point of union of the two halves, affords a better configuration of the lip than is obtained by Malgaigne's proceeding; and it causes, at the inferior margin of the lip, two horizontal cicatrices, which serve to counterbalance the contraction resulting from the vertical cicatrix. The best issue is obtained by this proceeding, when the edges of the fissure can be brought together below, in the mesial line. The projection then occupies the middle of the lip, which normally forms an arch at this point. If the union is not in the mesial line, the arched projection has to be formed lower down; as the movement of the lips is restored, and they become developed, the button is removed, and the edge of the lip becomes perfectly straight. I have in three other cases obtained as perfectly satisfactory results as in the case of Heinrich W.

In another instance it became necessary to adopt a modification of this proceeding. The patient was a girl, with double labio-palatal fissure, in whom the red margin mounted at the edge of the left half of the lip towards the floor of the left nostril, while on the right side it was only about three lines in length. The child was put under the influence of chloroform ten hours after its birth, which had occurred at the full period. After uniting the three portions of lip in the form of a Y, I bound the lip with the red margin derived from the left half of the lip alone. I turned down this red margin, and folded it under the point of union of the lateral portions of the lips, so as to form a convexity downwards. I united these two halves of the fold, the cut surfaces of which covered each other, by fine stitches. I then fastened the fold, drawing up the red margin, to the point of union of the lateral portion of the lip. That portion of the red margin of the left side which now remained and projected to the right side, was carried under the right half of the lip; its free edges were bevelled off,

and laid under, and attached to, the free bevelled end of the red margin of the right side. The result of this operation was complete. I have already had occasion to advert to the spontaneous closure of the palatal fissure which took place in this case.

Accordingly, we may detach the red margin of the lip even beyond the angle of the mouth, and make use of it to bind any part in which it may be deficient. After it is detached, it is simply drawn out and transferred to the part to which it has to be applied; it is then attached in its entire course by small stitches, both anteriorly and on the side directed towards the alveolar process. This method of transplantation I have carried out with complete success in the following case, which offers other points of interest.

I operated upon a female infant under the influence of chloroform, three hours after birth, which was perfectly normal. The labial fissure extended to the left nostril, its margins presented a very short red border. The septum narium exhibited below an arched projection, over which the tip of the nose was a little pushed up to the right. The left nostril also made an arched projection with the middle portion of its free edge, was broader, and descended lower than the right one. The child was otherwise well-formed and healthy. On account of the shortness of the red border, I adopted Mirault's method. As the upper portion of the left side of the lip deviated to the left, with the ala nasi, I introduced at the point of insertion of the latter a lance-pointed straight needle, through the lower part of the nose, and surrounded it by a cotton thread, in the figure of 8 form, by which I might the more readily draw forward the left ala nasi and the adjoining half of the lip, when detached. The child breathed well after the operation, but became very restless during the following night. On the following morning the right eyelids were slightly tumefied, the conjunctiva was spotted red, and rather dry. As several cases of diphtheritic ophthalmia occurred about that time, I was afraid that we might have to deal with such an attack in this case; I therefore applied nitrate of silver, fused with two parts of nitrate of potash, neutralized with a solution of chloride of sodium, and having passed over the parts a camel's-hair brush, dipped in sweet oil, I dropped in one drop of a solution of atropine (one grain to a drachm of water); compresses cooled upon ice were then applied every five minutes. In spite of the treatment, blennorrhœa, with purulent infiltration of the anterior fibrous layers of the cornea, ensued; fortunately, we succeeded in arresting the process, and to protect the left eye by closing it with adhesive plaster. The result of the operation for hare-lip was entirely frustrated by the diphtheritic process, which, as usual, affected the whole system. On the second day after the operation, the punctures and a few points of the cut edges assumed a dirty red colour and pulpy appearance, and there was no trace of adhesion. I at once removed the needle and one suture, and on the following morning was obliged to remove all the sutures, for the edges of the wound exhibited a diphtheritic condition. By energetic application of the diluted caustic, cold applications, and careful ablutions, and the internal administration of calomel, I succeeded in arresting the destruction of the edges of the fissure, but not until a portion of the red border, which had not been involved in the operation, had been attacked. Six

weeks later the upper lip was completely cicatrized, and on each lateral portion of the lip a red border of only two to three lines in length was seen. The child, under suitable treatment, recovered so perfectly, that in three months it was subjected to a second operation.

After sufficiently detaching the portions of lip from the subjacent structures, and removing the cicatricial border from the edges, I detached the red border on the left side, but extended the detachment beyond the angle of the mouth to the left half of the lower lip, so that the border sufficed to cover the lower margin of the upper lip when united. The border was distributed here as in the last case detailed of double labio-palatal fissure. As soon as the border of the upper lip was completed, I fixed the red margin in the left angle of the mouth, and then fastened it by a few stitches to the free edge of the left half of the lower lip. Throughout, healing by first intention ensued, except at the uppermost portion under the left nostril, where granulation took place. Four weeks later I excised a wedge-shaped piece, the base of which was turned forwards from the arched projection of the nasal septum, and united the edges of the wound. This little operation secured a marked improvement in the nose.

Omitting other details, the consideration of which would carry us too far, I conclude, that in operating for hare-lip the choice of the procedure to be adopted for the formation of the free margin of the lip must depend upon the relation of the red border of the fissure, if we wish to avoid indentation. The following propositions may accordingly be maintained:

1. The usual procedure of simply uniting the scarified edges of the fissure ought not to be employed at all, as the method pursued by Pétrequin certainly yields more satisfactory results.

2. If the red border of the edges of the fissure is too short to form a sufficient button under the point of junction of the lateral parts of the lip, I consider the transplantation of the red labial border, as suggested by me, to be indicated.

3. Mirault and Malgaigne's methods are inferior to the process of transplantation.

4. If the red border of the edges of the fissure suffices for the formation of a broad, arched button, beneath the point of union of the lateral portions of the lip, the method which I have proposed, of making the border to the lip with the formation of a button-like projection, promises the most satisfactory results.

PART FOURTH.

Chronicle of Medical Science.

ANNALS OF PHYSIOLOGY.

By Hermann Weber, M.D.,
Licentiate of the Royal College of Physicians, Physician to the German Hospital.

I. Blood; Circulation; Respiration; Animal Heat.

1. Hirt: *On the Numeric Proportion of the Red to the White Blood-Cells.* (Müll. Arch., pp. 174 ss. 1856.)
2. Brunner: *On the Average Tension in the Vascular System.* (Zürich, 1854.)
3. Brunner: *On the Tension of the Blood in the State of Rest in the Living Animal.* (Henle und Pfeufer's Zeits., für rat. Med. 1855, and Schmidt's Jahrb., vol. lxxxvii. p. 287.)
4. Becker: *On the Tension of the Carbonic Acid in the Blood, as a Measure for the Metamorphosis of the Carbonaceous Constituents of the Body and Food.* (Henle und Pfeufer's Zeits. für rat. Med., vi. 3, 1855.)
5. Seux and Roger: *On the Pulse in New-born Children.* (L'Union Médicale, vol. ix. No. 130, 1855.)
6. Marié: *On the Relation between the Frequency of the Respiratory Movements and the Contractions of the Heart.* (Arch. Génér., Juill. 1855; and Schmidt's Jahrb., vol. lxxxviii. p. 166, 1855.)
7. Vierordt and G. Ludwig: *Contribution to the Knowledge of the Respiratory Movements.* (Vierordt's Arch. für Phys. Heilk., xiv. 2, 1855.)
8. Moleschott and Schelske: *Comparative Researches on the Quantity of Carbonic Acid excreted, and the Size of the Liver in nearly-allied Animals.* (Moleschott's Untersuch. z. Naturlehre d. Menschen u. d. Thierre, pp. 1 ss. 1856.)
9. Moleschott: *On the Influence of Light on the Excretion of Carbonic Acid by Animals.* (Wien. Med. Wochensch., No. 43, 1855.)
10. Valentin: *On the Interchange between Muscles and surrounding Atmosphere.* (Vierordt's Arch. für Phys. Heilk., xiv. 4, pp. 431 ss. 1855.)
11. Van der Becke Callenfels: *On the Influence of the Vaso-motor Nerves on the Circulation and Temperature.* (Henle und Pfeufer's Zeits. für Rat. Med, vii. pp. 157 ss. 1856.)
12. Kussmaul and Tenner: *On the Influence of the Circulation in the Large Vessels of the Neck on the Temperature of the Ear, and its Relation to the Alteration of Temperature by Paralysis and Irritation of the Sympathetic Nerve.* (Moleschott's Untersuch. z. Naturlehre d. Menschen u. d. Thiere, i. pp. 90 ss. 1856.)

Hirt, who made his observations principally on his own blood, found the number of the white globules always considerably increased from half an hour to one hour after each meal; the influence of the latter in general disappeared two hours later (i.e., two and a half to three hours after the end of the meal). Thus, at 8 A.M., when breakfast was taken, the proportion of the white to the red globules was = 1 : 1760; from half-past 8 to 9, = 1 : 700; between 9 and half-

past 10, it sank to 1 : 1510; at 1 P.M. (dinner) = 1 : 1510; from half-past 1 to 2 P.M. = 1 : 420; at half-past 3 P.M. =1 : 1480; at 8 P.M. (supper) =1 : 1480; at half-past 8 P.M. = 1 : 550; between half-past 10 and half-past 11 = 1 : 1230; from then to 6 A.M. it sank gradually to 1 : 1760. Hirt agrees with Funke and Vierordt, that the blood of the vena lienalis is much richer in white cells than that of the artery; but according to him the proportion is only = 1 : 60, while Funke asserts that the white cells sometimes form the fourth part of the whole number; and Vierordt described it in the vein of a decapitated criminal (one hour and a half after death) as large as 1 : 4,9. The author corroborates Lehmann's statement, that the blood of the hepatic vein contains a larger amount of colourless corpuscles (1 : 136) than that of the vena portæ (1 : 514). Some experiments with tonic remedies lead him to the inference that these possess the power of increasing the number of white globules in a very remarkable manner; the tincture of myrrh being in this respect superior to that of bark, and still more to that of the malate of iron.

Brunner measured the tension of the blood in the vessels by means of C. Ludwig's "Kymographion." He endeavoured to arrest the motion of the thorax, the limbs, and the heart, by placing the animals under the influence of opium or chloroform, and of electrifying at the same time the peripheric ends of the dissected vagi. We must refer for the details to the original communications; stating here only, that from the author's experiments it becomes evident, that the blood is under a considerable degree of pressure, also when in a state of rest. The cause of this is attributed to the circumstance that the capacity of the blood-vessels, when their walls are not stretched, is smaller than the bulk of the blood contained in them; that the vessels must therefore be distended in order that the blood may find room within them. The degree of tension varies much in the same animal: (*a*) it becomes increased by the quantity of the blood being increased, (*b*) diminished in the opposite case; thus, the tension in a small dog was found to be at first equal to 10·4 millimètres of mercury, it rose to 19·0 mm. after the injection of 280 grammes of blood, and fell later to 8·5 mm. when 356 grammes of blood had been detracted. As the quantity of the blood is frequently changing, according to the state of health, the meals, and other influences, it is without doubt that the degree of tension is not always the same. The quantity of blood remaining the same, its tension varies with the expansibility of the vessels, the amount of contraction or relaxation of the surrounding muscles, the position of the limbs, &c. In Brunner's experiments the tension varied during the continuance of the irritation of the vagus between 4 mm. and 29 mm.; but however long he applied the irritation, he never succeeded in reducing the state of tension to so low a grade as it assumed at the moment of death. The power of resistance in the walls of the vessels must therefore undergo a change while life ceases. The influence of the irritation of the vagus shows itself almost immediately in the arteries, but only after the lapse of some seconds in the veins.

Becker's method appears to be more simple than any one of those hitherto employed. The air is expired into a glass bell placed over mercury; the carbonic acid is determined by Bunsen's method (eudiometer and balls of potassium). The inspirations and expirations were of course made, in Becker's observations, according to a certain rule, sixty seconds being the time during which the breath was retained after a profound inspiration, whenever no intentional deviation is mentioned. The principal results are:—1. That the tension of the carbonic acid in the blood varies; that it increases and decreases with the quantity of carbonic acid contained in the blood, which is proportionate to the quantity of carbonic acid expired in a given space of time. 2. That the carbonic acid is expired in varying quantity, according to the length of time during which the breath is retained. We will quote the results of Vierordt's, as well as Becker's, experiments on this subject:—

			Becker.	Vierordt.
Time of retention	0 seconds	...	3·636 per cent. CO_2	... —— per cent. CO_2.
" "	20 "	...	5·552 "	... 4·80 "
" "	40 "	...	6·265 "	... 5·21 "
" "	60 "	...	7·176 "	... 6·06 "
" "	80 "	...	7·282 "	... 6·44 "
" "	90 "	...	—— "	... 6·50 "
" "	100 "	...	7·497 "	... 8 06 "

3. While the *temperature*, and the *frequency of pulse* and *respiration*, reach their maximum immediately, or at least within the first hour, after the principal meal, the tension of the *carbonic acid* becomes greatest after two or three hours, and *urea* is excreted in the largest proportion from the second to the fifth hour following the meal. 4. Concerning the *period of the day*, we find the tension considerable (6·904 p. c.) at 6 A.M.—i.e., immediately after waking; then, no food being taken, it decreases until 10 A.M. (6·287 p. c.); after this it rises, to reach its maximum at about 2 P.M. If a meal is taken at noon, the tension begins immediately afterwards to rise more considerably, reaching its highest degree within two hours and a half (7·593 p. c.) When no food was taken, Becker found this increase less marked (only to 6·89 p. c.); but, in opposition to Vierordt, he asserts that it is never altogether absent, and that it coincides with the increase of temperature and frequency of pulse, which are likewise observed at that time, independently of the ingestion of food. After about 3 P.M. the tension begins again to diminish. 5. Increased consumption of water has scarcely any influence on the tension of the carbonic acid, while the excretion of urea becomes remarkably augmented. Thus Becker shows that "as the elimination of carbonic acid is considerably influenced by the quantity of air inspired, that of urea is to some degree dependent on the ingestion of water."

Seux gives the results of his own and Dr. Magail's observations on the pulse of infants, from the period commencing directly after birth, and ending at the age of two months. These observations, which are made at the Charité Hospital at Marseilles, and for the greatest part corroborated by Dr. H. Roger, at the Hospital for Children in Paris, lead to the following inferences:—1. The pulse of infants, when in the state of health and quietude, may vary from 80 to 164. 2. In the greater number, it ranges between 120 and 140; then follow the cases between 140 and 160; afterwards, those from 100 to 120; later, those above 160; lastly, those below 100. 3. It is in general regular; sometimes, however, several pulsations follow each other more quickly, and are succeeded by others which are separated by a longer interval. These irregularities were found in cases that were below the average frequency. 4. The sex, constitution, salubrity of residence, or time of the year, appear to exercise no influence. 5. The pulse is more frequent during the first few hours after birth, but from the end of the first day to that of the second month, no difference is to be attributed to the age. 6. The periods of the day are without influence. 7. The act of sucking in general quickens the pulse, which influence remains perceptible during about half-an-hour. 8. Sleep and waking, quietude and agitation, have a marked influence. During sleep, the frequency is diminished, it rises when the child awakes but remains calm, and still more when it becomes agitated. Thus it rose in one instance at first from 104 to 120, and afterwards, when the child began to cry, to 134. A sudden effort or emotion may cause an increase of 20 or 25 beats in a minute.

It will be seen that these observations confirm those of Knox, Guy, Valleix, and others, in most points, but that they differ in their statement on the influence of the period of the day and of the sex.

Marié gives 20 as the average number of the *respiratory movements*, 71 as that for the *pulse;* the proportion of the former to the latter, 1 to 3·51. For women, he found the figures 77 and 23; for men, 69 and 19. The proportion between the two increases and decreases with the frequency of the pulse, in support of which

inference, the following table is compiled from the examination of patients of different nature:—

Average number of respirations.		Average frequency of pulse.		Proportion.		Number of cases.
16	...	43	...	1 : 2·69	...	12
19·73	...	53·6	...	1 : 2·71	...	15
24·7	...	70 E	...	1 : 2·83	...	93
24·7	...	82	...	1 : 3·32	...	39
35·	...	104	...	1 : 2·97	...	54
42·43	...	142·28	...	1 : 3·35	...	21
50	...	172	...	1 : 3·4	...	4

Hourmann and Déchambre arrived at very similar results in their observations on aged persons. Exceptions to this rule are in general found after strong mental emotion, when the respiration mostly remains calm, while the frequency of the pulse becomes considerably increased; further, after meals, which cause about eight pulsations more in the minute, although the number of respirations is rarely influenced. Several pathological conditions, and the effect of digitalis, may likewise be named among the exceptions.

Vierordt and G. Ludwig performed their experiments on five male subjects, aged 36, 20, 51, 34, and 7 years. Concerning their method, we must refer to the original essay; we only mention here that the respiratory movements were measured on a point of the linea alba, a little below the umbilicus, and represented by means of a lever-like instrument on the Kymographion drum.

The principal inferences are—1. The duration of the *single* respirations in the same experiment (three to six minutes) varies considerably; the average duration drawn from the lowest figures in the five individuals to that from the highest, bears the proportion of 100 to 209, although the experiments are made in the state of quietude. 2. Each respiratory movement is divided into four periods,—*a*, inspiratory period; *b*, inspiratory pause; *c*, expiratory period; *d*, expiratory pause. The duration of the inspiratory period varies in the average from 100 to 232, that of the expiratory period from 100 to 226. For the relation of these four periods among themselves, the authors use the term *celerity* of respiration; *quick* they call a respiration with a short inspiratory period; *slow*, the opposite. The inspiratory period signified by 10; the expiratory period occupied in the quickest respiration, 38; in the slowest, 12. The respiration is very quick during reading aloud. The inspiratory pause is very short—frequently altogether wanting; the expiratory pause bears on the average to the whole respiratory movement the proportion of 10 : 44. 3. Another series of experiments exhibit the relation between the respiratory movements and the state of repletion of the lungs with air. The authors arrived at the following results:—*a*. The more calm the respiration, the less replete are the lungs with air, and *vice versâ*. 100 being the figure for the vital capacity, 19·3 corresponded in an average of nine experiments to the lowest point at the commencement of the inspiration, while during excited respiration, 56·9 was the lowest reading. *b*. The repletion of the lungs during the state of calmness is such, that the quantity of air changed by each respiratory act is about one-third greater than that which remains in the lungs, and might be expelled by forced expiration. *c*. The average repletion of the lungs was 33·5 per cent. of the vital capacity. Experiments with the spirometer led to the same result. *d*. The repletion of the lungs at the commencement of each inspiration varies much in the same experiment. *e*. When the state of repletion is lower, the inspirations are more profound than when it is higher.

Moleschott and Schelske employed in their experiments the Rana esculenta, and several nearly allied genera and species of animals. The following inferences are of physiological interest:—1. The batrachians yield, in proportion to their weight, less carbonic acid than man, but when they breathe in air charged with moisture, the difference is not so great as is generally assumed. If we adopt, with

Donders, 1593 milligrammes of carbonic acid within 24 hours, for 100 grammes of man, as the unity, Rana esculenta yields 0·37; Hyla arborea, 0·39; Triton cristatus, 0·68; Rana temporaria, 0·69; Bufo cinereus, 0·25; Bufo calamita, 0·37. 2. Several species of the same genus exhibit a wide difference in the proportionate quantity of carbonic acid excreted and in the size of their livers (weight of liver for 100 grammes weight of body in Rana esculenta, 6·0 grammes; in Rana temporaria, 3·37 grammes). It is therefore unsafe to transfer the laws of metamorphosis of matter found for one species to another one, although nearly allied. 3. Among the nearly allied animals, the most inert (Bufo cinereus, Salamandra maculata) yield the smallest, the most active (Rana, Bufo Calamita, Hyla, Triton) the largest amount of carbonic acid. 4. The comparison between Hyla arborea and Hyla esculenta shows that between nearly-allied animals, living more in the air and less in the water, yields the larger proportion of carbonic acid. 5. The experiments made on the two sexes separately corroborate (with the exception of Triton) the law found already by Andral and Gavarret, that the male sex produces, for the same weight, more carbonic acid than the female; the proportion in Bufo cinereus =1·43 : 1, in Rana esculenta and temporaria=1·28 : 1·6. 6. The liver can in no way be considered as a measure for the excretion of carbonic acid in different species of animals.

In another series of experiments on the influence of light—made likewise on frogs—Moleschott arrived at the following results:—1. Frogs excrete, the temperature being the same, or nearly the same, for an equal weight of body, considerably more (from one-twelfth to one-fourth of the whole amount) carbonic acid when under the influence of light, than when kept in the dark. 2. The more intense the light, the greater the amount of carbonic acid excreted. 3. The light appears to exercise its influence partly through the eyes, partly through the skin.

Valentin's researches, made on the muscles and other parts of the Rana esculenta, manifest that there is a constant interchange between the muscles and surrounding atmosphere, and that the muscles endowed with irritability and those deprived of it, exhibit a great difference in this respect. The difference becomes evident as soon as the muscle is dead, whether this death is caused suddenly, as by cold, heat, mechanical injury, &c., or gradually, as in the amputated limb. Again, a difference is perceptible in the action of the dead muscle on the surrounding atmosphere, according to the manner in which the death has been effected (cold, heat, mechanical power, &c.). The difference between the action of the living and that of the dead muscle increases with the advancing decomposition of the latter. The living muscle produces a considerable diminution of the volume of the surrounding atmosphere; this diminution becomes less evident as soon as the irritability ceases; by degrees it becomes equal to zero, and during putrefaction an increase of volume is observed. These changes are caused principally by the absorption of oxygen, the development of carbonic acid and nitrogen. Concerning the *oxygen*, Valentin remarks, that as well the living as the dead muscle absorb more than is proportionate to the carbonic acid given off by them. Not only the muscles, but also the other tissues, exercise an influence on the surrounding atmosphere, by absorbing oxygen and yielding carbonic acid. The quantity of the *nitrogen* undergoes only slight alteration, or none at all, while the irritability of the muscle persists, but development of nitrogen takes place as soon as the decomposition commences.

Callenfels considers—*a.* The nature of the periodic contractions and dilatations of the arteries of the ear of rabbits, lately described by Schiff. He admits the existence of this phenomenon, but could not observe the regularity and frequency, as represented by Schiff. While the latter had seen from two to six periodic changes in a minute, Callenfels found each change occupy a whole minute or more. During cold weather, the state of contraction continued sometimes even for hours; while in warm weather, the dilatation predominated. A close relation existed always between the lumen of the vessels and the temperature of the ears, the latter

being low during the contraction, high during the dilatation, of the vessels. He is inclined to ascribe, from his observations, to the ears of the rabbit the function of husbanding the temperature of the animal, by giving off much warmth when the vessels are dilated, and little when they are contracted.

b. The experiments on the *sympathetic nerve* on the neck lead Callenfels, on the whole, to similar results as those obtained by Bernard and others;* he found, however, in opposition to Bernard, that mere section of the nerve exercises a greater influence on the temperature than extirpation of the ganglion supremum. And again, in contradiction to the same author, he states that "the connexion between the dilatation of vessels and the temperature is so close, that we can almost ascertain the temperature of the ear by mere inspection."

c. Concerning the vessels of the pia mater, Callenfels obtained, after many negative results, the decided proof, that the arteries of the pia mater are under the influence of the sympathetic nerve on the neck. Irritation of the nerve produced distinct contraction of the small arteries on the same side; discontinuance of the irritation was followed immediately by dilatation of the same vessels.

Kussmaul and Tenner draw, from their ingenious experiments on rabbits, the inference, that the *sympathetic* nerve has no direct influence on the formation of animal heat, as Bernard had concluded; but that it acts merely through the coats of the bloodvessels, by allowing a larger or smaller quantity of blood to enter into them. Our authors adopt, therefore, the mechanical view propounded by Donders, Schiff, Callenfels, and others. They do so, on the following results of their experiments:—1. By direct increase of the supply of blood, the same increase of temperature of the ears may be effected, and even a greater one, than by paralysis of the sympathetic nerve. 2. By direct arrest of the supply of blood, the same degree of lowering of temperature may be obtained as by irritation of the sympathetic nerve. 3. The differences in the temperature of both ears, effected through the arrest of the supply of blood on one side, are as great as those produced by paralysis or irritation of the sympathetic nerve on one side. 4. The arrest of the supply of blood on *one* side causes increased redness and warmth in the ear of the opposite side, just as irritation of the sympathetic on *one* side. 5. After section of the sympathetic nerve, the temperature of the ear can be still increased by increasing the lateral pressure in the vessels. 6. In the ear deprived of blood, the decrease of temperature continues in spite of section of the sympathetic nerve. 7. The temperature in the ear sinks more rapidly in consequence of arrest of the supply of blood, if the sympathetic nerve has been previously cut.

II. Lymphatic System and Ductless Glands.

1. Krause: *Contribution to the Physiology of the Lymph.* (Henle und Pfeufer's Zeitsch f. rat. Med., vii. pp. 148 ss.)
2. Führer and H. Ludwig: *On the Physiological Compensation of the Spleen, and on the Sources of Urea.* (Vierordt's Arch., xiv. pp. 307 ss. 1855.)

Krause collected the lymph from the truncus lymphaticus cervicalis of dogs, previously narcotized by opium. His experiments show—1. That the quantity of lymph yielded by one kilogramme of the dog, in the state of fasting, fluctuated between 246 and 638 grammes, the average being 435 grammes; 2. That it is not materially reduced by diminished tension in the arterial system (tying of the carotids); 3. That it is increased by irritation of the sensitive nerves; 4. That the composition of the lymph varies considerably; 5. The reaction was in all cases slightly alkaline; 6. The existence of *leucin* (Staedeler and Frerichs) could not be distinctly proved; 7. The presence of sugar (of milk or grapes) became, by the use of Trommer's test, very probable.

* British and Foreign Medico-Chirurgical Review, No. 33, p. 231. 1856.

Führer and H. Ludwig corroborate the result, already obtained by other experimenters, that the lymphatic glands become considerably enlarged after the extirpation of the spleen. The glands of the abdomen, chest, and neck exhibited the greatest increase; while those of the legs and inguinal regions were little altered. The convexity of the upper and the concavity of the lower surface of the hypertrophied glands were very marked, the veins issuing from the hilus much increased in diameter, the lymphatic vessels exhibiting only the usual size. The authors attribute the enlargement principally to the exaggerated development of the glandular bodies (Brücke, Donders, Kölliker), which appear to be surrounded by a close network of newly-formed capillaries, analogous to those described in the spleen.* Führer and Ludwig are of opinion that the lymphatic glands, thus developed, exercise, instead of the spleen, the function of forming blood-corpuscles.

Concerning the final destination of the blood-globules, the authors entertain the view, that they form the principal source of urea. They thus object as well to the theory of Liebig and Bischoff, that the urea is the production of the "metamorphosis of matter of the solid tissues" (this being, according to Führer and Ludwig's view, far too steady and too slow to account for the frequent changes in the quantity of urea, and its enormous and rapid increase, in consequence of increased ingestion of azotized food); as also to that of Frerichs, Bidder and Schmidt, who maintained that a part of the urea was derived directly from the metamorphosis of the overplus-consumption—i.e., that part of food which is taken over and above what is required for the conservation of the system. Führer and Ludwig contend that, in the same way as bile, milk, and mucus are not excreted directly from the blood, but through the intermediate formation of cells, thus also urea is eliminated by means of cells—namely, the blood-globules. As favourable to this opinion may be mentioned the rapid development, short duration, and frequent change of the blood-globules, as also the absence of any other known metamorphosis of these bodies.

III. Secretion; Metamorphosis of Matter.

1. Boedeker: *On the varying Composition of the Milk at different Times of the Day.* (Henle und Pfeufer's Zeits. f. rat. Med., vi. 2, 1855.)
2. C. G. Lehmann: *Communication concerning the Question of the Formation of Sugar in the Liver.* (Schmidt's Jahrb., vol. lxxxvii. pp. 282 ss. 1855.)
3. Cl. Bernard: *Sur le Méchanisme de la Formation de Sucre dans le Foie.* (L'Union Méd., No. 119, vol. ix. 1855.)
4. Limpert and Falck: *Researches on the Excretion of Sugar through the Kidneys, when Injected into the Blood.* (Virchow's Archiv, ix. 2, pp. 56 ss. 1856.)
5. H. Nasse: *On the Section of the Vagi*—see under the head of *Nervous System.*

The milk examined by Boedeker was that of a cow in good state of health, fourteen days after calving. The cow was fed between 6 and 10 A.M., and between 5 and 8 P.M., with hay, oat-straw, beet-roots, oil-cakes, and ground beans; it had an additional meal of oat-straw at 10 P.M.; it was milked at 4 A.M., at noon, and at 7 P.M. Leaving the chemical details to the Report on Physiological Chemistry, we give here only the principal results of repeated examinations:—1. The percentage of *fat* was smallest in the morning milk, larger in the milk of noon, and largest in that of the evening, which bore to that of the morning milk the proportion of 2 to 1—viz., sixteen ounces of milk obtained in the evening contained six drachms of butter, of milk obtained in the morning only three drachms. 2. *Casein* was likewise increased in the evening milk, but not in so considerable a degree as fat. 3. The quantity of *albumen* was diminished in the milk of the evening in very nearly the same proportion as the casein was increased. 4. The

* British and Foreign Medico-Chirurgical Review, vol. xxvii. p. 253.

sugar of milk and the *salts* exhibited only slight variations at the three periods. These facts show again, how necessary it is, to examine the secretions and excretions of the body at different periods of the day, in order to obtain an accurate knowledge of their composition.

Lehmann communicates the results of several new experiments, performed partly on horses, killed five hours after the last meal; partly on dogs, of which some had been killed in the fasting state, others five hours after a meal of raw meat, others after one consisting of boiled potatoes. The blood of the portal vein of the dogs, killed in the state of fasting, and of those fed with raw meat, contained no sugar; that of the horses, and of the remaining dogs—viz., those fed with potatoes, only a small quantity; while the blood of the hepatic vein exhibited in all cases a very large amount. Lehmann attributes the origin of the sugar formed in the liver, in part at least, to the fibrin and albumen, the proportion of which is diminished in the blood of the hepatic vein. The author's repeated experiments confirm again the fact pointed out by Bernard, that the arterial blood is usually free from sugar, that only when the venous blood in the right ventricle contained 3 per cent. of sugar or more (viz., in cases of diabetes mellitus), a part of the latter passes into the arterial blood. Poggiale* has recently arrived at results very similar to those of Lehmann, concerning the proportion of sugar in the blood obtained from different vessels. Leconte,† too, found in his experiments on dogs no sugar in the blood of the portal vein after meals of meat, while the blood of the hepatic vein contained 0·1 to 0·4 per cent.

Bernard took the liver of a dog, fed exclusively on meat, immediately after death by section of the medulla oblongata; he washed it out by a continued stream of water through its vessels, so completely, that it was quite exsanguious, and that the decoction of a piece of it did not any more yield a trace of sugar. When he examined the remainder of this liver after twenty-four hours, he found it to contain a very large amount of saccharine matter. From this simple experiment, frequently repeated with the same result, Bernard concludes, that the sugar is not formed, as other physiologists have suggested, by a kind of catalytic action, exercised by the glandular tissue on a constituent of the blood, while it circulates through the liver, but by the metamorphosis of a substance contained in the tissue of the liver itself. The same experiment shows, that this substance, which is to be transformed into sugar, is insoluble in water; the author has further convinced himself, that it is also insoluble in alcohol and ether; that its transformation into sugar is, in general, terminated within twenty-four hours; that it is accelerated by the free exposure to the atmospheric air (as by cutting the liver into very small pieces); that, on the other hand, this faculty of undergoing the transmutation in question is destroyed by the process of boiling. Bernard remarks, that, in the state of health, this substance is constantly reproduced in the tissues of the liver, and as constantly afterwards transmuted into saccharine matter.

Limpert and Falck injected milk-sugar, grape-sugar, and cane-sugar into the jugular veins of dogs, in solutions containing from 5 to 13 grammes of the one or other kind of sugar. The urine was obtained by means of the catheter. The principal results of their experiments are—1. Of 5 grammes of *sugar of milk* injected in 4 cases, 2·6 grammes were excreted with the urine in two cases, 2·04 grammes in the third, 3·36 grammes in the fourth. The excretion of sugar with the urine had in all cases ceased after five hours. 2. Of 5 and 7 grammes of *grape-sugar*, only very slight traces were found in the urine, and even these traces only within the first couple of hours after the injection. 3. Of 10 and 13 grammes of *grape-sugar*, 1·45 grammes and 0·2 grammes were excreted within the first five hours—none later. 4. Of 8 grammes of *cane-sugar* injected in two cases, 5 grammes passed through the kidneys in one of them, 4·87 grammes in the other. This elimination was in both cases completed within seven hours. 5. Of each kind of

* Gazette de Paris, Nos. 17, 18. 1855.
† Annales des Sciences Naturelles, pp. 61 ss. 1855.

sugar, therefore, when injected in a considerable quantity, a part quitted the body through the kidneys, which part was largest for cane-sugar, smaller for milk-sugar, smallest for grape-sugar. It will be seen from this statement that the results obtained by Limpert and Falck are in accordance with those obtained by Bernard, Lehmann, Kersting, Uhle, and Becker. They differ from all of them, by taking into consideration the exact quantity of sugar secreted by the kidneys, and in common with only those of Becker, they give account of the time within which sugar is eliminated in this way.

IV. Nervous System.

1. Marmé and Moleschott: *On the Influence of Light on the Irritability of the Nerves.* (Moleschott's Untersuch., pp. 15 ss. 1856.)
2. Brown-Séquard: *Experimental and Clinical Researches on the Physiology and Pathology of the Spinal Cord and some other Parts of the Nervous Centres.* (Richmond, 1855.)
3. Brown-Séquard: *Experimental and Clinical Researches upon the Channels of Transmission of the Sensitive Impressions through the Spinal Cord and Medulla Oblongata.* (Med. Times and Gaz., Nos. 304-5, 1856.)
4. Pflüger: *On the Influence of the Anterior Roots of the Spinal Marrow on the Lumen of the Vessels.* (Preliminary Communication. Med. Centr. Zeit., Nos. 68 and 76. 1855. And Schmidt's Jahrb., vol. lxxxix. No. 1, 1856.)
5. H. Nasse: *On the Influence of Section of the Vagi in Dogs, with special regard to the Metamorphosis of Matter.* (Arch. f. Wissensch. Heilk., vol. ii. 3, 1856.)
6. Luschka: *On the Sensitive Branches of the Nervus Hypoglossus in Man.* (Müll. Arch. i. and ii. pp. 62 ss. 1856.)
7. Th. Weber: *On the Want of the Sense of Touch in Parts Denuded of the Cutis.* (Vierordt's Arch. f. Phys. Heilk., xiv. pp. 341 ss. 1855.)
8. Pflüger: *A System for Arresting the Peristaltic Movements of the Intestines.* (Monatsber. d. Berlin Akad., and Schmidt Jahrb., vol. lxxxix. No. 1, 1856.)

Callenfels', and Kussmaul's, and Tenner's Essays on the Sympathetic Nerve, are reported on under the head of "Animal Heat."

Marmé's and Moleschott's experiments prove distinctly that frogs kept in the light possess a higher degree of irritability of the nerves, and greater power of the muscles, than such that have been deprived of the influence of light; the sex, the size of the body, the state of nutrition, the time of the year, and the temperature, were of course in both cases the same.

Brown-Séquard has continued his ingenious researches on the physiology of the spinal marrow. We must restrict ourselves for the present to the most important results; the more so, as the author promises a detailed account in his 'Physiology and Pathology of the Spinal Cord,' which he is about publishing in Paris. Physiological experiments, together with pathological facts, lead Brown-Séquard to the following inferences:—1. The idea that the sensitive impressions are conducted to the encephalon along the posterior columns, is entirely erroneous. 2. The grey matter of the spinal cord, although itself deprived of sensibility, is an organ of transmission of the sensitive impressions. 3. There are two kinds of sensitive fibres in the posterior columns of the spinal cord, some going up towards the encephalon (centripetal or ascending fibres), some going in the opposite direction (centrifugal or descending fibres). 4. There are also ascending and descending fibres in the posterior grey horns, and very likely in the posterior parts of the lateral columns. 5. These ascending and descending fibres in the posterior columns come mostly, if not entirely, from the posterior roots of the spinal nerves. 6. The posterior roots send also fibres to the posterior grey horns, and very likely to the posterior parts of the lateral columns. 7. All these fibres soon leave the posterior

columns, the posterior grey horns, &c., in order to go into the central grey matter. 8. All these sensitive fibres decussate very near their entrance into the spinal marrow from the posterior roots. 9. There are some transverse fibres in the spinal cord, coming from the posterior roots, which do not seem to transmit sensitive impressions. 10. The *motor* nerves remain, after their entrance into the spinal marrow, on the same side, until they reach the lower part of the medulla oblongata, where they decussate.

Pflüger made his observations on frogs. By applying at first weak, then gradually-increasing, currents on the anterior roots of the crural nerves (by means of Du Bois-Reymond's apparatus), he constantly effected contraction of the arteries of the web-membrane. Sometimes even the largest arteries of the web became so completely contracted, that all the blood-globules disappeared from them, and this took place principally in a retrogressive direction. The greatest degree of contraction took place in general about ten or fifteen seconds after the commencement of the irritation. The same observation has been also made on the mesentery of the frog. Regarding the influence of irritation of the spinal marrow on the veins, the author promises further communications.

On the effect of *section of the vagi*, Nasse has performed a series of valuable experiments. Particular attention has been paid to the influence upon the animal temperature, the frequency of pulse, the respiration, the composition of blood and urine, the loss of weight, &c. Section of *only one* nerve causes some functional disturbance, which, in general, is only transitory—as increased frequency of respiration, diminished frequency of the contractions of the heart, increased secretion of saliva, &c. The most constant effect, however, is considerable emaciation, in spite of abundant ingestion of food. The blood shows increase of albumen and water, decrease of the number of blood-globules. The quantity of fæces increased, less digested; the urine contains a diminished amount of solids; perspiration above the average. Death was never caused by the section of one pneumogastric nerve; but when the animals were killed some days after the operation, they always exhibited hyperæmia of *both* lungs. Section of *both vagi* always proved fatal between the second and sixty-second day after the operation. The principal phenomena were:—1. Diminished number of respiratory movements: in the average of six cases, they fell from 18·1 to 12 soon after the operation, and to 5·6, in one case even to 3, within eight days. 2. Increased frequency of contractions of the heart, in the average from 113 to 165 (viz., about 38 per cent.). 3. Increased impulse of the heart. 4. As regards the pressure of the blood, Nasse's experiments gave not so constant a result as those of other observers, who contradict each other—(Bernard contends that it is diminished, Ludwig that it is increased);—his result is most in accordance with that of Lenz, who found the pressure above the standard soon after the section of the nerves, below it in a later period. 5. *Vomiting* in all cases, frequently without ingestion of food. 6. *Desire for food* at first rather increased, gradually decreasing, sometimes altogether lost. Thirst excessive. 7. *Digestion* much impaired, principally that of meat; milk, bread, and fat in small quantities are better borne. The alvine dejections are increased in quantity, and very offensive. The gastric juice not quite deprived of acid, but the latter much diminished, as also the pepsine. The absorption of poisons undisturbed. 8. The *temperature* in general below the average during the first days, rather above it after the fourth day, sinking again below it one or two days before death. 9. *Blood*-globules, albumen, and fibrin in excess; water in diminished proportion. 10. The *loss of weight* is much greater than in animals merely deprived of food; the author ascribes this in a great measure to the excess of perspiration and secretion of urine. The daily loss of weight was greatest in those animals that died soonest after the operation, but the total loss was largest in those which lasted out longest. Concerning the post-mortem phenomena we must refer to the original; we mention, however, the interesting fact, that the liver was found to contain no sugar, either immediately after death or a few days later (Bernard), with only one exception.

Although Luschka maintains the entire absence of a posterior ganglionic root in the nervus hypoglossus of man, yet he thinks that we are compelled, by the distribution of its branches, to admit its mixed nature. The anatomical examination teaches that the source of the sensitive fibres cannot exist in the origin of the nerve; it must therefore be looked for in its course from the centre to the periphery. The author proves that the sensitive elements cannot be derived from the communication with the sympathetic nerve, as only the latter receives motor fibres from the hypoglossus (iris-sympatheticus of Budge); he further shows, that the connexion with the cervical nerve is only transitory, and that with the vagus inconstant. Luschka then finds the source of the sensitive fibres in the *ramus lingualis* of the fifth pair, and in the ganglion sublinguale, from which some fibres constantly join the hypoglossus, and run along its trunk backwards to the place where they are distributed to the periphery. The sensitive elements thus obtained are distributed to the occipital bone, the vena jugularis, the sinus circularis of the foramen magnum, and to the circellus venosus hypoglossi—a circular sinus-like arrangement of veins, with very delicate membranes round the nervus hypoglossus at its entrance into the canalis nervi hypoglossi.

Th. Weber performed his experiments on a patient in whom the cutis and the sub-cutaneous cellular tissue had been recently destroyed on a great part of the right arm and fore-arm, in consequence of inflammation and gangrene of the subcutaneous cellular tissue; the muscles were denuded to a considerable extent. These experiments show that muscles deprived of skin do not possess the faculty of perceiving heat and cold, high degrees of warmth being perceived not as temperature, but only as pain; they further confirm, that the sensibility of muscles is in every respect less acute than that of the skin; the smallest distance between the points of the compass to be felt as two, amounted, in the longitudinal direction, to ten centimètres.

Pflüger communicates the very interesting discovery, that *irritation of the nervi splanchnici* arrests the motion of the small intestines. We have therefore, it appears, another instance of the phenomenon, that the increased action of a nerve stops the motion of a muscle. Pflüger draws his inference from the following experiments:—1. One of the electrodes of the apparatus was applied to the denuded muscles of the back of a rabbit, between the fifth and sixth, the other to those between the tenth and eleventh, vertebræ. As soon as the apparatus was put into action, the trunk and extremities became tetanized; while at the same time the peristaltic motion of the small intestines ceased altogether; the colon and rectum continued to move. The removal of the electrodes was immediately followed by the recommencement of the peristaltic motion. 2. The peristaltic movement of the intestines does not cease, if in the preceding experiment the splanchnic nerves have been cut through before the electricity is applied. 3. Application of the electricity to either of the splanchnici alone is sufficient to effect the cessation of the peristaltic motion of the small intestines.

HALF-YEARLY REPORT ON MATERIA MEDICA & THERAPEUTICS.

By Edward Ballard, M.D.,
Lecturer on the Principles and Practice of Medicine at the School of Medicine adjoining St. George's Hospital.

I. *On Cantharidin, and its relation to Spanish Flies.* By Dr. Schroff. (Zeitsch. der k. k. Gesellsch. du Aerzte zu Wien. July and Aug. 1855.)

Comparative experiments were made upon rabbits with the cantharidin and Spanish flies, and one comparative experiment by M. C. Heinrich, who took at one time 10 drops of a strong tincture of cantharides, prepared by himself from fresh undried flies; and at another time, 0·01 gramme of cantharidin. It is clear from these observations that the cantharidin is the irritating principle of the flies, as it not only produced gastro-enteritis, but, after absorption, also proved irritant to the urinary organs. One interesting result obtained by Heinrich is, that the cantharidin, although producing inflammation along the whole digestive tube, and in the urinary organs, failed to produce any excitement whatever of the sexual system, while the latter was a marked effect of the tincture of cantharides. The facts in our possession point to the volatile principle in the living flies, which gives them their disagreeable odour, as that which most rapidly occasions sexual excitement.

II. *On Lactate of Zinc in Epilepsy.* By M. Herpin. (Bull. Gén. de Thérap. Nov. 1855.)

M. Herpin points out the fallacy of deductions from cases treated *en masse* by any remedy, without classifying the cases, and taking the prognosis into consideration. He divides cases of epilepsy into three groups. 1. Where the prognosis is *favourable*. This embraces cases in which there have been less than 100 attacks. 2. *Little favourable* cases, where there have been from 100 to 500 attacks. 3. *Unfavourable* cases, where there have been above 500 attacks. The duration of the affection, together with the age and sex of the subject, also influence the prognosis. All things being equal in respect to the number of fits, the most recent cases are the most favourable. Under five months' duration, the chances of recovery are twice as great as from five months to a year. After ten years, success is rare. Of all ages, old age is the most favourable; then youth and infancy; and least of all, adult age. In M. Herpin's hands there have been twice as many failures with males than with females. Adult men are most unfavourable subjects. To apply this sort of division to the cases treated by lactate of zinc:—of 41 epileptics, the treatment was only sufficiently advanced in 35 for any decision as to its effect being arrived at. Of these 35, 15 were favourable cases, 12 little favourable, and 8 unfavourable. Of the 8 unfavourables, 2 have improved to an extent which militates strongly in favour of the remedy. Of the 12 little favourable cases, in 2 children, aged respectively eight years and twenty-one months, the fits were suppressed; and a remarkable amelioration took place in one man. Of the 15 favourable cases, 4, in which various other remedies had failed, were uninfluenced by the lactate; one of these had suffered 90 attacks in fifteen years and a half; a second, aged forty-four years, had symptoms of commencing general paralysis; a third, aged four or five years, had a hydrocephalic head; and a fourth, which had lasted three years and a half, was otherwise favourable. In 6 of the remaining 11, the attacks were suppressed; and of the 5 others, 3 have had the intervals so much prolonged, as to afford hope of complete cure on continuance of the remedy; the remaining two were amended. The remedy was given for a period of from five or six to twelve months.

III. *On Iodurretted Chloride of Mercury, and its Use in Acne Rosacea.* (Bull. Gén. de Thérap., Oct. 1855.)

This compound is formed either with one equivalent of iodine and two equivalents of calomel, or with single equivalents of the ingredients. For the preparation of the former compound, two equivalents of calomel are roughly powdered and introduced into a matrass, and gently heated and shaken till it begins to sublime; the iodine is then added in small portions, and the combination takes place noisily, without any sensible loss of iodine. To obtain the second compound, only one equivalent of calomel is used; the method of preparation being otherwise the same. The following formulæ are in use by M. Rochard:—*Pomade.* Take of ioduretted chloride of mercury, 0·75 grammes; fresh axunge, 60 grammes: mix carefully. *Pills.* Take of ioduretted chloride of mercury, 0·25 grammes; gum arabic, 1 gramme; bread crumb, 9 grammes; orange-flower water, sufficient for twenty-five pills, of which one to three are to be taken daily. These medicines are made with the first compound; the second is formed into sticks, to act as a caustic. In the treatment of acne rosacea, the pomade is used by M. Rochard once a day, by way of friction on the diseased surface. This is repeated for two or three consecutive days, leaving the parts uncovered in the interval. The skin becomes excited under its influence, the circulation accelerated, and the heat augmented; an abundant discharge of simple serosity or puriform matter escapes, and, by exposure, becomes converted into crusts, which cover the points affected by the disease. These crusts after awhile fall, leaving the surface less red and less indurated. A new application produces a new discharge and new crusts, which, after their fall, leave a surface even more deeply altered than the first time. After several repetitions of this process, the skin resumes quite its natural aspect and normal texture. The amendment is announced by a diminished energy of the reaction; and the time at length arrives when no further discharge is produced, and this is the period of cure. When this topical treatment fails, the medicine may be cautiously given internally.

IV. *On the Administration of Quinine in Intermittent Fevers, &c.* By M. Briquet. (Bull. Gén. de Thérap., Oct. 1855.)

M. Briquet discusses the two modes of administering quinine—viz., in *small* doses, where the object is to obtain the greatest febrifuge effect possible from the smallest dose; and next, in large doses.

1. *Administration in Small Doses.*—He gives thus an acid solution of 25 or 30 centigrammes, with 1 centigramme of acetate of morphia, in 120 grammes of eau sucrée in five doses in five consecutive hours; and he has established the following points. 1st. That when the last dose has been administered at the moment of the invasion of the paroxysm, the latter has been very rarely modified. 2nd. That when an interval of from six to eight hours has elapsed between the last dose and the future paroxysm, this was arrested or favourably modified in half the cases, and that almost always the succeeding fit was prevented. 3rd. That when an interval of twelve hours had elapsed, the paroxysm was arrested in three-fourths of the cases, and the succeeding paroxysm constantly. 4th. That when an interval of fifteen hours was left, the future paroxysm never failed to be stopped, and the fever cut short completely. M. Briquet concludes from these that the sulphate should be commenced twenty hours before the fit, so that the specified quantity may be taken in the course of five hours, and that an interval of fifteen hours may be left between the last dose and the invasion of the paroxysm. This method, he says, is applicable to all simple cases, for we can always get eighteen to twenty hours between two paroxysms of quotidian fever, the paroxysms of which last, at the most, from four to five hours. In other types, tertian and quartan, there is no lack of time for using the quinine.

2. *Administration in Large Doses.*—In the fevers which are not simple—e. g., the double tertians, very severe quotidians, pernicious intermittents, and severe remittents, the time for acting with small doses is not obtainable, and recourse must be had to large doses. As it is known that, given in large doses, the quinine in solution begins to be absorbed almost instantly, and that it operates with all its intensity upon the nervous system in from half-an-hour to an hour after ingestion, it only remains to calculate the doses, so that the maximum of hypersthenization may be produced the greatest possible number of hours before the arrival of the paroxysm. In this way, with an interval of six hours between two paroxysms, there will be sufficient time—three hours to give the sulphate in, and three hours for it to act. Thus what is wanting in time is made up by quantity, which latter we can always control. He gives the following rules for avoiding the dangers and accidents which attend the use of large doses:—1st. To employ only a soluble preparation of quinine, and to give it in the form of solution. 2nd. To give it in divided doses, leaving an interval of an hour between each dose, and continuing it only during ten or twelve hours of the day, leaving the patient at rest during the remainder of the twenty-four hours. The quantities should consist of from 15 to 20 centigrammes an hour. 3rd. It is necessary gradually to increase the dose of each day in proportion to the resistance that the disease opposes. This increase may be from 40 to 50 centigrammes a-day; of course having regard to the tolerance of the patient. 4th. The sulphate of the alkaloids, and by preference the bisulphates, are the preparations to be employed. 5th. The treatment by large doses ought not to be employed with nervous impressible subjects, disposed to cerebral congestions, except with much attention, and very circumspectly. M. Briquet considers that the salts of morphia, united with the quinine, lessen the primitive excitant effect of this alkaloid, and notably increase its hypersthenic action: hence 10·2 centigrammes of acetate of morphia are advantageously added to 30 or 40 centigrammes of quinine. He says that bloodletting renders the nervous system more susceptible to the influence of quinine, and allows the quantity to be lessened without diminishing its febrifuge effect. In pernicious fevers, and acute articular rheumatism, where congestive phenomena exist, he considers bleeding a valuable adjuvant. He thinks that the value of emetics, as preparatory to the use of quinine, lies in their promoting the absorption of the latter.

V. *On Tar-Frictions—Absorption of the Tar.* By Dr. PETTERS. (Vierteljahrsch. für die Praktische Heilkunde, Band iii. 1855.)

Dr. Petters, in the instance of two individuals who were employing tar-ointment for the cure of psoriasis, has determined the fact of the absorption of the tar by the skin, and its excretion by the urine. He has specially determined the presence of carbolic acid, $C_{12} H_6 O_2$, in the urine of these patients, not free, but combined with soda; and considers that almost all the principles of the tar may pass, under such circumstances, through the organism, either in an unaltered or altered form.

VI. *On Coniin.* By Dr. SCHROFF. (Wochenblatt der Zeitschr. der k. k. Gesellsch. der Aerzte zu Wien, No. 2 and seq. 1856.)

Twenty-seven experiments were made with coniin upon the human subject, three medical gentlemen having each submitted to nine. The doses given varied from 0·003 grammes to 0·085 grammes. The last and strongest dose which was taken corresponded to two drops of newly-prepared coniin taken out of a bottle opened for the first time. Dr. Schroff has found, by observations on rabbits, that exposure to the air weakens the operation of the alkaloid. This dose was dissolved in thirty drops of alcohol. The following account of the symptoms produced embraces those which resulted from the operation of smaller quantities. A very

sharp taste, strong burning in the mouth, sense of scraping in the throat, salivation; the epithelium of the tongue was removed in spots; the papillæ were strongly prominent, and the organ lost sensibility, and was as if paralysed. In about three minutes, the head and face became very warm, accompanied by a sense of fulness, weight, and pressure in the head (symptoms which were not produced by the smaller doses). These head symptoms reached a high degree of intensity; became associated with giddiness, inability to think or to fix the attention on one subject, with sleepiness, great general discomfort, and malaise (*Katzenjammer*), which, in a less degree, lasted till next day. The vision was indistinct, objects floating together, and the pupil was dilated; the hearing was obtuse, as if the ears were stopped with cotton; the sense of touch was indistinct, and there was a feeling of formication, and as if the skin were covered with fur; general weakness and prostration, so that the head was with difficulty kept erect; the upper extremities could only be moved with the exertion of much effort; and, on account of the weakness of the lower extremities, the walk was very uncertain and tottering. Even the next day the weakness of the extremities continued, slight trembling being induced by much movement. While going home, the muscular debility was especially great, the walk consisting rather of a throwing forward of the body, so as to bring the muscular action into as little use as possible. On stepping, and, when at home, on pulling off the boots, cramps in the calves of the legs occurred, as well as in other groups of muscles when they were called into action—as, for instance, in the balls of the thumbs when the thumbs were closely bent. This symptom was constantly observed in two of the experimenters when the dose was at least one drop. Under strong effort to move, pain in the muscles and legs occurred. Fresh air diminished the giddiness and fulness in the head, but in one of the experimenters, occasioned temporary pain in the course of the supra orbitalis and cutaneus malæ nerves. Eructations, abdominal rumbling and distension, nausea, even efforts at vomiting, occurred in all the subjects, even after small doses; in one case, actual vomiting took place. Sometimes there was a tendency to diarrhœa. No effect was produced upon the urine. In all the cases there was dampness of the ends of the fingers; and after large doses, the hands were absolutely moist. The countenance was sunken and pale; the hands were cold and blue. After the larger doses, the pulse commonly increased in frequency to the extent of a few beats, but subsequently it always lessened; yet this diminution did not bear that relation to the extent of the dose as where aconite was given. Respiration was often yawning, but otherwise no constant anomaly presented itself. The sleep was good, and mostly very sound.

VII. *On the Use of a new Solution of Iodine in various Skin Diseases.* By Dr. Max Richter. (Wochenblatt der Zeitschrift der k. k. Gesellsch. der Aerzte zu Wien, 1855. No 51.)

The solution is made thus:—Half an ounce of iodine is to be dissolved in an ounce of glycerine, and subsequently half an ounce of iodine is to be added, which completely dissolves in a few hours. In the experiments made with this solution, it was applied to the surface by means of a hair pencil; the part was then covered with gutta percha paper, fixed at the edges with strips of plaster, so as to prevent the volatilization of the iodine. This was removed after twenty-four hours; and for a similar time, cold pledgets were applied. Burning pain, more or less intense, but rarely of more than two hours' duration, was produced. The repetition of the painting depends on the appearance of the part and the amount of disease. The conclusions of the author are—1. That the iodine thus applied acts as a caustic; 2. That while it possesses considerable curative powers in respect of scrofulous and syphilitic affections, it is especially useful in lupus; 3. That the solution dissipates even deeply-seated tubercles of lupus, and may be applied for this purpose to the most tender surface without fear of eroding it; 4. That when the solution was

applied only to a part of a diseased surface, the remainder was, nevertheless, influenced; 5. That it is particularly serviceable to large and superficial sores; 6. That after a series of paintings, and when the sore was almost healed, the local pains greatly increased in intensity.

Albuminuria.—M. Mauthner,* in the albuminuria and dropsy after scarlatina in children, recommends the exclusive use of a milk or rice-milk diet. Under its influence, he says, an abundant urinary flux becomes established, which causes the disappearance of the dropsy; but should it not suffice for the cure, he would seek to modify the urinary secretion by the use of alkalies. He recommends urea to be given in such cases, in doses of one-third of a grain, mixed with sugar. When a dose of 6 or 8 grains is arrived at, he says, it occasions an abundant secretion of urine, and rapid disappearance of the dropsy.

Chilblains.—Professor Berthold† employs decoction of nut-galls as a bath, or applied by means of pledgets. The itching and burning disappear in two or three days, but in old cases the remedy must be continued longer. Oak bark (1 ℔. to 2 ℔s. of water) may be employed as a poultice. These remedies are not applicable to broken chilblains.

Convulsions—Chloroform Inhalations.—M. Marotte‡ relates the case of an infant, eleven months old, suffering from convulsions, with spasm of the glottis, during dentition. Chloroform was very cautiously administered, with immediate relief; and in half an hour sleep was procured, and kept up by occasional respiration of the vapour for two hours, and then natural sleep ensued. Several relapses occurred—decreasing however in severity—each of which was treated in a similar manner; and altogether, 45 grammes of chloroform were expended.

Congenital Hernia.—M. Jobert§ relates four cases treated successfully by *iodine injections.* The plan was proposed by Velpeau, eighteen or twenty years ago. M. Jobert does not cut into the sac, after Velpeau's method, but merely punctures it, varying the operation a little, according as the sac is full or not of liquid.

Cephalalgia—Hydrochlorate of Morphia in Coffee.—M. Boileau‖ relates an obstinate case of cephalalgia, which he treated by hydrochlorate of morphia dissolved in strong infusion of coffee. The attacks occurred especially on any exposure of the head to cold air, and had resisted the operation of each of these remedies separately. It ceased almost immediately after taking them in combination, and by repeating the dose on each recurrence of the pain, the attacks became less frequent, and at length disappeared. M. Boileau says that he has found it successful in many other instances.

Chloroform.—Mr. Syme, in a clinical lecture,¶ directs attention to the importance of watching the respiration during the administration of chloroform vapour; regarding the indications afforded by the breathing as of greater value than those furnished by the pulse. On respiration becoming difficult, he directs that the mouth should be opened, and the tip of the tongue being seized with artery forceps, that it should be well drawn forwards.

Dr. Snow** has no hesitation in administering it, even in persons with fatty degeneration of the heart, believing that it is more likely to save life in such cases under operation than to destroy it, by preventing not only the straining and holding the breath, which would induce an over-distended state of the right cavities of the heart, but also the direct sedative operation of pain. He has given it to a number of persons with all the symptoms of fatty heart. His experience also

* Arch. Gén., April, 1855. † Pharm. Journ., Oct. 1855.
‡ Bull. Gén. de Thérap., April, 1855. § Rev. Méd.-Chir., April, 1855.
‖ Rev. Méd.-Chir., Feb. 1855. ¶ Lancet, Jan. 1855. ** Ibid., Oct. 20, 1855.

leads him to overlook the presence of chronic disease of the respiratory organs. He has given it without any ill effects in many cases where more or less paralysis remained from previous apoplexy, where patients have been reduced by various causes to a state of extreme debility, and in excessive exhaustion, in strangulated hernia, or compound fractures. He has also administered it to infants from ten days to three weeks old, and to one patient nearly ninety.

Chorea—Inhalation of Chloroform.—According to Dr. Géry,* chloroform inhalations have been used with advantage at the Hôp. des Enfans in severe cases where the violence of the movements have been beyond the control of opium or belladonna. It has been found at once to calm the movements and produce sleep, and in this way time has been gained for the employment of other remedies. On the first application of the vapour, the intensity of the movements is often greatly increased, but a calm succeeds as the inhalation is continued. Sound sleep thus induced lasts in children for ten or fifteen minutes, or even half an hour, and no ill effects have been observed to follow. The usual precautions, however, which are taken in the instance of adults, are necessary to be observed, such as ensuring that the stomach be empty, removing all obstacles to the respiratory movements, and watching the respiration and pulse, &c. The usual quantity administered has been ten to twenty grammes.

Dr. Bouchard† relates a case of a girl in which severe chorea had lasted twenty-one days. She was subjected to the influence of chloroform twenty-seven times in fourteen days, at first twice, then three times, and lastly once a day, at the end of which time she was cured.

Chorea.—The *gymnastic treatment* of chorea has already been discussed in this journal, and we now subjoin the conclusions drawn from an extended experience of its use in the Hôpital des Enfans by M. Blache.‡ 1st. No other method of treatment applied to chorea has produced so large a number of cures as the gymnastic treatment, either alone or associated with sulphurous baths. 2nd. It may be employed in almost all cases, without being arrested by the various contra-indications which present themselves at each step in the use of the other remedies for the disease. 3rd. The cure is obtained in a mean number of days about equal to that which the sulphurous baths require, but it seems to be more lasting, and the diminution of the affection is exhibited from the first. 4th. At the same time that the disorder of the movements disappears, the general health of the children sensibly improves, and the patients depart not only cured of the chorea, but also of the anæmia which most frequently accompanies it. 5th. The gymnastic exercises, which might be regarded as perilous, especially in the instance of the children who are submitted to them, present no danger at all, and may be put in practice without inconvenience in all seasons, an advantage which the baths do not possess. 6th. It is important to divide the exercises into two categories—*a*, the *passive* exercises, which can alone be employed in that period of the affection where the will has no influence over the muscular powers; and *b*, the active exercises, which the children execute themselves, either with or without the aid of apparatus.

Diabetes.—M. Durand Fardel has observed that the use of the Vichy waters for twenty or thirty days is palliative in the greater number of cases, the sugar disappearing almost or entirely from the urine, the secretion becoming less abundant, the dryness of the tongue and skin lessening, and the general strength, digestion, and nutrition, becoming improved. The symptoms return, however, at a period more or less distant from the cessation of the treatment. The palliation, however, lasts longer than that obtained by mere dietetic restrictions, which moreover may be somewhat relaxed during the use of the waters.

* Bull. Gén. de Thérap.. March, 1855. † Ibid., July, 1855.
‡ Rev. Méd.-Chir., Aug. 1855.

Ergot of Wheat.—Dr. Jobert* makes the following statements respecting this substance:—1. The medical and obstetrical property of this ergot is as incontestable as of ergot of rye, and its effects are as prompt, as direct, and as great. 2. Its hæmostatic action appears certain. Dr. Jobert has administered it several times against abundant discharges of blood, and immediately after labour it has almost constantly and fully succeeded. 3. In the dose of one or two grammes, according to urgency, in cases of uterine hæmorrhage, during any period of pregnancy, it has frequently succeeded in lessening, if not in completely arresting, the hæmorrhage; and this without appearing to produce any stimulant action on the uterus.

Epilepsy—Cotyledon Umbilicus.—Dr. Sieveking† narrates several cases in which this medicine was administered. No satisfactory conclusion can be drawn from them in respect to its efficacy.

Facial Neuralgia.—M. Lecointe‡ has employed chamomile, both in powder and concentrated infusion, in facial neuralgia, both periodic and non-periodic, with good results, even after other means of relief had failed; and believes it may in certain cases advantageously supplant the Peruvian bark. The dose, however, must not be less than four grammes; or, if given in infusion, the latter must be strong.

Ferrocyanide of Potassium and Urea.—Dr. V. Baud§ proposes this compound as a substitute for quinine in the treatment of some periodical diseases. He considers it applicable to those fevers, neuralgia, spasmodic diseases, and neuroses in which the intermittence is idiopathic, and not the result of marsh malaria. Its bitterness requires that it should be given in pills. M. Baud has usually given ten to fifteen pills of 15 centigrammes in the course of the day.

Gangrene of the Lung—Terebinthinate Inhalations.—Dr. Helm‖ narrates a case thus treated with success. The turpentine was poured upon hot water in Mudge's apparatus, and the vapour inhaled three times a day. The quantity of the expectoration began to diminish during the second week of the treatment. In the third week, the fœtidity lessened, and it lost its purulent aspect; and by the end of the fourth week the symptoms of the affection had almost disappeared. In the course of the next ten days all cough and expectoration ceased, and nothing morbid was any longer discoverable on examination of the chest.

Hæmorrhoidal Tumours—Actual Cautery.—M. Arthaud¶ directs attention to the advantages of the actual cautery to the treatment by ligature or excision, and relates some cases thus treated by M. Nélaton. He points out the precautions necessary to the operation. It is often followed by more or less severe vesical tenesmus, and sometimes by retention of urine. A tepid bath may not only relieve these symptoms, but calm the pain which succeeds the cautery. A light diet is recommended for the first five or six days, with a view to defer any action of the bowels. Towards the sixth day, however, in cachectic and debilitated subjects, an improved diet is indispensable, and tonics and steel must be prescribed. M. Nélaton has never observed any contraction of the rectum to result from this operation.

Hæmoptysis.—M. Aran** passes under review the several remedies which have been proposed to arrest this hæmorrhage. Venesection he considers not only to be useless, and its employment based upon no solid foundation, but dangerous, especially in phthisical subjects. He distributes the other remedies under three

* Gaz. des Hôpitaux, March, 1855.
† Med. Times and Gaz., Dec. 2, 1854.
‡ Rev. Méd.-Chir., Jan. 1855.
§ L'Union Médicale, May, 1855.
‖ Wochenblatt der Zeitsch. der k. k. Gesellsch. der Aerzte zu Wien, Aug. 1855.
¶ Rev. Méd.-Chir., Feb. 1855.
** Bull. Gén. de Thérap., p. 192, 1855.

heads—hæmostatics proper, astringents, nauseants and emetics, and sedatives to the circulating system. Among the medicines of the first class, he recommends the use of oil of turpentine in debilitated and cachectic subjects, where the hæmorrhage is characterized by passiveness and atony. He regards common salt also, in large doses, as possessing incontestable efficacy. Among the astringents, he considers the acetate of lead to be adapted chiefly to the chronic and prolonged hæmoptyses, and also places great confidence in gallic acid. Admitting the efficacy of nauseants and emetics, ipecacuan, tartar emetic, and especially of veratrine, in the arrest of the hæmorrhage, he points out that precedence will always be accorded to less disagreeable remedies. The principal medicine to which he draws attention in the last class, is a combination of nitre and digitalis. In ordinary cases he gives, in four doses during the twenty-four hours, 30 centigrammes of digitalis and 1 gramme 50 centigrammes of nitre: when the hæmorrhage is more severe, he has carried the quantity of nitre to 2 grammes 50 centigrammes, and that of digitalis to 50 or 75 centigrammes, and in some very rare instances to 1 gramme 50 centigrammes; while the quantity of nitre has been raised to 4 grammes. In the extremely abundant hæmoptyses, depressing medicines must give place to turpentine and gallic acid, and even these must not be relied upon alone. He recommends the application of ligatures to the limbs and ice to the chest as means of arresting the bleeding temporarily, and of affording time for the internal medicines to complete the cure.

Hydatid Cyst of the Liver—Injection of Alcohol.—M. Richard* relates the case of a lady, aged forty, in which the cyst was thus treated. It was situated in the left lobe of the liver. It was evacuated by a fine trocar, and then 8 grammes of alcohol, of the strength of 36° (Beaume's aërometer), were injected and left in the cyst, the canula being quickly withdrawn. There was some acute pain for about five minutes. In two days' time there were some signs of reappearance of the tumour, followed by vomitings, loss of appetite, fever, and jaundice, which disappeared under appropriate treatment; the tumour lessened, and in three months' time had completely vanished.

Inhalation of the Vapour of Sal Ammoniac.—Dr. Gieseler,† after inhaling the vapour to satisfy himself of its little irritating quality, administered it in several cases of chronic catarrh. The salt may be volatilized in a small Hessian crucible, heated by a spirit-lamp. The patient sitting before it, inhales the fumes, and the air of the apartment becomes also impregnated. Dr. Gieseler has found these inhalations, employed two or three times a day, cure obstinate catarrhs in a few days, and has on no occasion found them useless. He recommends the vapour also in syndesmitis and strumous ophthalmia.

Iodide of Iron Pills.—M. Perrens‡ proposes the preparation of pills of the iodide according to the following formula:—Take of iodide, 1 gramme; powder of iron (not oxidized), 1 gramme; honey, 1 gramme; liquorice powder, 2 grammes. Rub together rapidly in an iron mortar the iodine and the powder of iron until they are completely mixed, then add the honey, and beat it till the mass becomes black and ceases to exhale an odour of iodine, then incorporate the liquorice powder with it, and divide rapidly into twenty-five pills. Silver them, and preserve in a stoppered bottle, as they are slightly deliquescent. The presence of an excess of iron preserves the iodide for an indefinite period from the oxidating influence of the air.

Itch.—MM. Dusard and Pillon§ have employed with success the external application of a solution of 12 grammes of chloride of sulphur in 100 grammes of sulphuret of carbon. They pass lightly over the surface of the body a piece of lint

* Bull. Gén. de Thérap., May, 1855. † Henle und Pfeufer's Zeitsch., Band v. Heft 3.
‡ Bull. Gén. de Thérap., March, 1855. § L'Union Médicale, p. 439, Sept. 15, 1855.

soaked in the solution, an operation which does not occupy five minutes. The itching is said to cease at once, and the only subsequent treatment used after thirty-six hours is a simple bath, which is repeated every second day for a week. Complications are subsequently treated.

Juice of Meat.—Dr. Christison* writes in favour of the juice of meat prepared by Mr. Gillon of Leith. He finds it contains only six and a half per cent. of solids —beef tea containing in a pint scarcely a quarter of an ounce of solid matter. Gillon's meat-juice contains no fibrin, albumen, or gelatine, only osmazome and the salts and sapid principles of the meat. The value of beef tea in the treatment of protracted disease is familiar to all. Dr. Christison, considering how little really nutrient matter it contains, is disposed to think that it acts much as coffee appears to do, in hindering metamorphosis of tissue.

Lead Colic—Chloroform.—M. Aran,† after using chloroform in lead colic, both externally and internally, for four years, now repeats an opinion formerly expressed of its superiority over all other methods of treatment. At the same time he modifies some of his former statements. He regards the internal use of the medicine as the basis of the treatment, whilst he considers the application of it externally as only indispensable during the first days, and in the most severe cases. His observations have also taught him that it is impossible to lay down precisely the maximum dose, which must depend on the intensity of the pain, &c. It may be necessary to give as much as 100 or 300 drops (four to twelve grammes) in twenty-four hours, while 60 drops may suffice in slight cases. As the effects of chloroform rapidly pass away, the patient must be kept continually under its influence for a certain number of days by repeated small doses, given by the mouth or by enema. He applies the chloroform topically by dropping it on a fine and dry compress, to an amount varying with the degree of pain (e.g., two to four grammes), and after placing this upon the abdomen, it is covered with some dry compresses. It produces its effect in from one to five minutes. He gives the chloroform internally suspended in water by tragacanth. The lavement contains from 30 to 50 drops, similarly suspended. The topical application is rarely of any use beyond the second day. Reducing the dose, he continues the medicine in lavement, as a precaution, when the case has been severe, up to the eighth or twelfth day. He founds his recommendation on the results of 21 cases.

Mercurial Stomatitis—Chlorate of Potash.—M. Demarquay‡ relates 6 cases in which chlorate of potash succeeded rapidly in curing the stomatitis arising from the action of mercury. In one case, where the mercury had been used for a syphilitic affection, two grammes of this salt given for four days removed the stomatitis, and on resuming the mercurial treatment, and conjoining it with chlorate of potash, salivation did not reappear. The harmlessness of the salt is well known. M. Gustin took eight grammes, but did not find the urine increased; a sense of constriction in the mouth and some roughness of the gums were produced, and although the saliva was not sensibly diminished, yet it appeared to him more liquid than usual. He has established the fact, that the chlorate is in great part discharged unaltered by the urine. When the salivation has recently commenced, two-gramme doses suffice to check it; but when it is intense and fully established, the dose must be increased rapidly to five, ten, or fifteen grammes. He advises also the conjoined use of an astringent wash for the mouth.

Neuralgia.—A case is related which was under the care of M. van der Kiefft,§ in which neuralgia of the scrotum, which had resisted the internal use of tartar emetic, inhalation of chloroform, and the topical application of extract of belladonna and of chloroform, gave way under the internal use of chloroform in doses

* Monthly Journal of Medical Science, Jan. 1855.
† L'Union Médicale, Jan. 1855.
‡ Bull. Gén. de Thérap., May, 1855.
§ L'Union Méd., Feb. 1855.

of twenty drops every quarter of an hour. The pain lessened after the second dose, and disappeared after the third.

Ovarian Dropsy.—Mr. I. B. Brown* records an additional case, in which, after emptying the cyst, he injected five ounces of the tincture of iodine (Ph. Ed.) without producing pain, and apparently with the effect of arresting the progress of the disease.

Paraplegia—Phosphorus.—This substance, in the form of phosphuretted oil (gr. iv. of phosp. to ℥j. of olive oil), has been given by Dr. Hughes Bennett† in severe cases of paraplegia from diseases of the spinal cord, without improvement resulting in any one of them. The doses commenced with three drops a-day, and when he increased them to ten or fifteen drops, nausea and vomiting were induced. In a case of chronic myelitis, which took ten and fifteen drops, the breath smelt strongly of phosphorus, but was not phosphorescent at night. In another case, much phosphate was passed with the urine.

Pericarditis, with Effusion—Injection of Iodine.—A case is related by M. Aran‡ which demonstrates that the pericardium may be injected with iodine for the cure of effusions as well as other closed sacs. It is that of a young man, aged about twenty-three. The effusion resisted the application of the ordinary remedies, and unless he were relieved death was imminent. The respirations were 40, and the pulse 120—irregular and unequal. A puncture with a capillary trocar was made at the fifth interspace, two or three centimetres from the external limit of the dulness: 850 grammes of a transparent reddish serum were removed. The injection was then thrown in, consisting of fifty grammes of water, fifteen grammes of tincture of iodine, and one gramme of iodide of potassium. It produced no pain, and a few grammes were allowed to run out before the wound was closed. Re-accumulation having taken place, a second puncture and injection were had recourse to, the strength of the injected liquid being—water and tincture of iodine, of each fifty grammes; iodide of potassium, four grammes. Complete recovery resulted.

Photophobia—Tincture of Iodine.—M. van Holsbeck§ recommends as completely successful the external application of tincture of iodine, especially in that form which accompanies strumous ophthalmia and chronic granular conjunctivitis. He paints the orbicular and superciliary regions once or twice a-day, according to the duration and intensity of the symptom, and says that a single application usually suffices to remove the symptom in twenty-four hours.

Rheumatism—Veratrine.—Dr. Aliès‖ relates several cases of rheumatic affections in which he has rapidly effected a cure by the use of veratrine, in doses of five milligrammes every five or six hours.

Sciatica.—Dr. Blakiston¶ has treated eighty-three cases of the uncomplicated disease without a failure by the endermic application of morphia. A blister the size of a crown piece is used over the chief seat of pain, and a grain of acetate of morphia daily sprinkled on the denuded surface, which is prevented from healing, if necessary, by savine or cantharides cerate. Should the knee become painful, the same plan is adopted at the ham.

Skin Diseases.—Dr. Hughes Bennett** treats impetigo and eczema by assiduously keeping the parts moist with lint saturated with a solution of half a drachm of sub-carbonate of soda to one pint of water, covering this carefully with oil-silk, to prevent evaporation. Favus he treats with oil, so as to exclude the atmosphere

* Lancet, April 14, 1855. † Monthly Journal of Medical Science, Feb. 1855.
‡ L'Union Méd., Nov. 8, 1855. § Ibid., March, 1855. ‖ Ibid., Sept. 1855.
¶ Med. Times and Gaz., Jan. 1855. ** Monthly Journal of Med. Science, Jan. 1855.

and prevent the growth of the parasitic fungi, first removing the crusts by poulticing; an oil-skin cap must be constantly worn. He says that six weeks suffice for cure in young persons, if the treatment be conjoined with cod-liver oil, generous diet, and antiscrofulous remedies.

Small-pox—Tincture of Iodine.—M. Boinet adds his testimony* to the efficacy of tincture of iodine in causing the abortion of the variolous pustules. He adds a number of formulæ for its use also in vaginitis, blennorrhagia, inflammations and ulcerations of the mouth and throat, and in mercurial ptyalism, erysipelas, and ophthalmia. In all these instances he combines it with tannic acid in various proportions.

Small-pox.—Dr. Wallace† has used a solution of gutta percha in chloroform as a topical application to prevent pitting in twenty cases, four being confluent, seven semi-confluent, seven discrete and moderate, and two with the eruptions discrete but copious. In all it was painted on immediately before complete maturation. Scarcely any pitting was observed when the patients were dismissed, except in two or three instances, where it was moderate. Some months after discharge, however, such of the cases as he then had an opportunity of seeing presented the marks much more distinctly. He insists, that in estimating the comparative methods of preventing pitting, regard should be had to the appearances a long time after treatment as well as on dismissal.

Spermatorrhœa—Belladonna.—In those cases which are connected with chronic inflammation of the urethra, good results followed cauterisation of the prostatic portion of that canal, but M. Trousseau‡ believes that cases arise in which the disease is purely spasmodic, and resembles certain forms of incontinence of urine in children. For these cases he advises the use of belladonna internally, of friction of belladonna ointment upon the perinæum, and of belladonna suppositories. Where an erotic condition is conjoined, he thinks the best sedative is the application of bags of hot sand to the perinæum for a few minutes morning and evening. Lupulin, in doses of fifty centigrammes a-day, may be tried; or bromide of potassium, on which M. Trousseau places reliance, in doses of one or two grammes a-day.

Suppressed Secretion of Milk—Faradization.—A case is recorded by M. Aubert of Macon,§ where, in consequence of suspension of suckling from the infant being attacked with pneumonia, the secretion of milk had gradually subsided. Fifteen days after the child had been first attacked, suction having failed to restore the secretion, Faradization (the application of the induced electric current) of the breasts, by means of the apparatus of Dr. Duchenne, was commenced, the force of the current and vibrations being gradually increased, but not to the extent of producing pain or contraction of the pectoral muscles. After a few minutes, the right breast increased in size. The operation was repeated daily, and on the third occasion the suction of the infant subsequently induced a slight "draught." The child had drawn a little milk since the second application of the remedy. On the fifth day it is reported that "the draught" had been twice complete, and the lactation was considered re-established.

Tetanus—Chloroform Inhalations.—Dr. Sprengler relates a case of traumatic tetanus,‖ in which the free and frequent inhalation of chloroform, but short of complete anæsthesia, was had recourse to. The disease lasted from the 13th of July to the evening of the 17th, and the diaphragm and heart remained unaffected till the last hours of life. The author thinks that if by this means the course of tetanus can be rendered more chronic, the prospect of cure may be improved.

* Rev. Méd.-Chir., Feb. 1855. † Glasgow Med. Journal, 1855.
‡ Rev. Méd.-Chir., March, 1855 § L'Union Méd., Sept.
‖ Henle und Pfeufer's Zeitschrift, Band v. Heft 3.

Uva Ursi as Ecbolic.—Mr. Harris* has found a strong decoction of uva ursi, during labour, in cases where ergot is applicable, produce vigorous pains, which soon caused expulsion of the child and placenta. The contractions induced are less violent than those from ergot.

Yellow Fever—Turpentine.—Sixty cases on board H.M.S. *Medea* were treated with turpentine by Mr. Laird.† Of these only four died. The usual dose was fifteen minims every three or four hours, with a little nitric ether and camphor mixture, continued till a remission was apparent. He regards its action as eliminative by the skin and kidneys.

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

By Edward H. Sieveking, M.D.
FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS, ETC. ETC.

I. *On the Black Colour of the Tongue, apart from Febrile Conditions.* By M. Bertrand de St. Germain. (L'Union Médicale, Dec. 8, 1855.)

M. de St. Germain states that he has, in twelve years, four times met with a spot of an oval shape and intense black colour on the middle line of tongue, gradually spreading over the entire organ. The discoloration remained stationary for about ten days, and then gradually disappeared from the circumference to the centre, the changes resembling those of an ecchymosis undergoing resolution. The entire duration of the phenomenon was from forty to sixty days. The patients complained of no local symptoms, but those of dryness of the mouth. The four cases were: 1. A girl, aged thirteen, whose increasing emaciation and paraplegia denoted a serious lesion of the nervous centres. 2. An asthmatic lady, aged seventy, whose health was not more than usually impaired. 3. An old man, in fair health. And 4. A girl, aged eleven, convalescent of typhoid fever. The author regards it as an accidental formation of pigmentary matter, not connected with hæmorrhage. He does not advert to the question whether his patients were or were not taking steel medicines at the time.

II. *Acute Fibroid Cancer of Thyroid Gland. Death from Compression of the Right Vagus.* By E. Montard Martin, Physician to the Hôpital St. Antoine. (L'Union Médicale, Feb. 23, 1856.)

Baumgartner, aged thirty-six, a female servant, in good bodily health, but subject to some intellectual obliquity, left the service of her master, a medical man, on a visit to her native country, and returned after three months' absence. A few days after her return (10th of November) she found her neck enlarged about the middle, and the enlargement increased so rapidly, that when admitted into the Hôpital St. Antoine, it had already acquired the size of half a hen's egg. It lay in front of the larynx and trachea, more to the right than the left, reaching above to the upper border of the thyroid cartilage. The larynx was strongly pushed over to the left, so that when the head was held erect, it lay exactly under the angle of the lower jaw. There was no pain or tenderness, during rest respiration was not laboured, but became very noisy as soon as the head was raised, or the patient moved actively about. There was no dysphagia. The treatment consisted in the administration of iodide of potassium and calomel; but the tumour rapidly enlarged in every direction, causing increasing dyspnœa, loss of voice,

* Virginian Med. Journal. † Medical Times and Gazette, April, 1855.

dysphagia, and, on the thirteenth day after admission, death ensued, without any material change in the symptoms. The affection was regarded by Dr. Martin and his colleague, M. Richet, as an inflammatory affection of the thyroid. The autopsy showed the tumour to consist of two distinct lobes which laterally extended as far as the vertebral column, and from the central portion a shoot descended to 4 centimètres (1·5 English inch) below the upper border of the sternum. The larynx was completely carried over to the left of the neck, but the trachea was curved still further to the left, then returning to the sternum, assumed a vertical position. Nowhere did it show any contraction. The œsophagus followed the same direction as the trachea, being enveloped equally by the growth. The left vagus, common carotid, and jugular vein, were free, and lay at the back and side of the tumour. The right phrenic nerve passed over the tumour, as did the right internal jugular vein; but the right vagus, which was separated about half an inch from the vein, was involved in the tumour, hypertrophied, and reddened. The right carotid was equally imbedded in the growth. At the inferior part of the tumour, a large vein presented a perfectly white appearance, from containing a firm clot of a semi-transparent opaline character. The tumour, on being excised, resisted the scalpel, presented a mottled white surface; it was divided into compartments by fibrous tissue, and on scraping the cut surface, the characteristic juice of cancer was obtained. The microscopic appearances, which, however, are not detailed, presented by the tumour and the clot in the vein were identical, and convinced the author that he had to do with a fibrous cancer of the thyroid body. All the other organs were healthy.

The author observes that the case is an exceptional one, on account of the rapid growth of the fibrous form of cancer. He considers that death resulted from the compression of the right vagus, since the calibre of the trachea was not interfered with. The patient died of defective hæmatosis, in spite of the admission of air into the pulmonary vesicles. For the same reason, he congratulates himself upon not having performed tracheotomy, which could not have influenced the morbid condition of the right vagus.

III. *On the Treatment of Pneumonia and Pleurisy.* By Dr. Niemeyer, Professor of Clinical Medicine in Greifswalde. (Prager Vierteljahrsschrift, 1855. Band iv. p. 121.)

Professor Niemeyer is much opposed to the employment of general venesection in pneumonia and pleurisy, and only uses it exceptionally with a view to prevent impending suffocation, and to facilitate the reflux of the blood from the brain, but not for the purpose of arresting the inflammation. He agrees with the observation of Dieck, that the convalescence is more rapid in those cases that have been treated without than those which have been treated with venesection; and he explains the fact by the increase of fibrin, and diminution in the amount of red corpuscles, induced by the venesection.

The treatment adopted by Professor Niemeyer consists in the application of compresses wrung out in cold water over the affected part of the thorax, and their renewal as often as they become warm. The great relief experienced by the patient is a sufficient guarantee that the repetition of the application will be carefully attended to. The only internal remedy employed was nitre, in doses of two drachms in the course of twenty-four hours. Although employed at different ages, and in various forms of the disease, no metastasis or other evil consequences have ever been noticed by the author. He has seen persons attacked with very tumultuous symptoms, enabled by this treatment to return to their occupation on the seventh day after seizure. Professor Niemeyer recommends an early exhibition of steel in the convalescence from the diseases under consideration.

IV. *A Case proving the Advantage of Iodine Injections in Empyæma.* By M. Boinet. (L'Union Médicale, Dec. 22, 1855.)

In the April number of the 'Medico-Chirurgical Review' we gave a case proving the advantage of iodine injections into the pleura in the treatment of hydatids in the thoracic cavity. The present case, that of a lady, aged thirty-four, affected with pleurisy, occurred in the practice of M. Trousseau. Thoracentesis was performed in January, 1855, and two and a half litres (about five pints) of serum evacuated from the right pleura. The pleura refilled, and on the repetition of thoracentesis the fluid was found to have become purulent. It was now (in April, 1855) entirely withdrawn by M. Boinet, the pleural cavity washed out with warm water, and an injection of equal parts (fifty grammes of each) of water and tincture of iodine, with two grammes of iodide of potassium, thrown in. The injection was withdrawn after five minutes, and repeated at intervals of at first two or three days, and then at longer periods. The canula was left permanently in the thorax, the orifice being well closed after each operation. No bad symptom occurred; after a fortnight the patient was able to leave her bed, and a week after to walk about. In June she went about Paris, and in July she was well enough to undertake a journey. The right side of the thorax has sunk in, but the corresponding lung has in part recovered its functions.

V. *A Paper on the Effects of Lead on the Heart.* By T. W. Corson., M.D. (New York Journal of Medicine, March, 1856.)

In the course of a general inquiry into the relation of an altered impulse of the heart to various maladies, Dr. Corson has observed that house-painters and others, with either paralysis or great muscular debility from lead-poisoning, had uniformly a more or less weakened impulse of the heart, and generally on going upstairs complained of some faintness and cardiac distress. In the earlier stage, where lead colic only existed, it seemed as if the heart had as yet escaped, and these symptoms were absent. The practical advantage of a correct diagnosis of heart-symptoms depending upon such a cause, is that we can at once hold out great probability of a complete cure, by means of those agents which counteract the lead-poisoning and eliminate the metallic salts from the system. Dr. Corson gives the details of ten cases of well-marked lead-poisoning, all characterized by the Burtonian streak, in which, in addition to more or less paralysis of the extremities, much alarm was excited by the great feebleness of the heart, with palpitation, consequent fainting, weak and soft pulse. The uniform effect of the treatment employed for the metallic poisoning, by iodide of potassium and nux vomica, aided by occasional tonics, baths, laxatives, and electricity, was to relieve or remove the cardiac symptoms.

Dr. Corson observes that the debilitating effects of lead most commonly occur in hearts previously sound; but that they sometimes, as in two of his cases, complicate existing organic cardiac disease from rheumatism or other causes. He is also of opinion that while the agencies or causes of lead-poisoning are very numerous and often obscure, the slighter cases, supposed to be ordinary dyspepsia, constipation, or bilious colic, are frequently undetected. Dr. Corson regards the action of lead upon the heart as analogous to that of digitalis; oil of tobacco, upas antiar, woorara, tending specially to paralyse the organ. His treatment is that adopted by many practitioners, more especially since the appearance of M. Melsen's memoirs; and consists in the exhibition of iodide of potassium with a view to the elimination of the lead from the system, followed by strychnia or nux vomica, with a view to rouse the enfeebled action of the nervous system.

VI. *Elimination of Urea by the Gastric Mucous Membrane in a Case of Fatal Traumatic Hæmaturia.* By Dr. M'Dowel. (Dublin Hospital Gazette, March 15, 1856.)

A gentleman, aged thirty-eight, received, in December, 1855, a severe injury in the right lumbar region by falling down stairs, followed by uncontrollable hæmaturia. A few days later vomiting supervened, without symptoms of gastritis; the tongue was natural, and the epigastrium free from tenderness. "It was observed that during these attacks of vomiting the amount of blood discharged from the kidney seemed to be greater, the urinous smell being faintly perceptible, whilst at other times it was strongly marked from the presence of ammonia. Medicine seemed to have little influence in relieving these later attacks, each of which lasted for about two days, and seemed to subside of its own accord."

The erethism of the stomach increased, and continued until death. Everything was rejected, and medicine was wholly useless. The mental faculties were unimpaired to the last. Owing to this circumstance Dr. M'Dowel suspected that the mucous membrane of the stomach was vicariously secreting urea; and on taking the vomited matter to Dr. E. Davy his suspicions were confirmed, for this gentleman found the vomited matter to contain both urea and ammonia, the amount of nitrogen in both being equal to one-fifth part of a grain of urea in the fluid ounce. The renal secretion was alkaline, and contained 1·23 grains of urea to the fluid ounce. Dr. Davy makes the following remarks on the analysis:

"These results show that the quantity of nitrogen removed from the system by the kidneys is extremely small (if we except that contained in the blood occurring in the urine). They also show that the stomach assists the kidneys in removing nitrogen out of the system, which function those organs, in their present diseased state, are unable sufficiently to perform; and though the fluid vomited contains in the same bulk a much less amount of nitrogen than that in the urine, yet if the quantity of vomited fluid is much more copious, the stomach may, in the twenty-four hours, eliminate a greater amount of nitrogen from the system. The large quantity of albumen found would indicate that a considerable amount of blood was present in the urine, as it is most probable the albumen was furnished from the serum of the blood.

"The quantity of oily matter present in the urine was very remarkable; such matter is said to occur in the urine in diseases in which there is a very rapid loss of substance or force, which I believe is the case with your patient."

VII. *Fatty Degeneration of the Liver and other Organs considered as the Chief Cause of the High Rate of Mortality among the European Troops of the H. E. I. C. in Bengal.* By C. N. Macnamara, Esq., Assistant-Surgeon H. E. I. C. S. (Indian Annals of Medical Science, October, 1855, p. 169.)

The title of Mr. Macnamara's paper indicates in part his object, which he also expresses thus: "What I wish to prove is, that it is the mode of living of Europeans in India, and not the climate, which kills them." "Perhaps," he shortly afterwards adds, "one of the best proofs I could bring forward to show that such is the case, is the fact that, taking the whole strength of the officers and men of the First European Bengal Fusiliers during the last seven years, it will be found that the mortality among the former amounts to 11 per cent. for that time, and that the mortality among the latter amounts to nearly 80 per cent., thus proving what I have repeatedly been told by the officers of the corps,—that the whole of the fighting men in the regiment are changed about once in every ten years." The average duration of life of the privates of the same corps is only twenty-seven or twenty-eight years, which is at least seventeen less than that of the journeyman bakers of London, whose habits and mode of living are by no means enviable. In order to ascertain the causes of the remarkable mortality just adverted

to, the author institutes a comparison between the dietary of the various corps. In her Majesty's navy, each man is allowed from 31 to 35½ ounces of dry nutritious food, 26 being vegetable, the rest animal; in addition to sugar and cocoa. In the army, a man is allowed one pound of bread and three quarters of a pound of meat. The Company's allowance is to each man per diem, one pound of bread, one of beef, a quarter of a pound of rice, and half a pound of vegetables. This would make 44 ounces, in addition to which, Mr. Macnamara states that the men generally consume meat, vegetables, and rice, so as to make the day's allowance altogether amount to 76 ounces. The author argues that the Indian soldier cannot possibly require more, or as much food as the sailor, whose body is exposed to greater wear and tear, and who requires so much more carbonaceous food to keep up the due temperature. The author further reasons that as the hydrocarbons taken into the body in India cannot be all eliminated by the lungs, and they are not deposited in the form of subcutaneous fat, we may expect to find them clogging the liver, as the organ, physiologically, complementary to the lungs. That there is a great excess of oily matter in the circulation is shown, according to Mr. Macnamara's observation, by the microscope. In a former number, we had occasion to advert to Dr. Forbes Watson's observation of a fatty degeneration of the blood-globules occurring in certain diseased conditions in India; it is not improbable that, in other cases of fatty degeneration, the oily deposit will show itself in and external to the cell growth of the part. Mr. Macnamara does not supply us with copious statistics in proof of his position, but reports that in twenty-four post-mortems he only found one in which the liver did not present a state of greater or less fatty degeneration; the exceptional instance was accounted for by the fact that the man had been in the hospital for some time previous to his death, and that his diet had been much restricted.

As a secondary affection, the author finds fatty urine, resulting from the oily blood causing fatty degeneration of the renal epithelium and secretion of oil with the urine. In many instances, Mr. Macnamara has detected large quantities of cystine in the urine, which he regards as resulting from the decomposition of the urea and uric acid, owing to the excess of sulphur in the system, caused by the impeded excretion from the liver.

It is not surprising that, with so large a surplus of oily matter in the circulating fluid, the central organ of the circulation should also manifest considerable fatty degeneration. In all the post-mortems but one, in which the liver was healthy, there was a large accumulation of fat upon and among the muscular tissue, and in many, the sarcolemma was replaced by fat globules. A fatty degeneration of the large vessels, arterial and venous, was also strongly marked. In three of the twenty-four post-mortems, death was the result of rupture of vessels in that condition; in one, the vena cava was ruptured just at its junction with the auricle; in the other two cases, the aorta had given way, in one case, within, in the other, just outside, the pericardium. The walls of the vessels were found infiltrated with fat, so as to render them incapable of resisting the impulse of the blood.

The author concludes his interesting and valuable communication by dwelling on the necessity of counteracting the fearful mortality and its causes adverted to, by a corrected dietary. "Nearly half the men in the regiment," he observes, "are Irish or Scotch; most of them, before they left their native shores, had never tasted fresh meat; they had also, from the force of circumstances, been obliged to live a sober life." If such men, on joining the regiment at Dinapore, take to eating three pounds and upwards of solid food, under circumstances in which there is less necessity than before for so large an increase, they indeed come under the category of the glutton, and may be classed with those unfortunate birds who, when forced into similar circumstances, supply us with the *pâté de foie d'oie*. The commissariat is out of our department; but as we have shown the pathological view of the question, it is to be hoped that it will not be lost sight of by those whose special office it is to attend to the regimen and dietary of the troops in question.

VIII. *On Paralysis from Muscular Atrophy.* By M. CRUVEILHIER. (Archives Générales de Médecine, Jan. 1856.)

In this paper, Cruveilhier claims the priority of the observation of muscular paralysis* apart from any lesion of the nervous centres. He gives a brief account of the first cases which came under his notice, in 1832 and 1848, and describes the surprise he felt when after, in the first instance, having diagnosed disease of the spinal cord, he found no trace of any affection of that part. He details two later cases, which satisfied him that the anterior roots of the spinal nerves exhibited a degree of atrophy closely corresponding to the amount of muscular degeneration upon which the paralysis depends. We must content ourselves with directing our readers' attention to these cases,† and giving an abridged summary of the author's views on the subject. They are as follows:

1. There is a species of paralysis, partial or general, which gradually affects the voluntary muscles, without involving the general or special sensibility, the intellectual or emotional powers, or any function of nutrition, except that bearing upon the muscles.

2. This muscular paralysis is the result of the progressive atrophy of the anterior roots of the spinal nerves, together with the progressive atrophy of the muscles; the posterior roots of the nerves, the spinal cord, and the encephalon remaining sound.

3. This form of paralysis is analogous to that resulting from section of a nerve.

4. This form of paralysis fully confirms the doctrine of Sir C. Bell, relative to the functions of the anterior and posterior roots of the nerves.

5. These observations establish the fact, previously not suspected, that the anterior roots of the nerves exercise a definite influence over nutrition.

6. These observations establish an independence of the anterior spinal roots, from the antero-lateral tracts of the cord, in which not the slightest disorganization was traceable. From this the author concludes,

7. That the anterior roots of the spinal nerves do not spring from the anterior-lateral tracts of the cord, but necessarily from the central grey matter.

Those interested in the whole history of symptomatology and therapeutics of progressive muscular atrophy, we would refer to a very complete memoir on the subject in all its bearings, by Dr. Adolph Wachsmuth.‡ He does full justice both to Cruveilhier and Dr. Meryon's claims, but points out that the 'Medical Gazette' of 1831§ contains an article by Dr. Darwall, in which that writer describes several instances of muscular atrophy with paralysis of the upper extremities, but attributes them to previous disease of the peripheral nerves. Dr. Wachsmuth has collected altogether sixty cases of the affection.

IX. *Communications from the Department for Spasmodic Affections in the Charité.* By Dr. LUDWIG MEYER. (Annalen des Charité Krankenhauses. Sechster Jahrgang, Heft 2, p. 1.)

In spite of the careful investigations to which the various forms of nervous affections have been submitted, and the rigid classifications laid down in standard works, the diagnosis between them is by no means always easy, and the transition from one to the other often imperceptible. Thus, hysteria has been commonly regarded as essentially dependent upon derangement of the female sexual system, and as essentially distinct from epilepsy; but, although the difference in well-

* It is but just to Dr. Edward Meryon (who published some interesting cases of granular and fatty degeneration of the voluntary muscles in the Medico-Chirurgical Transactions for 1852, p. 73) to state, that he was the first to observe the disease in England, and apparently without any knowledge of Cruveilhier's discovery.

† See, also, British and Foreign Medico-Chirurgical Review, Oct. 1855, p. 532.

‡ Henle und Pfeufer's Zeitschrift, Band vii. Hefte 1 & 2, pp. 1—128.

§ Cases of a Peculiar Species of Paralysis. By John Darwall, M.D.: Medical Gazette, vol. vii. p. 201.

marked symptoms is sufficient to justify a distinct classification, we see hysteria pass into epilepsy, and we frequently meet with undefined seizures of a nervous character, in which it is difficult, if not impossible, to determine the class they belong to. Brachet* has repeatedly met with cases of a transition of hysteria into epilepsy; and Thomas Willis already mentions similar instances.† Dr. Meyer gives one of two cases which he observed, in which symptoms of an hysterical character were converted into epileptic paroxysms. No observant practitioner can fail to have seen cases in which symptoms long unappreciated, or not properly regarded, were after the outbreak of an epileptic paroxysm at once interpreted as its precursor, or as what the French call the *petit mal.* The author also adverts to the question of the relation of insanity to spasmodic affections, and more particularly to the predominant influence of moral or emotional causes in the production of epilepsy.

Dr. Meyer's observations with regard to the efficacy of remedial agents are very unsatisfactory; he says, that although all the lauded specifics against epilepsy have been largely tried in the Charité, he has only met with one case in which there was definite proof of medicinal benefit. In this case it was produced by large doses of disulphate of quina.

X. *Observations on the Temperature of the Body in Intermittent Fever.* By Dr. S. A. Michael. (Archiv für Physiologische Heilkunde von Vierordt. Jahrg., 1856. Heft i. p. 39.)

This paper contains two series of observations on the temperature of the surface in persons suffering under intermittent fever. The first contains eleven cases of intermittents of different types, in which the observations were taken every hour, or at least frequently, in the course of the day; the second comprises three cases, in which the observations were made during the paroxysms themselves, and generally every five minutes. The state of the pulse and the respiration were generally noted at the same time. The number of thermometric observations amounted to about 260. They were taken by placing the thermometer in the armpit. The following are the general conclusions arrived at by Dr. Michael:—

1. An increase of temperature from the normal state or the lowest apyretic condition, at first slow, shortly before or at the commencement of the rigor, rapidly and continuously advances, and then attains its maximum by successive intermittent advances.

2. The temperature remains at its maximum height for a period never exceeding two hours, but generally much less.

3. The diminution always takes place less rapidly than the elevation. It is affected in a graduated manner, each depression of the temperature being followed by an arrest.

4. The sensations of the patient are not in the ratio of the changes of temperature. The temperature is above that of the normal condition, both at the commencement of the rigor and at the termination of the sweating stage. The maximum temperatures occur either during the hot stage, towards the termination of the cold, or at the commencement of the sweating stage. These remarks apply to the various forms of intermittent fever.

5. In most of the cases, the maximum lay between 32° and 33° R (104°—106¼° F.) The highest maximum was 33½° R.

6. The duration of the paroxysms varies considerably in the cases presenting a tertian type. The limits are sixteen and thirty-two hours; in the quotidian forms they are nine and eighteen hours.

* Traité de l'Hystérie. Par T. L. Brachet. Ouvr. Cour. p. 376. 1847.
† Pathol. Cereb. et Nervor. Gener. Specimen. 1667.

7. The duration of the period of increase is always shorter than the period of decrease in the quotidian forms; in the tertian it is sometimes shorter, sometimes longer.

8. During the free intervals, the temperature generally falls below the normal temperature, still, the instances—especially of the quotidian fevers—are not rare in which it is at least several degrees (Réaumur) above the normal temperature.

9. After the exhibition of sulphate of chinidine (the salt commonly employed in Dr. Michael's cases) in doses of from ten to fifteen grains, there is either no recurrence of an increase of temperature, or a single increase of almost the same intensity, but with less violent subjective symptoms; or again, the temperature rises, though to a lower degree, and the symptoms are scarcely perceptible; or, finally, there is a feeble increase of temperature without any subjective symptoms. Only one case occurred in which there were two increases of temperature.

10. During convalescence, the temperature is generally under the normal elevation, but may occasionally rise a few tenths of a degree above it. At times there are evening exacerbations or evening remissions, or it is the same morning and evening.

XI. *Idiopathic Gangrene of the Four Extremities, resembling Gangrenous Ergotism.* By Bernard Henry, M.D. (Hollingworth's Medical Examiner, March, 1856.)

Our readers may remember the details of an interesting case of gangrene attributed by the author to ergotism, which was published in this Review by Mr. Camps.* The instance given by Dr. Henry closely resembles the former case, but was more severe, and did not in any way appear connected with ergotic poisoning. It occurred in a sempstress, aged forty-two, a widow, but of abandoned habits, who was admitted into the Philadelphia Hospital on November 22nd, 1855. A persistent attack of diarrhœa during the preceding summer, with habits of intemperance and syphilitic affections, had reduced her much. A fortnight before admission, she first felt a stinging sensation in her hands and feet, which became more painful by scratching, assumed a dusky red colour, which increased in lividity up to the period of her admission. She was at first supposed to be labouring under purpura. Dr. Henry says that—

"When she first came under his care her countenance was icterode, with an anxious expression, the conjunctivæ yellow, eyelids puffy, the intellect remarkably clear, the hands and fore-arms, for about a third of their length, of a leaden hue, deepening off to the fingers; these were flexed on the hand, black in colour, and dry and shrivelled in appearance. The feet and lower third of the legs were in a similar state. The tip of the nose and the skin over both patellæ were of a dusky colour, as though brushed over with bronze paint. The tongue was not much coated, but was marked with two longitudinal reddish-brown stains. The pulse was eighty, quick and small.

"The affected extremities were icy cold to the touch, and sensibility was so destroyed that the prick of a pin inserted in them was not felt. Sensibility above the line of discoloration was acute. Movement gave much pain; the weight and warmth of the bed clothes could not be borne; the cold air was more agreeable. The cartilages of the ears showed a commencing similar condition. The bowels at this time were constipated, and the urinary secretion small in quantity.

"She was ordered milkpunch, opium, and nutritious diet. The legs were enveloped in cotton wadding; this was afterwards removed at her own request.

"November 24th. The discoloration of the extremities has extended up an inch higher; no line of demarcation is perceptible, the livid hue shading off into the normal colour. The pulse remains small, and the urine scanty. Ol. terebinth. gtts. x. every fourth hour were added to the treatment.

* See British and Foreign Medico-Chirurgical Review, July, 1855, p. 196.

"November 26th. Vesications, filled with a dark-red serous fluid, made their appearance at the edges of the discoloured parts. A specimen of urine was obtained, passed before breakfast. It was high-coloured, of a reddish tinge, sp. grav. 1010, reaction alkaline, and exhibits mucus and purpurine.

"The case progressed without much alteration in the general symptoms. The lines of demarcation between the sound and affected parts by degrees became more distinct."

The line of demarcation gradually became sufficiently distinct on the arms to justify their removal. On the 26th December—

"The right hand was removed by sawing through the exposed bones. The granulations were dissected up, to make as fair a stump as possible under the circumstances. No vessels were taken up, but the cut extremities of the bone bled freely. She experienced very little pain, and no inconvenience, from the operation."

Two days after, the left arm was taken off, under similar circumstances.

"Very little change took place during the following week; the stumps showed a disposition to heal well; and the line of separation between the sound and gangrenous parts of the lower extremities was so marked as to justify their removal by amputation, did her strength permit. Her appetite, however, continued bad; she rejected nearly all food, and sleep was procured only by means of large doses of opium.

"Tonics and stimulants were administered whenever they could be retained."

But she sank gradually, and expired comatose on the 14th January.

"*Autopsy thirty hours after death.*—Present, Drs. West, Hunt, Kenderdine, Hall, and Henry. Emaciation not very great. On opening the thorax and abdomen, the viscera were found remarkably dry—scarcely any moisture; very little blood in cutting across the large arterial trunks. The whole venous system appeared engorged with black, thick blood. The lungs were perfectly healthy. Adhesions of the right pleura. On opening the pericardium, no fluid was found. The heart was rather small, the coronary veins engorged, as was the whole venous system. The tissue, also, of this organ was more soft than natural, with a tendency to fatty degeneration, and slight fatty deposits in the valves. The pulmonary artery and valves were natural in their structure, but contained a venous clot. The auriculo-ventricular opening was contracted, so as with difficulty to admit the finger. The valves of the aorta were normal; a coagulum was found in the descending aorta. The brachial and femoral arteries were dissected up and examined; they presented no unnatural appearance, but were found adherent to the bone, and closed at the line of demarcation.

"On opening the abdomen, the liver presented itself fatty and very much enlarged. There appeared to be commencing cirrhosis; there was resistance to the knife on cutting through the lobuli of that viscus. The other organs presented nothing remarkable."

Dr. Henry is of opinion that, in this case, there was no ground for suspecting ergotism, as the patient had always had abundant and excellent food, and as the harvest of the past year had been remarkably good, and sufficiently plentiful to place good food in the reach of the poorest classes. He attributes the gangrene to a diseased crasis of the blood, unconnected with any morbid condition of the arterial coats, and dependent upon the dissolute habits of the patient, which had impaired her constitution. It is not, however, to be overlooked that she is said to have eaten rye bread shortly before her attack, though "the amount is described as small; nor was there any evidence of its being of bad quality; and no other individuals of the family in which she resided, or living in her neighbourhood, were similarly affected."

XII. *Analysis of the Cadaveric Inspections made in the Pathologico-Anatomical Institution at Prague from May* 1, 1854, *to March* 30, 1855. By Dr. Arthur Willigk. (Prager Vierteljahrsschrift für praktische Heilkunde, xiii. Jahrgang, 1856. p. 1.)

This analysis is based upon the cadaveric examination of 1146 subjects (out of a total of 1806 deaths); of which 516, or 45·0 per cent., were males, and 630, or 54·9 per cent., females; but it also includes the results obtained by the total cadaveric inspections made by the author in previous years, amounting in all to nearly 5000 cases. We are unable to go through the different items seriatim or in detail, but shall select one or two which may appear to present points of special interest to our readers.

One quarter of all deaths (452) were due to tuberculosis; 254 were males, and 198 females. Among the former the greatest mortality occurred between the ages of thirty and forty; among the latter the greatest mortality was between twenty and thirty. The mortality from this cause was 9 2 per cent. in March, April, and May respectively; 6·3 per cent. for each of the three succeeding months; 5·9 per cent. for each of the autumn months; and 6·5 per cent. for the winter months, December, January, and February. The mortality from tuberculosis was 2 per cent. less than in the preceding four years, during which the author met with 1317 cases of tubercular disease in 4547 post-mortems, or 28 9 per cent. The per-centage of tubercular disease was found to be much more favourable to females than to males. Dr. Willigk found that in a total of 2114 males examined after death, 35·6 per cent. were tubercular; while of 2433 female subjects inspected, only 23·2 per cent. were tubercular. This closely corresponds with the per-centage established by Dr. Boyd, in his report of the pauper lunatics at the Marylebone Asylum—viz., 36 per cent. males, and only 21 per cent. females. A curious fact, elicited by Dr. Willigk's analysis, is that the number of male victims to tuberculosis varies less at different periods than that of females; among the latter, fluctuations to the extent of 5 per cent. were observed at Prague, whereas the greatest variation among men was 1·4 per cent. Thus, in 1852 and 1853, when above 26 per cent. of all females brought to the deadhouse had succumbed to puerperal fever, 29 per cent. (after subtracting the puerperal cases) were found to be tubercular; whereas in 1854, when puerperal fever presented a much milder form, only 24·6 per cent. were found to have died of tuberculosis.

The following observations, bearing on the cure of pulmonary tuberculosis, deserve attention. In the year 1854–55, Dr. Willigk met with 81 cases (in 302) in which tubercular disease had undergone a curative process in the form of obsolescence, or chalky conversion; 40 occurred in male, 41 in female subjects. Taking the whole of his 1255 cases of pulmonary tuberculosis, Dr. Willigk met with this conversion in 309 cases, or 24·6 per cent. The largest number of cures were found during the epoch at which the largest number of deaths occur—viz., between thirty and forty years. This process is more frequent among females than males.

The post-mortem examinations of cholera subjects present no feature of special interest. From the author's remarks on the puerperal cases, we extract the observation that 43·1 per cent. exhibited the puerperal osteophyte of Rokitansky.

The number of cases of cancer amounted in five years to 477, or 10·5 per cent. Among the males, the number of deaths from cancer progressively increased from the tenth year to the eightieth at the rate of from 2 to 6 per cent. for every decennium; whereas the females presented the greatest mortality from this cause between fifty and sixty, after which the numbers rapidly decline. If cancer of the sexual organs be eliminated, the preponderance is actually on the side of the male sex by 2 per cent., which depends upon the predominance among them of cancer of the stomach and bones. Dr. Willigk has met with four cases of bone-cancer, in which the morbid product had been spontaneously separated, leaving clean and partially-healed ulcers. He has met with one case of scirrhus recti, in which a spontaneous cure was demonstrable.

From the analysis of the apoplectic cases, of which 208 occurred in five years, we extract the remark bearing upon the frequency of cure. Dr. Willigk found cicatrices (43·3 per cent.) or cysts (56·7 per cent.) in 97 instances. This favourable relation loses its value in part by the circumstance that the majority of the individuals, soon after the cure of the local process in the brain, died of diseases of the respiratory or digestive organs, which, from their frequent coincidence with apoplexy, may well be designated as sequelæ of that affection.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E., London.

I. *On the Treatment of Nævus by Vaccination.* By M. Legendre. (Archives Générales. May, 1856, p. 513.)

M. Legendre believes that the ill success which has attended this practice in the hands of some, is chiefly due to the defective mode adopted. He sums up his observations as follows:—

1. *Choice of the Lymph.*—It is of the greatest importance that all the vaccinated spots should take, as it is from their multiplicity and confluence the inflammatory process results sufficing to transform the erectile tissue into non-vascular cicatricial tissue. When but two or three from among seven or eight punctures succeed, the number is usually insufficient for the production of the requisite amount of inflammation, while it prevents the repetition of the operation. The lymph should be therefore taken directly from the arm of the child that supplies it, the lancet being charged afresh after each puncture, and the operation performed slowly, so as to involve only the superficial lymphatic net-work of the skin.

2. *Number of Punctures.*—There is nothing fixed with regard to this, depending as it does upon the size of the nævus; and while one nævus may require seven or eight insertions, double this number may be necessary for a very extensive one. It may be laid down as a general rule, that a sufficient number of punctures must be made to admit of the edges of the pustules, after their complete development, running into each other. M. Pigeaux states that this end will be attained by making the punctures at the distance of a centimètre from each other.

3. *Place of Vaccination.*—Most authors direct the inoculations to be made in the erectile tumour itself, and not at its circumference; but this practice not infrequently gives rise to hæmorrhage, which alarms the friends. It is generally very difficult to make several punctures in the excessively thinned skin of an erectile tumour, without piercing the erectile tissue, especially as it is impossible so to control the movements of the child, as to be certain that the lancet will not penetrate farther than we desire. Notwithstanding this inconvenience, direct inoculation must be resorted to whenever the nævus is situated on the face; for if we vaccinated around its circumference, the ensuing cicatrix would be larger than the tumour itself. When the nævus is out of sight we need not mind this, and by vaccinating near to, without implicating the erectile tissue, we avoid all danger of hæmorrhage, while we can produce a circle of pustules that entirely surrounds the tumour. The erectile tissue more and more invaded by the increasing pustules, diminishes in size, inflames, and becomes connected together with the pustules into a large, dry, blackish crust. When this falls off, the place of the nævus is found to be occupied by a smooth cicatrix, which is either quite white or scattered over with a few red isolated spots, the size of a small pin's head, and devoid of elevation, the further development of which is prevented by the surrounding cicatricial tissue.

M. Legendre points out the desirableness, before vaccinating infants, of inquiring whether any erectile tumour exists, in order that the opportunity of so treating it may not be passed over.

II. *On Urinary Calculi and Lithotomy in Egypt.* By Professor REYER, at Cairo. (Wien Mediz. Wochensch., 1856, Nos. 14—17.)

Professor Reyer states that calculous diseases are of such frequent occurrence in Egypt, that practitioners awhile since were accustomed to reckon their lithotomy operations by hundreds. Although competition has now divided practice, he has still in five years had to perform lithotrity nine, and lithotomy fifty-six, times. He has collected one hundred and twenty-eight calculi, of all of which a careful chemical examination has been made. This prevalence of the disease has been attributed by some to the great concentration of the urine that ensues on profuse sweating, erroneously, for the disease is rarely met with in Upper Egypt, and not at all among the negro race. The urine of the Arabs, notwithstanding the paucity of their flesh diet, is very rich in nitrogenous materials; and uric acid, urate of ammonia, and oxalate of lime are often found in it, prevailing in the same ratio as catarrh of the bladder, which is endemic, and usually dependent, as shown by Dr. Bilharz, upon the presence of the *distomum hæmatobium* in the tissues of the bladder.

The *composition* of the 128 calculi is stated to be as follows:—The nuclei in 69 consisted of uric acid, in 2 of urate of ammonia, in 15 of oxalate of lime, in 1 of ammonio-magnesian phosphate, in 4 of uric acid with urate of ammonia, in 22 of uric acid with oxalate of lime, in 8 of urate of ammonia with the phosphates, in 8 of urate of ammonia with oxalate of lime, in 2 of these two last and uric acid, in 1 of oxalate of lime and phosphates, and in 1 of the ova of the *distomum*. The considerable *predominance* of uric acid (combined usually with more or less oxalate of lime) was observed in 63 out of 110 calculi; the predominance of urate of ammonia was observed in 9, of oxalate of lime in 24, and of the phosphates in 10. Uric acid, in more or less considerable proportions, was found in 102 calculi; and oxalate of lime (often in slight proportion) in 107. In only 8 out of the whole collection did the calculus consist of but a single ingredient; viz., in 4 of uric acid, and in 4 of oxalate of lime. In 42 there were two constituents, in 37 three, and in 41 four. No example of only phosphatic formation was known at Cairo, and in one instance only was the nucleus so composed. Among 25 calculi collected by Dr. Schledehaus in Alexandria, several consisted of phosphates only—a difference which the author attributes to the greater frequency of inflammatory irritation of the bladder in the wet and changeable climate of Alexandria. In the author's cases more than one stone was met with in 13 instances, or 16 per cent. Seven in his collection weighed above five ounces, the heaviest being seven and a half and six and three-quarters. The great bulk of the cases occurred between the ages of twenty and forty.

Professor Reyer performed lithotomy fifty-six times, and lost nine cases, the operation almost universally resorted to at Cairo being the *median*, as practised by Vacca. A sound, having a very broad groove, is introduced, and held perpendicularly by an assistant. The operator feels for the groove through the soft parts by means of the nail of his forefinger, which has been allowed to grow, and having found it, he thrusts the point of a slightly convex scalpel directly into the groove, at about an inch and a half above the anus. He passes the point for about half an inch along the groove towards the bladder, and then, depressing the handle, brings the blade out in the mesial line so as to leave a wound of more than an inch long in the perinæum. The scalpel is laid aside, and the left forefinger is passed into the wound, having the free surface of the nail looking towards the perinæum, and serving to guide into the groove of the sound the button end of a long lithotomy knife, which is trenchant along its anterior third. The finger is now withdrawn, and the operator, taking the sound in his left hand, depresses it somewhat as if he desired to pass it deeper into the bladder, raising the handle somewhat towards himself in order to bring the urethra nearer the arch of the pubis. At the same time he passes the button end of the knife along the groove into the bladder. The left finger is now again introduced into the bladder, the

sound withdrawn, and the forceps introduced along the volar surface of the finger.

Reviewing the different stages of this operation, the opening into the urethra so high up, anterior to the bulb, first calls for notice—the Arab practitioners choosing a point still nearer the scrotum, owing to the greater ease with which the groove can there be felt. The division of the bulb would be feared in Europe, on account of hæmorrhage; but in the author's fifty cases operated upon by this median plan there was little bleeding, while secondary hæmorrhage, easily arrested, only occurred in three cases. Hæmorrhage after any operation is rarely met with among the Egyptians, and the author has never seen it occur after amputation. Professor Schuh, performing this median operation in three cases at Vienna, met with alarming hæmorrhage in two of them. In children and fat persons dissection of the soft parts of the perinæum has to precede opening the urethra, as the piercing it through all these would be uncertain.

In the second stage of the operation the extent of the incision of the prostate is of chief importance, as this, except in the case of very large stones, should be small—one or two lines sufficing for a stone weighing three or four ounces. A too considerable incision might extend through the fibrous covering of the gland, and expose the patient to the greatest of all dangers, that of urinary or purulent infiltration, while the rectum would be more liable to be injured. The cases which have best succeeded in the author's hands have been those in which, in stones of a medium size, the prostate has scarcely been divided. In the author's fifty operations the rectum was wounded in three accidentally, and in one intentionally, on account of the size of the stone.

In the third stage the author lays great stress upon the importance of the information derivable by the introduction of the finger prior to that of the forceps, in regard to the size, form, and position of the stone. To acquire this, when the neck of the bladder is placed very high, considerable pressure has to be exerted over the pubis. While guiding in the forceps, care should be taken not to direct the volar surface of the finger downwards, so as to bring the forceps between this surface and the anterior wall of the rectum, for in that way the cellular tissue between the prostate and rectum may be easily torn. The difficulty in extracting the stone chiefly depends upon the amount of resistance offered by the prostate, this yielding sufficiently for small stones. In larger stones its tissue is torn in one or more places; but as these ruptures are limited by its extensible covering, infiltration does not occur. When the prostate is hard and thick, much force is required even for the extraction of medium-sized stones, while even a large stone can be removed easily when the gland is thin and soft. The author removed a six-ounce calculus with remarkable ease, the prostate having diminished in size. Breaking of the stone during extraction occurred in seven out of the author's fifty-six cases, an accident that may happen in spite of every care on the part of the operator, owing to the loose texture of the calculus. Four of these patients died, and three recovered; and he strongly disapproves of the practice, except when quite unavoidable, of breaking up large calculi in the bladder, and removing them in fragments through the wound. In a case of very large stone, the author preferred inducing a recto-vesical fistula, in order to obtain room for the extraction.

III. *On a New Mode of Reducing Strangulated Hernia.* By Baron Seutin. (Bull. de Thérap., tome l. pp. 161 & 206.)

Baron Seutin declares, that with his mode of reducing strangulated hernia, which he has now practised for twenty years, he hardly ever in his large practice finds it necessary to have recourse to an operation.

The patient is laid upon his back, with the pelvis raised much higher than the shoulders, in order that the intestinal mass may exert traction upon the herniated portion. The knees are flexed, and the body is slightly turned to the opposite

side to that on which the hernia exists. The surgeon ascertains that the hernia, habitually reducible, cannot be returned by continuous and moderate taxis. He next seeks with his index finger for the aperture that has given issue to the hernia, pushing up the skin sufficiently from below, in order not to be arrested by its resistance. The extremity of the finger is passed slowly in between the viscera and the herniary orifice, depressing the intestine or omentum with the pulp of the finger. This stage of the procedure demands perseverance, for at first it seems impossible to succeed. The finger is next to be curved as a hook, and sufficient traction exerted on the ring to rupture some of the fibres, giving rise to a cracking very sensible to the finger, and sometimes to the ear. When this characteristic crack is not produced, the fibres must be submitted to a continuous forced extension, which, by distending them beyond the agency of their natural elasticity, generally terminates the strangulation. This mode of procedure is more applicable to Gimbernat's ligament, the hooking and tearing of which are more difficult than in the case of the inguinal ring. Considerable strength has sometimes to be exerted, and the index finger becomes much fatigued. When, in consequence of the narrowness of the ring, the finger does not at once penetrate, it is to be pressed firmly against the fibrous edge, and inclined toward the hernia. After a time the fibres yield and the finger passes. When the finger becomes fatigued it is not to be withdrawn, but it should be supported by the fingers of an intelligent assistant, who seconds the action it is desired to produce. In inguinal hernia, the traction should not be exerted with the finger upon Poupart's ligament, but in a direction from within outwards, and from below upwards, by which the aponeurotic layers between the two ligamentous pillars constituting the inguinal aperture are easily torn through.

The ring is then enlarged by this tearing, just as if it had been divided by a cutting instrument, or largely dilated, and reduction takes place easily, by performing the taxis in a suitable direction. The mobility of the skin, its laxity in parts where hernia prevails, and its extensibility, greater in proportion to its thinness and to the absence of a lining of fatty cellular tissue—by allowing the sliding and the thrusting of this membrane in front of the finger it cushions, affords protection to the intestine from all immediate contusion. When the strangulation is induced by the issue of a considerable mass of intestine, or an accumulation of fæcal matters, it is desirable first to disengage one of the extremities of the noose, and to seek to expel the gas or fæcal matters by moderate pressure, in order to facilitate the reduction of the tumour. In the few cases in which the finger cannot be introduced, a small incision may be practised in the skin, and the handle of a spatula or any blunt instrument may be passed in by separating the cellular tissue. Pressing this against the border of the ring, while avoiding the intestine, this orifice may be eroded or dilated without danger. The greater the resistance offered by the aponeurotic fibres, the greater will be their tension, and the more easily will their laceration be produced.

As a general conclusion, it may be laid down, that the facility and promptitude of this procedure, and the immunity that attends it, ought to diminish the gravity of the prognosis of strangulated hernia, by rendering the circumstances under which recourse need be had to an operation quite exceptional. Such exceptional cases will be found (1) in old, irreducible herniæ. (2.) When the strangulation in inguinal hernia occurs at the internal ring. Generally the external ring and inguinal canal are large, and allow of the easy penetration of the finger; and then the new method is applicable, and the rupturing or dilatation of the internal ring should be attempted, and the manœuvre is rendered the easier by the fact, that in these cases the canal is much shortened, and the two rings much approximated. If, however, the external ring is too narrow to admit the finger, an operation is required. (3.) When there are general symptoms of a gangrenous state of the intestine.

IV. *Perchloride of Iron in Panniform Keratitis.* By M. FOLLIN. (Archives Générales, April, 1856, p. 424.)

M. Follin, after adverting to the obstinacy of chronic keratitis, and to the frequent inefficacy of the various means proposed for its relief, states that he has found in the employment of the powerful astringent, the perchloride of iron, a most useful application, and that he has had his views of its utility confirmed by MM. Broca and Gosselin. He does not recommend its employment in a high degree of causticity, and believes that at 30° Baumé it is best suited for this purpose. He lets fall a large drop into the eye, by means of a quill, every second or third day, the great contraction of the eyelids that ensues rendering it necessary that all should be introduced at once. It imparts a yellow colour to the eye, and gives rise to a sense of painful constriction, which lasts for about a quarter of an hour, and then gradually diminishes, a burning sensation still continuing for some hours. A slight phlegmasia is sometimes induced the next day in the eye; but however that may be, the perchloride must be abstained from, and cold and slightly astringent applications—among which, the decoction of rhatany is a good one—must alone be resorted to. The perchloride is to be used again on the second, third, or fourth day after, according to the amount of irritation remaining from the former application; and generally it is after the second or third application that decided amelioration is perceived. The perchloride has never given rise to any accidents; and when its application could not be borne, it has not aggravated the condition of the eye. Several cases are related.

V. *Perchloride of Iron as a Hæmostatic.*

A correspondent of the 'Moniteur des Hôpitaux' (1856, No. 24) states that one of the principal elements of his success in the difficult and dangerous operations M. Maisonneuve is famous for undertaking, is the remarkable use he makes of hæmostatics during their performance. He cites a recent case, occurring in a lad of sixteen, of fungous tumour of the dura mater, the growth of which, after having been temporarily arrested by ligature of the carotid, took on enormous proportions, and was accompanied by exhausting hæmorrhages. M. Maisonneuve determined upon its removal, but the tumour bled on the slightest contact, and the patient would not be able to bear the slightest loss of blood. The line of incision extended from the anterior parts of the ear to the summit of the head, and descending along the nose, was carried backwards, and then upwards to the base of the jaw, and its point of departure. A great number of arteries were thus divided, five or six of which, by reason of their anastomatic enlargements, had acquired almost the size of the radial artery. Intelligent assistants immediately compressed them with the finger, but it was impossible to thus continue the dissection without exposing the patient to the danger of death from syncope. M. Maisonneuve therefore applied to each vessel a little pledget of charpie soaked in perchloride of iron, which was allowed to attach itself to the wound. At every stroke of the bistoury or scissors he applied a new plug, so that during the operation the patient scarcely lost a spoonful of blood; and when the tumour had been entirely removed, the entire surface of the wound was found completely dried and tanned, and was at once dressed, without the necessity of the application of a single ligature. The brown eschar which covered the wound was detached about the 20th day, without giving rise to any hæmorrhage; and although the cure can scarcely be expected to prove radical, the patient for the present is perfectly well.

VI. *On Gangrene from Arteritis.* By Professor Porta. (Omodei Annali di Med. Feb. 1856, p. 416.)

The following are some of the conclusions arrived at by Professor Porta, from the observation of thirty-one cases of his own, and the consideration of those published by others:

Although the tunics of arteries consist of tissues little disposed to inflammation, yet are they not exempt from liability to it; and external violence, the extension of phlegmasia from other tissues, rheumatism or metastasis may induce an arteritis that may lead to gangrene of subjacent parts. Among all these causes, metastasis is pre-eminent, so that eighteen out of the thirty-one cases are referrible to it. Not infrequently, on the decline or disappearance of some serious internal malady, a reverberation is directed to the arteries of the limbs, the original disease either then disappearing, or remaining as a complication of the newly-developed arteritis. The large external arteries, such as the axillary, humeral, femoral, or popliteal, are usually the subjects of such reverberation, but it has not as yet been met with in the carotid. Exceptionally, smaller arteries are attacked, such as the radial, ulnar, or tibial.

The end to which arteritis tends is the closure of the artery, all the manifestations observed subsequent to the cessation of its pulsation being but the sequelæ of that. Strictly speaking, however, such cessation of pulsation is not pathognomic of obliteration, as sometimes a minute stream continues to pass, which excites so feeble an oscillation of the vessel as not to be perceptible to the touch. The obstruction of the artery does not necessarily give rise to gangrene, for not only may it be incomplete, but even when complete, it may have been formed with sufficient slowness to allow the development of the lateral anastomoses; the amount of the obliteration, indeed, exerting less influence than the rapidity with which the coagulum is formed. This local condition is not the sole cause of the gangrene, for the production of this may be favoured by a disordered state of the general circulation, or a temporary enfeeblement of the cardiac impulse. There is, however, no lesion of the function of the capillaries operating, as the minute vessels are found healthy and empty in the midst of the gangrened parts, just as they are in mortifications that supervene upon ligature. Gangrene from arteritis presents a great analogy to senile gangrene, which may take place slowly or rapidly, according to the amount of ossific deposit, and the other conditions of the subject.

There is nothing constant observed as regards the form, extension, or duration of this result of arteritis. Sometimes the patient dies during the prodromic stage, in consequence of the rapid exhaustion of his powers before the limb has mortified. In other cases, there are eschars, limited to the skin, or the gangrene may attack only one or more toes. Frequently, however, it extends to the foot and leg, or the hand and fore-arm, until the power of the lateral circulation restores the equilibrium, if it succeed in so doing. If even it is arrested, there is a disposition to relapse; and a paresis, and temporary or permanent atrophy of the limb, remains. Danger to life, however, is not alone dependent upon the degree of extension of the gangrene, but also upon the general state; this allowing us sometimes to hope for recovery in even extensive gangrene, while at others it renders a limited gangrene a most grave circumstance. So dangerous an affection is it, that few succeed in escaping from its effects.

Besides the internal changes that may exist as the effects of the malady which has also caused the arteritis, we often find in the artery supposed to be affected but slight traces of lesions. In bad cases, however, a sero-gelatinous fluid is found external to the artery, the cellular coat is finely injected, and the proper tunics are adherent to each other, and fragile. Sometimes there is thickening of the cellular tunic, and exudation of puriform matter or plastic lymph, externally to the vessel, affixing it to neighbouring parts. All these lesions are not of frequent occurrence in arteritis; and except in the case of violence, all the coats of the

vessel may present a normal appearance, and they would be so pronounced, were it not for the obstruction caused by the product of inflammation. This consists of a solid coagulum of plastic lymph, varying in size, length, and degree of adhesion to the vessel. Sometimes small coagula are observed obstructing the artery at intervals; but more commonly it is a single coagulum, one or more inches in length, converting the vessel into a cord. Sometimes, however, the coagulum assumes the form of a canal, or presents here and there small lacunæ, containing a milky or semi-fluid reddish matter, which may also cover the whole surface of the coagulum, or almost constitute its entire substance. Maisonneuve and Cruveilhier have found even the smallest vessels corresponding to the gangrened part obliterated; but, for the most part, the closure will be found only in the vessels above the gangrened part, those corresponding to this remaining open—showing that the coagulum has preceded the gangrene.

The principal veins of the limb sometimes participate in the inflammatory condition, and exhibit the signs of this more plainly than do the arteries. Their coats become thickened, and rich in vasa vasorum; while their cavity is filled with lymph, or, oftener still, by puriform matter combined with cruor. In ordinary cases, however, the principal veins remain free, contain a small quantity of blood, in part fluid and in part coagulated, or, without exhibiting any signs of phlegmasia, are obstructed by a sanguineous coagulum.

As the arteritis is unpreceded by any prodrome, no prophylactic can be employed; but in order to prevent or circumscribe the formation of coagula, the arteritis itself must be actively combated by antiphlogistic means, general or local, according to the amount of reaction and the condition of the patient. These must, however, be employed with due caution; for while we combat the inflammatory action, we have to favour the lateral circulation. As soon as the more urgent symptoms are mitigated, aromatic fomentations or warm applications should be made to the part, improving the patient's diet, and even exhibiting stimuli, if not specially contra-indicated. If the pain is violent, opium is here, too, of great use. These means are, however, often of no avail; for the arteritis, especially when metastatic, appears suddenly, gives rise to the exudation, and at once disappears; gangrene following if the lateral circulation cannot resist, and leaving to the practitioner only the office of administering palliatives. So, too, all attempts at dissipating the coagulum are useless, this remaining even in the case of recovery; and all that can be done is to endeavour to limit it by favouring the lateral circulation. Even in the case of recovery, until the circulation is completely re-established, there is great danger of relapse.

VII. *On the Treatment of the Hydrocele of Children.* By Dr. Linhart. (Froriep's Notizen, 1856, vol. ii. No. 4.)

In hydrocele, met with immediately after birth, there is usually a wide communication with the abdominal cavity; and as there is frequently a fold of gut at the upper part of the tumour, it sometimes occurs that hernia and hydrocele alternate —so that two practitioners, called at different times, may give different opinions respecting the case. This form scarcely requires any special treatment, since the serum returns, during the horizontal position, into the cavity of the abdomen, where it is easily resorbed. The only treatment likely to be of any use would be the keeping the neck of the processus vaginalis compressed by a bandage.

It is otherwise when the hernia occurs later after birth, when it is tense, and the communication with the abdomen is either very small or absent, the processus vaginalis being closed above. In the first case, the fluid will often return slowly into the abdomen, although it may occupy six or eight days in so doing; and such cases deceive the attendants of the child into the belief that the means employed have produced the resorption of the fluid. The deception is the more likely, as, in very great narrowing of the upper mouth of the processus vaginalis, which is often

more than an inch long, re-position cannot be induced by the taxis. This difficulty of returning the fluid is often mistaken for an impossibility, and unnecessary operations resorted to. Indeed, the diagnosis of complete closure is very difficult. When such closure does exist, the case does not differ from one of ordinary hydrocele of the tunica vaginalis.

The indications of treatment are, the removal of the fluid and the closure of the processus vaginalis. With regard to the first, resorption frequently occurs spontaneously, but it can rarely be influenced by the practitioner. The various stimulants employed for this purpose are inoperative, or may be even hurtful by irritating the scrotal skin. When they seem to have been of avail, an aperture has, in fact, existed. The resorption, however, is remarkably facilitated by a subcutaneous incision of the processus vaginalis, which allows the fluid to become effused into the scrotum, where it is rapidly absorbed. A fold of the scrotum should be raised, and a concave tenotomy knife passed in flat between the scrotal skin and the serous sac, so as to make an incision of from one to one and a half inches in length in the processus vaginalis. Dr. Linhart prefers this to seeking to obliterate the vaginal process by means of pressure applied to its neck, which is either ineffectual or cannot be borne, or to the employment of injections, which at this age are not without danger.

VIII. *On the Influence of Phosphate of Lime in the Production of Callus.* By M. A. Milne-Edwards. (Comptes Rendus, xlii. 631.)

The question of aiding the formation of callus by the administration of phosphate of lime has recently been revived in Paris, and the author of this paper alludes to some experiments tried by M. Gosselin at the Hôpital Cochin, especially in cases of fracture of the arm, which are sometimes so long in uniting. In the six cases observed by him the result seemed satisfactory, inasmuch as the apparatus could be removed in from twenty-seven to thirty days, the fracture appearing quite consolidated. As, however, in these cases, the condition of the callus could not be verified, M. Edwards undertook a series of comparative experiments on animals. Fractures as nearly as possible alike were executed upon dogs and rabbits of the same size and strength, to some only of which the lime was administered. The phosphate employed was prepared by the calcination of bones, and consequently was combined with carbonate. The results were decidedly favourable; and the author believes that the phosphate may be usefully employed as an adjuvant, expediting the union in ordinary fractures, and tending to prevent the non-consolidation of others.

From another communication,* it appears that in one of M. Gosselin's cases of fracture of the lower third of the humerus, complete consolidation occurred in thirty days. He administers as a minimum dose half a gramme per diem.

IX. *On the Diagnosis of Sebaceous Tumours.* By M. Chassaignac. (Moniteur des Hôpitaux, 1856, No. 49.)

M. Chassaignac observes, that when sebaceous tumours occur in unexpected localities, he has often found a useful means of diagnosis in observing the exceeding degree of paleness which their surface presents when the base of the tumour is compressed so as to throw this surface into relief; this being much more decided than is the paleness of surface of any other description of tumour submitted to the same procedure.

* Gazette des Hôpitaux, No. 150. 1855.

X. *On the Treatment of Ranula.* By M. Gosselin. (L'Union Médicale, 1856, No. 2.)

M. Gosselin, after alluding to the various modes of treating ranula that have been adopted, and the relapses that are so common after them, describes the plan he has himself found beneficial. He first of all performs excision, as recommended by Boyer, and then cauterizes with the nitrate of silver. Next day he introduces a probe into the wound, owing to its tendency to close, and repeats the cauterization the day after that. On the third or fourth day he enlarges, by means of the scissors, the aperture, which has become too narrow, and on the following days cauterizes again. After ten or twelve days of this assiduous attention, if on the introduction of a probe he finds the cavity is obliterated, he leaves the opening to itself. If, however, a track of a certain extent still exists, he again enlarges the orifice with the scissors. This attention to the case is rarely required beyond fifteen days, when the external opening becomes closed, and the cavity being obliterated, there is no fear of relapse. M. Gosselin has operated in this way in several cases, and in three of these, which he has watched for several years, no relapse has ensued, the opening remaining closed. This plan of procedure has also been extended to various analogous cases, in which there is a cavity with secreting walls, having no spontaneous tendency to approach each other.

XI. *Injection of Balsam of Copaiba in Gonorrhœa.* By M. Dallas. (Gaz. des Hôp., No. 45, 1856.)

Mr. Dallas, of Odessa, states, in confirmation of the observations already published by Taddei, Marchal, and others, that the injection of balsam of copaiba is the most efficacious mode of treating gonorrhœa. In sixteen cases he has so employed it, using no internal remedy, either in recent or old gonorrhœa, with complete success. His formula is copaib. five drachms; one yolk of egg; gummy extract of opium, one grain; water, seven ounces. The injection should be used several times a-day.

XII. *Lupulin in Spermatorrhœa.* By Dr. Pescheck. (Buchner's Repert. für Pharm., No. 1, 1856.)

Dr. Pescheck has employed lupulin for several years in a great number of cases in which spermatorrhœa seemed to depend upon no mechanical cause. At first, he used to give two grains night and morning; but finding such doses of no avail, he prescribed from ten to fifteen grains to be taken just before bedtime, prohibiting the drinking of water after it. From such doses, even continued for a long time, he has found no inconvenience to arise, while they have acted beneficially on the disease. In some cases he combined with it one or two grains of pulv. digitalis. A valuable peculiarity in the operation of lupulin, is the beneficial action it exerts upon the digestive process, which so often suffers in these cases. It is also very useful in mitigating the urethral irritation and discharges consequent on former excesses, and in many cases more so than iron or quinine. Its especial utility in the chordee of gonorrhœa, Dr. Pescheck has had many opportunities of witnessing. It is best administered without any additions that might diminish its bitterness, as its effects are very proportionate to the intensity of this property. Old lupulin deprived of its oil and bitter taste is almost always useless.

XIII. *Occlusion of the Eye in Ophthalmia.* By M. Bonnafont. (Bulletin de l'Académie, t. xxi. pp. 437—524.)

A very animated discussion has recently been occasioned at the Académie de Médecine by a paper of M. Bonnafont's on this subject. He states that at the

military hospital, Du Roule, he has found this treatment more efficacious than any other. A piece of fenestrated and cerated rag, the exact form of the orbit, is applied over the closed eye. Over this, a light pledget of charpie is laid, and the whole covered by a largish disc of diachylon, which is retained by a bandage. The eye remains thus closed during several days. In bad purulent ophthalmia the apparatus has to be removed every day, to prevent the accumulation of pus; but when the conjunctiva is only moderately injected, the dressing need only be changed in from two to four days, at the end of which time the surgeon examines the eye, and determines whether the occlusion should be recommenced. In slight conjunctivitis three or four days usually suffice for a complete cure. A chemosis or ulcerated keratitis requires a little longer time; while one of the cases related shows that complete success may follow this plan in eight or twelve days in ulcer of the cornea, with chemosis and well-marked iritis. It requires some tact to know when to remove the apparatus. If the patient complains of much pain, or if the plaster is distended by discharge, it must be at once removed, and the eye and the surrounding parts well cleansed before renewing it. M. Bonnafont believes the treatment is applicable to *all* varieties of ophthalmia, always taking care to adapt the apparatus very accurately, and to watch its effects closely.

During the discussion at the Academy, M. Bégin stated that he had employed this permanent closure of the eyelids in ophthalmia for a long time at Val-de-Grâce, on the principle of abstracting the inflamed organ from the operation of its habitual stimuli—light, air, and friction of the eyelids. He has found it especially useful in cases of keratitis, that are so often obstinate; in iritis; and especially in the scrofulous ophthalmia of children, when accompanied by excessive photophobia.

M. Larrey stated, that from observations he had made many years since, he had come to the conclusion that occlusion might often be employed with advantage in affections of the eyes, but that he was far from joining in M. Bonnafont's indiscriminate recommendation of it. He thinks collodion a very good means of securing the occlusion, although some patients dislike this, on account of the irritating heat it gives rise to. The occlusion is especially applicable in wounds of the globe, which may give rise to hernia or evacuation, in contusion and concussion of the eye to prevent inflammation and promote absorption, and in passive ocular congestion, as hydrophthalmia. Active congestion and phlegmon, in his opinion, contra-indicate it. He thinks it injurious at the commencement of purulent and other acute forms, but of service when these have reached a stationary or chronic stage. It is useful in conjunctival chemosis, and in aiding the reduction of granulations and varicosities, but it is contra-indicated when any tendency to abnormal adhesions exist. It is especially in affections of the *cornea* that it is applicable, it being as useful in recent wounds as in old ulcerations. It may also facilitate the diminution of staphyloma and the absorption of superficial opacity. All things being alike, it is especially indicated in patients who are refractory to other means of treatment—as children, peasants, conscripts, the insane, &c.

M. Piorry also had long employed occlusion with advantage in *non-purulent* ophthalmias, in wounds of the eye, in procidentia of the iris, after the operations of cataract and artificial pupil, in iritis, in certain neuropathies of the fifth pair, and after the entrance of foreign bodies into the eye.

M. Velpeau did not agree with those who think the air acts mischievously on inflamed eyes, and he denied the utility of keeping patients suffering from these in the dark. The best means of diminishing photophobia is obliging the patient to bear the exposure to air and light; the photophobia then soon diminishing, and the recovery proving more rapid than when the patient is kept in the dark. He has always found that the photophobia was rapidly diminished by such exposure of the eye. Comparing the facts adduced by M. Bonnafont with the results of his own practice, he finds that the mean duration of the treatment is greater under occlusion. Occlusion is, however, of utility within narrow limits, and M. Velpeau has employed it with some advantage in non-purulent conjuncti-

vitis, in some abscesses and traumatic lesions of the cornea, but not with the view of excluding the light, but of maintaining a certain amount of compression, and always using at the same time other means appropriate to the disease in question.

XIV. *On the Comparative Value of Amputation at the Knee-Joint and of the Thigh.* By M. Baudens. (Comptes Rendus, tome xli. p. 1077.)

M. Baudens states, in a recent communication to the Académie des Sciences, that the above question is one of those that have engaged his attention during his directorship of the French army in the East. He found that the opinions of all the medical officers whom he consulted, whether in the Crimea, at Constantinople, or the military hospitals at Marseilles and Toulon, were in favour of disarticulation of the knee whenever the amputation of the extremity could not be performed below the patella. And, in fact, the disarticulation of the knee has succeeded in a given number of cases oftener than the amputation of the thigh, even when performed at the lower third. But the disarticulation is only to be preferred upon one express condition—viz., that it be performed immediately after the receipt of the injury. Consecutively, amputation of the thigh should be preferred. This second statement agrees in every respect with all that he has observed, written, and taught during the ten years he has been at the head of the Val-de-Grâce. The excellent results of disarticulation of the knee, especially recorded in his Clinical Observations upon Gun-shot Wounds, were obtained in soldiers who had just been wounded on the field of battle. This difference in the results derivable from immediate and secondary amputation at the knee-joint, depends upon the fact that even in a state of health the size of the bones is not in complete accord with the amount of soft parts—a disproportion that becomes still greater when the patient has lost flesh during prolonged suffering and abundant suppuration.

In another communication, M. Baudens observes that, although the surgeons of the Sardinian army in the Crimea hesitated to employ chloroform, those of the French army have used it in twenty-five thousand cases without any accident resulting. It was always administered with great care, so as not to go beyond the production of insensibility.

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D. (Lond.)
Late Physician-Accoucheur to the Western General Dispensary.

I. Physiology and Pathology of the Unimpregnated State.

1. *Obstacle to Menstruation from a Fibrous Membrane capping the Os Uteri.* By M. Fournet. (L'Union Médicale, January 26, 1856.)
2. *On the Therapeutic Effects of Ergot of Rye and Digitalis in Metrorrhagia.* By M. Carrière. (L'Union Médicale, March 11, 1856.)

1. A lady, aged thirty, had suffered at fifteen from measles, which was attended by a very severe peritonitis or metro-peritonitis. At sixteen the first appearance of menstruation. After menstruating pretty regularly for a year, at seventeen each menstruation was preceded and accompanied by great sufferings, which went on increasing ever since; then there were very violent colics, sometimes followed by convulsions, a sensation of swelling and weight in the region of the uterus. These subsided as soon as the blood escaped externally. A marriage of several years had remained sterile. Examined in August, 1855, Dr. Fournet found the os uteri thrown forwards, and covered by a dense fibrous membrane, which adhered

by five-sixths of its margin to the circumference of the os tincæ, where it was continuous with the substance of the cervix. The point of interruption of attachment was marked by a crescentic notch, with a thick border, behind which the point of a probe could be inserted. This membrane was dissected away. The patient was relieved of her dysmenorrhœal symptoms.

2. M. Carrière insists much upon the value of a combination of ergot of rye and digitalis in the form of pills in metrorrhagia. In the cases he relates, the conditions, whether systemic or local, connected with the hæmorrhage, are not noticed. He points out that in order to succeed it is important to attend to the following circumstances:—When there is depression of the pulse, digitalis would keep up hæmorrhage, instead of repressing it; when it is full and strong, this is the case for combining it with ergot. It is to be observed, he says, that the further we recede from the small doses, the nearer we arrive at those which call into action the contractions of the whole organ. This is going too far. Tolerance of the remedies by the stomach is ensured by the trisnitrate of bismuth.

II. Pregnancy.

1. *Case of Tetanus in a Pregnant Woman.* By Dr. Mikschik. (Wochenbl. d. Zeitschr. d. Gesellsch. d. Aerzte zu Wien, No. 33, 1855.)
2. *On Extra-Uterine Pregnancy.* By J. W. Wilson, M.D. (Indian Annals of Medical Sc., October, 1855.)

1. A girl of sixteen (?), eight months in third pregnancy, five days before admission to hospital, without known cause, was seized with very painful cramp in the fingers and toes of right side. The fore-arm was bent, but could be extended, but returned immediately to former position. No fever, no cephalalgia. Child alive. Eight leeches to nucha. Next day, cramp of right foot quite ceased; frictions with chloroformised oil. Third day, excepting a little stiffness of fingers, cramps quite stopped. Fourth day, in the night former state returned, both sides being affected. Fifth day, same state, with some tension of masticatory muscles; in the evening a tetanic fit, not relieved by chloroform. From the sixth to the tenth days, the tetanic fits recurred with more frequency and violence; on the eighteenth she died during a fit. The Cæsarian section gave a dead child. The abdominal muscles were not involved in the tetanus. No albumen in urine. Nothing found on autopsy. No trace of injury.

2. Dr. Wilson, Professor of Midwifery, Calcutta, relates two cases of extra-uterine pregnancy.

Case I.—Loocharee, aged twenty-three, admitted September 12th, 1855, had always menstruated regularly till about five or six months before admission. When, as she supposed, about four and a half months gone, she suffered an attack of fever, with pain in the iliac regions. She stated she had partially miscarried. The uterine tumour was felt two fingers' breadth above the navel; a great deal of abdominal tenderness; much pain, but unlike labour. Sanguineous discharge from vagina; os soft, open; diarrhœa. Attempt made to induce premature labour. Uterine sound was passed nearly as high as the navel. Increasing tympanitis; as attempts to relieve tension by drawing off the air and liquid by metal and flexible catheters, introduced their entire length through the os, failed, a trocar was pushed into the left iliac region, where the tympanitis was most marked. A considerable quantity of stinking gas issued, and some ounces of an exceedingly fœtid liquid, but the tumour could not be emptied of liquid or air. A second puncture was made below the umbilicus, where the tumour felt soft and fluctuating; arterial blood, evidently placental, welled up through the canula; slight pressure restrained this. The woman's health was now rapidly declining; a director was introduced

through the open puncture, with the view of enlarging the opening to extract the fœtus and wash out the cyst; but the introduction of the director was followed by a free discharge of arterial blood, readily checked by pressure. As appeared afterwards on dissection, had the incision been carried in the direction intended, the mass of the placenta would have been divided. With the view of introducing the finger to ascertain the position of this, I directed the skin-wound to be enlarged Death on 2nd of October. *Autopsy:*—The parietal layer of peritoneum was of a dark colour, and thickened. From below the navel it was adherent for about two inches, but a free shut sac was formed over the pubes, where adhesion had not taken place. The true cyst was beneath this, and in it a five and a half or six and a half months' fœtus, in an advanced stage of decomposition, lying in a dirty offensive fluid. The placenta was large, situated anteriorly, and to the left. There were no distinctive membranes. The uterus, of normal size, lay behind, and a little to the right; there was a small opening on the left side, communicating with the sac. The intestines and parts contiguous to the sac were so adherent and matted together, that the ovarian tubes and ovaria could not be traced.

Case II.—Dwya, aged twenty-one, admitted 26th April, 1855. Says she menstruated first at twelve years old, and became pregnant immediately after this first menstruation, and at her full time had an easy labour. Subsequently, for eight years, no return of menstruation. She supposes she is now in her eighth month (natives of Bengal reckon pregnancy by lunar periods). Six weeks before admission, she was much reduced by an attack of cholera (every evidence of spanæmia). No indication of kidney disease. Sounds of fœtal heart, 140; mother's pulse, 120. Child last ascertained alive on 16th of May. On the 18th, she said, "My child is dead; it has not moved for some days." From this time she lost ground. Unnatural gestation not being suspected, attempt to produce premature labour was made, ineffectually. The uterine sound being passed up to rupture the membranes, passed to the usual depth of the unimpregnated uterus, where its progress was arrested. At this time, the child could not be felt through the abdominal parietes, and there was a hard, lumpy substance felt above pubis. Extra-uterine pregnancy now diagnosed. Her full time was complete in the first week of July. The solid tumour became less distinct. On the morning of the 3rd, the abdomen seemed swollen and more tense; and on percussing it, the usual completely dull sound was not returned; the tumour over the pubis was no longer perceptible. She had suffered during the night a paroxysm of shivering, followed by febrile symptoms. An enema given returned with some fæces; and shortly afterwards there was observed a continued dribbling of a turbid-looking fluid from the rectum, similar to what was afterwards found to exist in the sac containing the child. She gradually sank, and died on the 6th. An attempt to relieve the tympanitis and explore the case, made by puncturing the fundus of the uterus, failed of success; as was afterwards ascertained from the circumstance of a thick placenta adhering to the outer part of the fundus of the uterus, and preventing the instrument reaching the interior of the sac containing the child. *Autopsy:* Peritoneum, abdominal viscera, and sac, firmly matted together. The sac occupied the fore part of the abdominal walls to the ensiform cartilage. Uterus of natural size. Surmounting the fundus was the sac, of a dark-grey colour, without any distinctive membranes, its walls adherent on all sides to the contiguous parts before and behind, out of which it seemed formed. It contained a child of apparently eight months' development, and a placenta attached to the exterior of the fundus uteri. Where the ovum was originally detained and developed was not ascertained; but the parts, as they were displayed, had an appearance as if it had lodged somewhere near the fundus, either in the Fallopian tube where it joins the substance of the uterus, or near to this extremity; and that it had in its growth lifted the peritoneal covering from the fundus, the peritoneum being carried out as an investment of the sac. The communication with the rectum was not found.

III. Labour.

1. *Case of Tetanus following Abortion.* By Mr. Bazunjee Dossabhoy. (Trans. of Med. and Phys. Soc. of Bombay, 1855.)
2. *Two Cases of Unnatural Labour in which the Cæsarian Section was performed.* By J. Fayrer, M.D. (Ind. Ann., Oct. 1855.)
3. *Three Cases of Puerperal Convulsions successfully treated by Chloroform.* By M. Frémineau. (L'Union Méd., 5th Feb. 1856.)

1. Early in the morning of the 24th of April, 1853, a Parsee female, aged twenty-six, mother of four children (the youngest a year and a half old), in the third month of pregnancy, after suffering for several hours during the night from uterine pains, had hæmorrhage come on. The os uteri was dilated a very little. She was treated by dilute sulphuric acid and opium, with cold applications; but the pains and discharge continued throughout the day, and at six P.M. an embryo was expelled. The pains then ceased, and the hæmorrhage merged into the ordinary sanious discharge. On the 30th, difficulty in opening the jaws to the full extent was complained of; but there were no other symptoms of tetanus until the 5th of May, when the abdomen became somewhat hard and tympanitic. No stiffness of any other muscles, except those of the jaws. The pulse continued at 100—small, compressible. She remained in this state to the 17th, when spasmodic contraction of the muscles of the back and lower extremities came on; and there was increased stiffness of the muscles of the neck and abdomen. The pulse was 112, of same character as before. She was treated by hemp, quinine, and generous diet; and recovered by the middle of June—menstruation having occurred at the usual period, but rather profusely.

2. Dr. Fayrer relates two cases of Cæsarian section.

Case I.—A Mussulmanee, aged thirty-five, admitted into the King of Oude's Hospital, Lucknow. Labour arrested. Obstruction was caused by general distortion of pelvis; the tubera ischii closely approximated; the rami of pubes are almost in apposition. The outlet is reduced to a passage so small that one finger can with difficulty be introduced. The woman was much deformed in body from rickets during infancy. Spine curved laterally and posteriorly. Extraction of child, even piecemeal, by natural outlet, would have been impossible. The Cæsarian section was performed under chloroform. Hæmorrhage not very great. Opium, calomel, and salines constituted after-treatment. Death on third day.

Autopsy.—Wound partially united; intestines adherent to each other, to the abdominal parietes, the uterus, and the bladder, by coagulable lymph. The cavity of the abdomen contained a quantity of bloody fluid, with shreds of lymph; and the surface of the peritoneum was also covered with it. The uterus had contracted, but the wound was not completely closed; hence the hæmorrhage into the cavity of the abdomen. The child presented some singular abnormities. The body was in size fully natural; weight, eight pounds; length, twenty inches and a half. The right foot was absent. On the left hand, the index, middle, and ring finger were wanting. The fingers of the right hand all present, but malformed; some of them consisting of only two phalanges. The frontal bone was all or nearly absent; upper lip cleft. A tumour, about three inches above the left eye, appears to be a portion of the brain. The child lived for about twelve hours, sucked some milk, and passed meconium.

Case II.—A Mussulmanee slave, aged thirty, admitted at same hospital, 15th April, 1854. Says she has been in labour eight days. First conception; says she is considerably over nine months. Irritative fever. The outlet of the pelvis is so contracted that the finger can with difficulty reach the os uteri. The rami of the pubes approximated; the sacrum bulges forward, reducing the passage so much, that delivery by natural passage seems impossible. Cæsarian section under chloroform. On making incision through the uterus, the placenta was exposed,

being attached to the anterior surface of the womb; profuse hæmorrhage instantly took place. Without loss of time, two dead female children were removed, attached to a single placenta. The hæmorrhage for a moment was frightful, filling the cavity of the abdomen; it was sponged out as quickly as possible, and the uterus stimulated to contract by pressure. She remained under the influence of chloroform for about two hours. Calomel and opium were ordered at once, and ice kept to the abdomen until it produced shivering; the bleeding then ceased. Next day, vomiting. Third day, death.

Autopsy.—No attempt at union of wound in abdomen. Commencing inflammation of intestines; some watery fluid effused into abdominal cavity; the peritoneum in an incipient state of inflammation; the wound of uterus closed by coagulated blood and lymph; and some coagulated blood in cavity. The uterus had contracted so as almost to close the wound. The inlet of the pelvis was contracted to a diameter of less than three inches each way; the outlet was so narrow that the finger could but just pass through, the sacrum bulging forward, and the tubera ischii and rami of the pubes being in close approximation. The body, with this exception, not much deformed.

3. M. Frémineau relates three cases of puerperal convulsions treated by chloroform, which terminated successfully.

Case I.—A primipara, aged twenty-five, at end of gestation had general cephalalgia, redness of face, vertigo, vomitings. At access of labour, a sudden intense fit of eclampsia, with protrusion of the tongue, which, cut and compressed by the teeth, became so swollen that asphyxia seemed imminent. This state lasted for two days. Delivered of a living child. Symptoms persisted; convulsive fits very frequent. Chloroform inhalations, lasting twenty minutes each, and an injection of chloroform. After each inhalation, the patient fell into a state of complete sedation; then, when a fit appeared imminent, she was again submitted to anæsthesia. In the evening she was much better. This treatment was continued for three days, during which time the fits gradually diminished in frequency and severity.

Case II.—A primipara, aged twenty-four, six months pregnant, had been seized two days with eclampsia when admitted (25th March, 1855) into the Hôtel-Dieu, under M. Piedagnel. Loss of consciousness, dilated pupils, convulsive movements, no albuminuria. Attacks followed by hemiplegia. Venesection, and potion containing twenty minims of chloroform. Next day, another attack, same treatment. 27th, another attack; venesection. From the 1st to 7th April, the consciousness and movement returned; at times, still stiffness in limbs; prolonged baths. The 11th, a violent fit; venesection, and chloroform potion. Patient improved. The 14th, after some annoyance, another violent fit; venesection, chloroform potion. Patient now very anæmic. Question of artificial delivery discussed; postponed. Slight convulsive attacks recurred at intervals until delivery on 13th May. Child living. No attack after delivery.

Case III.—A primipara, aged twenty-four, was delivered at the Hôtel-Dieu, in the night of the 21st-22nd November. For some days before, headaches, vertigo; a fit of eclampsia on the 19th, the day when indications of commencing labour appeared. 22nd, profound coma, dilated immovable pupils, stertorous breathing, and occasional convulsive shocks; chloroform potion. 23rd, better. 24th, coma, but no convulsions. 25th, consciousness. From this time gradual recovery.

(The perusal of these cases leaves the impression that proof is wanting of the influence of chloroform in promoting recovery. The repeated bleedings in the second case may have been the more efficacious remedy. The cases seem, however, at least to show that chloroform may be taken in eclampsia without producing injurious effects; and encourage further research.)

IV. Puerperal State.

1. *Note on Puerperal Fever, as it occurred in the Clinique d'Accouchements of Paris under M. Dubois in* 1854. (L'Union Méd., 6th November, 1856.)
2. *The Erysipelatous Disease of Lying-in Women.* By D. Leasure, M.D. (Amer. Journ. of Med. Science, January, 1856.)

1. In the months of October and November, 1854, there broke out in the clinical hospital of Paris a certain number of cases of puerperal fever. The invasion of the disease in the obstetric wards was preceded, as almost always happens, by the appearance in a great number of delivered women of gangrene of the vulva and vagina. At the same time there was observed in several new-born children a disease rarely seen under other circumstances,—namely, muguet. The course of the disease, its spread, and decline, are not described, so that several of the most interesting questions in the history of this disease receive no illustration from this communication. Three cases in which autopsies were performed are given in detail.

Case I.—On the 17th of October, a woman was delivered after an easy labour. The same day, slowness in answering and embarrassment in speech were observed; the following night, agitation and delirium; next day, face hot, red, eyes injected, abdomen painful, a violent shivering, lasting fifteen or twenty minutes; bowels freely opened by castor-oil; at night, again agitation and delirium. Fourth day, bled to 300 grammes. Fifth morning, patient much worse, respiration irregular, 32 inspirations a minute, pulse, 132; abdomen not very tender on pressure. Calomel, half a centigramme every hour. A large red spot, very painful, on the elbow. Sixth day, she died.

Autopsy, forty-eight hours after death.—Injection of the intestinal peritoneum; no pus or false membranes; a small quantity of limpid serum in the retro-uterine peritoneal sac. Uterine peritoneum injected, especially in front; a small false membrane at point of union of right Fallopian tube with uterus; left Fallopian tube adherent to side and back of uterus, its canals containing a creamy, homogeneous pus; tissue of uterus not inflamed; ovaries, nothing remarkable; marked injection of the meninges, especially at base of cranium.

Case II.—Woman delivered after natural labour on 17th October. Two days after, she complained of a pain in right fore-arm; nothing particular observed there. Third day, pain persists. Fourth day, less pain in fore-arm; abdomen a little painful; pulse, 108. Fifth day, general state bad; pulse 124; great depression. Seventh day, salivation induced by mercurial inunctions to arm and abdomen; seems better, but complains of pain in calves and in course of femoral vein; nothing observed. Eighth day, seems still better. Tenth day, difficult breathing; prostration; died.

Autopsy.—No trace of peritonitis. Uterus, at right angle, in substance of walls, several small purulent deposits, surrounded by pyogenic membranes; these abscesses are of size of nuts; the vessels and uterine sinuses near are healthy; elsewhere, and especially in the opposite angle, the venous sinuses contain small, firm clots. Right kidney contains in its upper part a small abscess. Miliary tubercles in summit of left lung. Right fore-arm: skin red; sub-cutaneous cellular tissue red, denser than natural; muscles violaceous, gorged with blood, thickened, hard; no pus.

Case III.—Woman delivered 16th October. On the third day, she complained of pain in the left fore-arm; there is a little œdema; skin hot. Fourth day, pain much increased; limb held fixed in pronation; pain also in leg; nothing observed. Fifth day: arm in same state; pain in outer side of foot and ankle; pain in median line of abdomen. Mercurial inunction; calomel. Sixth day: abdominal pain continues; arm swelled and painful; redness on outside of painful leg. Ninth day: fluctuation commencing in arm; the place was punctured next day, and pus escaped. Eleventh day: another abscess on back of hand; acute

pain in left side, aggravated by coughing; dulness at lower part of lung; mucous rales, and diminished respiratory sounds, besides ægophony. Thirteenth day: death.

Autopsy, fifty hours after death.—No effusion in abdominal cavity; no adhesions; intestinal peritonæum slightly injected; a small false membrane on broad ligament behind ovary. Uterus: marked greyish granulations on inner surface at seat of placenta; sinuses at this level are coloured yellow by pus, but no purulent liquid in their cavities. Ovaries: the left contains an abscess; the corpus luteum is near this abscess. The lymphatics running from the broad ligaments are filled with pus, swollen. Right ovary holds a little pus; lymphatics scarcely visible on this side; ganglions swollen, red, not purulent; kidneys, liver, and pancreas contain no pus. Thorax: left pleural cavity contains sero-purulent effusion; false membranes on costal pleura; lung compressed, tissue hepatised, and containing small indurated foci and droplets of pus. Pericardium: signs of inflammation.

2. (The communication of Dr. Leasure is of great interest, as illustrative of the pathology of puerperal fever.) During the month of March, 1852, an epidemic erysipelas made its appearance in Newcastle, which seemed to put on features of extreme malignity from the very outset, but few of those attacked survived. In the early part of April the first case occurred in my practice. The case was that of a young woman. The throat seemed the principal seat of the disease for the first five days, when the erysipelatous spot made its appearance on one cheek, from which it spread all over the face, head, and neck, and ultimately proved fatal, after rendering her almost a putrid mass whilst still living. On the 11th of April, while engaged in the attendance on this case, I was called to attend Mrs. S. in her seventh labour, a very easy one. About twenty-four hours after delivery she got out of bed, feeling very strong, had a chill, followed by fierce fever; delirium in night. Next morning, abdomen swelled rapidly. Forty-eight hours after delivery, when seen, she was *in articulo mortis.* She died next day. The infant died four days afterwards of malignant erysipelas.

I now, says Dr. Leasure, declined attending any more cases, as I was still attending cases of erysipelas. But on the 6th of August, in the absence of the physician, I took charge of Mrs. ——, in labour with her eighth child, of which she was delivered at 1 A.M. Labour natural; child healthy. She continued to do well until 11 P.M. of the 7th, when severe chills set in. They continued for eight hours. Fever followed; pulse 126; intense pain in uterus; no tumefaction of abdomen; countenance anxious, haggard, with frequent frowns and earnest staring, as if at some strange object; lochia not suppressed, but dark and dirty-looking; tongue natural; thirst; obstinate vomiting. I opened a large vein, intending to bleed *ad deliquium,* but the blood soon ceased to flow. The blood did not coagulate, and resembled some dirty mixture, that looked like anything but blood. It had not even the colour of blood. I gave full doses of opium. She died in thirty-six hours of the first chill. The child died of malignant erysipelas within a week; and the old lady who washed and dressed her for the grave took erysipelas within five days of the time she died, but finally recovered.

Under precisely similar circumstances I was constrained to attend Mrs. —— on the 24th of May. Labour natural, not lasting over three hours. She did very well until twenty-six hours after delivery, when she had a chill. Six hours afterwards I visited her, and found her in very nearly the same condition as No. 2. I determined "to bleed her to death," or break down the disease. I opened a vein and took half a gallon of blood. The pulse was not reduced in frequency (130), but became very soft. The blood did not coagulate, but seemed to be dissolved. She died in thirty hours after the first chill.

At the same time, Dr. J. W. Wallace was attending some cases of malignant erysipelas, and the only two cases of labour he attended were followed by precisely similar symptoms, both dying within thirty hours of the first chill. No cases occurred in the practice of the other physicians of the town.

MEDICAL INTELLIGENCE.

Microscopic Appearances of Evacuations in Yellow Fever, observed by Dr. Blair.

THE following letters from Dr. Blair to Dr. John Davy contain observations of so much importance, bearing upon the pathology of yellow fever, that we have much pleasure in making room for them. We may add, relative to the morbid specimens referred to in Dr. Blair's first letter, that Dr. Davy finds their appearance under the microscope to correspond with the description, and that the latter gentleman entertains no doubt of their being portions of vessels.

George Town, Demerara, March 8th, 1856.

MY DEAR SIR,—I beg to enclose for your examination a sma'l fragment of material which was expectorated by a seaman, Thomas Bailly, suffering from yellow fever in the Seaman's Hospital, on the 29th ult. The expectoration at the time of observation was of considerable quantity, amounting probably to an ounce. Some of it had a clear glairy appearance, and some was of rather an opaque white, and of a tenacious consistence. Mixed with this expectoration were several red spots, apparently minute blood-clots. On microscopic examination, the pale portion was found to consist chiefly of epithelium, but no cilia were observed on the cells, which were in general very perfect. Several fragments of broken capillary vessels were found mixed with it. When the red spots were subjected to examination, they were found to consist of bundles of capillary fragments, tinted of a bright pink or crimson, and without blood-corpuscles being present. Under the one-fourth and one-eighth inch object-glass of Ross, several of these capillaries were found to be colourless. I enclose a small portion of this material in tinfoil; and, lest decomposition should injure the specimen before it arrives at its destination, I have mounted a minute portion in Canada balsam, which is also sent. Although it is only a week put up in the balsam, I find that it has lost much of its brightness of colour already. The fimbriated ends are also injured. It would likely have done better in a glass cell preserved in Goadby's solution, but I feared that the thin glass of the cell would have been fractured in passing through the post-office. I hope, however, that between the two samples sent, sufficient may reach you to enable you to form a correct idea of its structure. On the 4th of last month, in the case of a seaman named Morrison (fatal), I for the first time observed the undoubted presence of broken capillary vessels in the excretions of yellow fever. In his case, also, it was first noticed in the expectoration. On all former occasions, epistaxis or bloody expectoration was looked on carelessly, as merely a manifestation of the hæmorrhagic tendency, and nothing was expected to be seen but blood-corpuscles under the microscope. These symptoms were therefore almost unheeded hitherto. On this occasion, however, some turn of thought suggested more particular attention to the subject, and the examination of Morrison's bloody sputa led to important results. I have since found the existence of broken capillary vessels one of the commonest phenomena of the disease. They are to be found sometimes in great abundance in the urine, in the alvine evacuations, in the white vomit, in the flaky sediment of the black vomit, in the bloody exudations and hæmorrhages from the mouth, and even on the blistered surfaces. In the flakes of black vomit, it is sometimes necessary to dissolve off the albuminous matter by a drop or two of liquor potassæ before they come fully into view. I had often seen them formerly in the urine and black vomit, and other fluids eliminated from the subjects of yellow fever; but as in most cases they are colourless and empty when so found, I was wont to set them down as extraneous bodies, and suspected them to be fibres derived from the linen sheets and towels of the establishment. With this preconceived idea, they were of course overlooked and unrecorded. On turning up some old mounted specimens of "caddy stool" of the epidemic of 1851, I find these vessels still existing in them. The fragments of

capillaries are found generally in single cylinders; I have seen, however, a few branched and bifurcated. Their tendency to break off seems to be at the *bendings.* The fracture is occasionally clean, but generally the broken end is split into filaments. A separation of filaments seems to be the mode in which the fracture occurs; and in many fragments, the length of which will occupy three or four times the field of vision of a half-inch object-glass, several partial fractures may be observed in which the tube at such points is split all round longitudinally, and a perfect sub-division is about to occur. At such points on the outer angle, and at the open ends of the capillary fragments, the *débris* of blood corpuscles is to be seen, and these sometimes form a little dossil which is seen connected with the tube of the vessel by fibrillæ. In the urine, I have seen some of the capillary fragments enveloped in the tube-cast material, but encrusted evidently with flat instead of spheroidal epithelium. I cannot observe in the specimens which I have now sent, any epithelial lining within the capillaries; and yet their calibre, I think, is such as would lead us to expect its presence, were they not diseased. In some of the specimens which I have kept of the same expectoration, epithelial matter is visible alongside of the broken capillary vessels, as if it, as well as the blood, had escaped from their cavities. Finding that ecchymosis of the conjunctiva, epistaxis, and some other hæmorrhagic appearances, are common in yellow fever long before the blood has apparently lost any of its fibrine; and finding that even when black vomit is established and the tongue is smeared with blood, the corpuscles are normal in appearance, I cannot but look on the textural lesion of the capillaries as a primary effect of the yellow fever poison, and as the *cause* of the congestions, ecchymoses, oozings, and hæmorrhages, and all their consecutive mischief. The phenomena of the present minor epidemic also corroborate the view that the poison attaches itself to the mucous membranes in the first instance. Its early effects seem to be local. The system is thereby inoculated, and the poison spreads to all the analogous tissues of the body. A general impregnation of the *circulation* in the first instance would be scarcely compatible with the fact of the slow, steady march of the pathogenic influence through the various organs of the body. I enclose the case of Thomas Bailly, as reported in our hospital case book. It will be seen how his attack commenced like a "common cold"—began in the bronchi, and how it gradually extended to the conjunctiva, mucous membrane of the mouth and fauces, to the liver and kidneys, and its final resolution. I may mention, in "reporting progress," that I have detected the glandular cells of the liver to be a common and very large constituent of black vomit. Their shape and size and tint, and the presence of minute oil-globules beside the nucleus in the epithelium, leave no doubt in my mind as to their identity. In the flakes of the black-vomit sediment, also, there is not much difficulty, with the addition of liquor potassæ, in distinguishing the bile-flakes from the blood-flakes.

I remain, my dear Sir, ever faithfully yours,

D. Blair.

Dr. John Davy, F.R.S., &c. &c.

George Town, Demerara, April 24th, 1856.

On the 11th inst., while visiting early in the morning a patient of the Seaman's Hospital, named Nolin, I saw in his basin a few ounces of black vomit, with clear, slightly brown-tinted, supernatant fluid, and well-defined sooty sediment in little flakes. This vomit seemed formed from intermixture of food, drink, medicine, blood, saliva or expectoration. Seeming to be as pure as could be obtained, I carried home with me for experiment a small phial of it. My first experiment was to evaporate a large drop of the sediment and serum on glass slips, in the sunshine. I mounted both in Canada balsam. That of the former has given me a fine specimen, in which, along with numerous loose oil globules, many of the glandular cells of the liver (in which the black vomit abounded) are well preserved. I noticed when spreading out the sediment with needles, in order to render it sufficiently translucent for mounting, that it was somewhat flossy in texture, and showed a reluctance to be subdivided. After mounting these, I dropped a little

of the sediment on a number of glass slips, for the purpose of applying reagents. In dropping it from a wide-mouthed pipette, I found the sediment had a tendency to fall in little separate masses or blebs. They dried in the shade in a few hours. After drying, I found each specimen of sediment encircled with a pellucid ring of dry serum, which had oozed out of the sediment. This under the microscope showed only an amorphous glittering. But when I applied a drop of acetic acid to the centre, and it flowed over the margin, the whole pellucid ring started into view filled with colourless, slightly opaque tubules, in the most beautiful loops and reticulations. I need not say with what surprise and delight I looked on this unexpected vision. The tubules were in two sets—the inner in regular network—their diameter filled one space of Ross's micrometer eye-piece, under the half-inch glass. The external set of tubules were at least twice the diameter of that of the others, and their arrangement was in large open festoons. Some of them seemed terminal and acuminated at one end, and appear to lie in a curtain of basement membrane. Within both sets there appeared numerous minute granules. When the acetic acid was stirred about in the sediment (the coloured central portion), numerous detached hepatic cells were brought well into view, and by tearing up this part of the sediment with needles, I observed that several of the large tubercles permeated the mass. As usual, several common capillary fragments were present. After a few minutes, the tubules seemed to dissolve, and the acid evaporated, but on a re-application of the acid they reappeared, although not at all with the former clear definition. To another specimen of the dried sediment I applied a drop of water, and found that by it I could detect the tubules, but they were faintly marked, and might have escaped observation had they not been looked for. These vessels are evidently quite different from the capillaries I have hitherto noticed in the excretions of yellow fever patients; while the latter are generally straight and rigid, or broken off at sharp angles, the others are beautifully wavy, and sometimes duplicate, and symmetrical. Alcohol and ether acted peculiarly on these tubercles. A movement was instantly caused among them. The meshes swelled up and unravelled themselves, and showed at the angles of the network that the gyrations preserved the same calibre as the other parts. The ether acted in a very fugitive manner, the field soon being obscured by condensed vapour and the haziness from the diffused fat of the liver cells. The dissolved fat of the alcohol did not offer much obstruction to the light. *Liquor potassæ* brought out the tubules faintly and transiently, probably from dissolving them rapidly, but it at the same time extricated several fine large films of basement membrane from the coloured sediment. After the evaporation of the acetic acid and ether, the tubules became again invisible. But after the evaporation of the alcohol they were still to be observed, but in an abnormal condition, and much less distinct than when wet with that reagent. The specimens acted on by liquor potassæ and nitric acid remained in a moist condition for several days, but without a trace of tubule after that time. In the first instance, the nitric acid acted fully as well as any of the other reagents, and brought out the festoons still more distinctly. But it moreover enabled me to trace some of the tubules into the centre of the specimen, and showed them to be a continuation of vessels contained in the dark material which had floated out while the specimen was drying. But still more important, this reagent enabled me to detect *within* them distinctly liver cells, with their minute oil globules. I think there can be little doubt that these tubules are the radical secreting ducts of the liver disengaged from their attachments (or sloughed off) by that destruction of capillary tissue which I am now satisfied is the essential anatomical lesion in yellow fever.' May not these observations throw some light on what I believe is still an undecided point in anatomy—viz., the exact manner in which the bile radicles originate in the hepatic lobules? To me, what I have seen seems a demonstration of the induction of Kiernan on this point. It is true that only granules were visible in the sides of those tubules which had been floated out in the serosity; but may not these have been embryonic

cells? or may they not have been the markings of the site of detached cells—the desquamatory process being common to the epithelial surfaces in yellow fever?

Nolin died after four days' illness. He was unusually yellow for that space of time. After death, I found (rather an unusual occurrence in yellow fever) the gall-bladder nearly empty, and what was in it only a little pale pea-green mucus.

I believe I now understand the source and relations of that alvine evacuation in yellow fever which, in the last epidemic, has been named the "*caddy stool.*" It is generally liquid, like dirty water, with a grey, gritty sediment. This sediment under the microscope shows an abundance of crystalline material, chiefly triple phosphates or uric acid, or both; also, although invisible to the naked eye, numerous oblong plates, of a bright yellow colour, which I have latterly called *bile crystals.* But the colour of this stool is derived from innumerable little amorphous masses, granular in surface, and of a jet black colour. This last material I believe is *carbon.* I have detected this black material and the bile crystals in several thin sections of the liver, in those who have died of yellow fever in the present epidemic; and I therefore infer that this peculiar stool is derived from the liver. I have also noticed that this stool seems most common when the respiratory function is embarrassed—in the pulmonary form of yellow fever. On the 5th instant, a Portuguese boy, named V. de Cambra, died with black vomit, well marked, in the Colonial Hospital. This was an exceedingly interesting case, from many circumstances. He suffered so much in his respiration, that his lips were markedly livid. His dyspnœa and restlessness were so great, that no careful auscultation could be made. His blistered surfaces bled so profusely, that the discharge might properly be called *hæmorrhage from the skin.* Two hours before his death, I examined this blood, and found the corpuscles normal; and I washed a small clot which I took up with forceps from his blistered surface, and preserved it in Canada balsam as a proof of the integrity of the fibrine. This boy's liver and spleen are full of what seems to be identical with the carbonaceous particles of the caddy stool.

Summary of Dr. Blair's Views on Yellow Fever.—Since receiving the previous letters of Dr. Blair, we have been favoured with a communication from him, containing the following remarks, which will doubtless obtain that consideration from our readers which is due to any opinion expressed by so careful an observer:

George Town, Demerara, May 25th, 1856.

The proximate cause of the disease is an aërial poison which enters the system through the mucous membranes, on which it impinges, and to which it becomes attached. In some extreme cases, all the mucous linings of the eyes, nares, alvine passages, and bronchi, are poisoned at once; but more commonly, the first application of the poison is only partial. The primary and specific action of the poison is on the capillary vessels, and this action spreads and extends itself until the large viscera and bladder become affected in varying degrees. The mode of action of this poison on the capillaries is first as an irritant, and it ends by inducing a physical impairment of tissue. The consecutive lesions are, desquamation of epithelium, exfoliation of basement membrane, sloughing of the minute capillaries, and deep erosions. The symptoms are first those of an irritant poison, and afterwards are made up of the composite results of hæmorrhage and the circulation of blood, which has been contaminated by impaired functions of the excreting viscera.

The Nursing Scheme of the Epidemiological Society and the Poor-Law Board.

Our readers may remember that we have on a former occasion drawn attention to the labours of a committee of the Epidemiological Society, appointed nearly two years and a half ago, for the purpose of considering a plan, suggested to supply Nurses to the Labouring Classes in sickness, throughout England. Extensive in-

quiries among medical officers of unions, the masters of workhouses, and the clergy, were made, and led to the final adoption by the committee of the proposition for qualifying female inmates of workhouses to act as nurses. In order to carry it into effect they proposed—

I. That the master and matron of every workhouse shall give such female inmates a routine of occupation, that shall afford them a knowledge of the duties required in the management of the sick.

II. That the medical officer of each workhouse, as soon as he shall consider an inmate competent to undertake the nursing of the sick out of the workhouse, shall certify to that effect.

III. That a register shall be kept at the workhouse of all those who have been certified by the medical officer as qualified nurses, containing their names, ages, certificates, and addresses. This register shall be open to the medical profession, the clergy, and the public at large, as a ready means of obtaining a nurse suitable to their wants.

Although these propositions could be carried out in each union without an enlargement of the powers which the boards of guardians already possess, it was manifestly most desirable that the committee should obtain the sanction and aid of the central Poor-Law Board. At an earlier stage of their proceedings the Board had declined adopting the plan altogether. Certain modifications removed the features which were regarded as offering unnecessary difficulties, and the committee, headed by the Earl of Shaftesbury and Lord Stanley, M.P., in April last again waited upon the President of the Poor-Law Board, with a view to urging the propriety of a circular letter being issued by the Board to the different unions, recommending the adoption of the propositions of the committee.

The committee shortly after had the great satisfaction of receiving from Lord Courtenay, the secretary to the Poor-Law Board, a copy of a letter addressed to the Poor-law Inspectors, concerning the employment of the able-bodied female inmates of workhouses as Nurses; we have much pleasure in publishing this document:

Poor-Law Board, Whitehall, May 10th, 1856.

SIR,—Adverting to the circular memorandum which the Board addressed to you in February in 1855, in reference to a proposal made by the Epidemiological Society for the training in workhouses of nurses for the poor, and to your remarks upon it, I am directed by the Poor-Law Board to inform you, that their attention has been again directed to the subject, and that they think it desirable to communicate to you, for your guidance, the views which they now entertain respecting it.

The Board are of opinion that any attempt on their part to establish authoritatively in workhouses a general system of training for nurses, would be alike impracticable and inexpedient, and they communicated their opinion to the Secretary to the Epidemiological Society in March, 1855. At the same time, the Board think it not improbable that in large workhouses where a paid nurse is employed, it may sometimes be practicable to adopt a system under which such of the female inmates as may be trustworthy and competent for the work, may be employed in the infirmary and sick wards, not only with the object of acting as assistants to the paid nurses, but also with the view of their being taught by them the duties of a nurse in such a manner, as may subsequently enable them to support themselves by becoming nurses on their own independent account.

It is of course unnecessary for the Board to point out, that this species of employment must, however, be subject to the qualification, that no person should be employed in attendance on infectious cases without her free consent. If such a scheme were carried successfully into effect, it is thought that recourse would be frequently had to the workhouses where it was in operation, for nurses to attend the sick; and it is suggested that a register might be kept of the names and qualifications of those inmates who shall have been thus taught, and who are fit for such attendance.

The Board are accordingly desirous that some such plan should be suggested by you to any board of guardians within your district, in which the arrangements of the workhouse are, or may be made, such as to admit of its being carried into practical effect.

The Board further request that in bringing the subject under the notice of any board of guardians, you will not fail to state the strong sense which they entertain of the evils resulting from the want of a sufficient number of trained and efficient nurses for the poor, and their confidence that the guardians will be ready to concur in any plan by which, consistently with a sound system of poor-law administration, and with the laws regulating the expenditure of the poor rate, their number may be increased.

I am, &c.,
(*Signed*) COURTENAY, *Secretary.*

It will be observed that, although the Poor-Law Board in the foregoing letter in the main adopt the principles of the committee, the Board do not make the introduction of the plan imperative upon the different unions, but leave it to the discretion of the Guardians. It would be most desirable that the medical officers of all unions should interest themselves in the plan proposed by the committee, who, we are given to understand, are now issuing printed forms of the certificates and register, with an outline of the qualifications, according to which the certificates might be awarded to the trained nurses, to the different unions; these forms have been maturely considered by the committee, and are offered as suggestions to those who may not hitherto have devoted any consideration to the subject.*

The Trial of William Palmer.

THERE has probably never been a medico-legal investigation which has so entirely absorbed the interest of all classes of society, as the trial which not long since ended in the conviction of William Palmer. Whether in regard to its bearing upon medical and chemical science, or to the relation between direct and circumstantial evidence, or whether in reference to our social and family ties and all those bonds which knit together a civilized community, this celebrated trial demands our most serious consideration. It is not our intention now to enter more fully into the various topics which force themselves upon the mind in connexion with scientific questions that have arisen in the course of the investigation, and that are still occupying the minds of men. We merely allude to it as a matter too momentous, whether regarded as a sign of the times generally, or as a landmark for the history of forensic medicine, to be passed over in silence. We hope to have an early opportunity of discussing the more prominent points that may appear to merit the consideration of our readers.

Report on the Pathology of the Diseases of the East.—London, 1856.

THE fruits which science and, we trust, humanity will gather from the experience of the late war, are beginning to ripen. One of the first that will claim our attention is the pathological Report drawn up by Dr. Lyons, a civil medical officer, who was specially appointed by Lord Panmure, in April, 1855, to institute researches into the morbid changes exhibited in the bodies of those of our soldiers who fell victims to the diseases that decimated our army in the East. The Report has only reached us at the last moment, so that it would be impossible to express an opinion upon it already. Besides, we are informed that a similar Report may be expected from our medical brethren of the army; and it will doubtless be of

* Any information or papers relating to the plan, may be obtained of the secretaries to the committee, 13, Upper Brook-street, Grosvenor-square, London.

great interest to compare the two, and extract from them conjointly those points which may be expected more particularly to interest our readers.

We perceive that Dr. Lyons states that he arrived at Scutari at the close of April, 1855, when he found "that all but the expiring embers of the terrible epidemics of the past winter had disappeared. To the past (he continues) no methods of pathological research are applicable; and to have been enabled to avail myself of the almost unparalleled opportunities for investigating the nature of disease, which had unhappily been presented at Scutari, I should have been in the East not less than fully two months earlier than the date at which my mission commenced." Moreover, it appears that Dr. Lyons laboured under other disadvantages, which somewhat embarrassed him, and caused a further loss of time; for not until the expiration of more than two-thirds of the period originally assigned for his inquiries, were he and his assistants put in possession of the necessary instruments and appliances for the due prosecution of their labours.

Dr. Lyons was assisted by Doctors Aitken and Doyle, the former of whom, as first assistant-pathologist, has also signed the Report, so that the responsibility appears to be divided between the two gentlemen. After spending some time in the investigation of the disease at Scutari, the due and complete appreciation of the pathological characters of the diseases of the army in the East rendered it necessary for Dr. Lyons to move his quarters to the Crimea.

From a cursory glance at his statement with regard to the causation of the diseases that so much impaired the strength of our army, it appears that Dr. Lyons confirms the opinion that the causes were essentially of a character that might have been avoided or removed, and that they were not inherent in the soil of the country. "It is not," he says, "to be inferred that the climate of the Littoral of the Crimea, on which the Allied armies were encamped, is naturally an unhealthy one. On the contrary, there are some very good grounds for quite an opposite opinion. Considerable immunity has been enjoyed by the Allied troops from more than one form of disease which elsewhere has caused extensive ravages amongst forces in the field. Thus, ophthalmic disease has been almost unknown; lesions of the respiratory organs have been of unusual occurrence; and, with the exception of certain marshy lands on the borders of the Tchernaya, intermittent fevers seemed to have no habitat within the lines occupied by the besieging armies. The more recent experience in the English camps seems very clearly to establish, that, with the realization of the other necessary conditions, respecting moderate duties and fatigues, proper and abundant food, suitable clothing, and protection against weather, the climate of the southern shore of the Crimea is such as to favour the maintenance of a very excellent and satisfactory state of health, even amongst very large masses of troops. That an opposite state of things is in great part, if not wholly, due to causes which are probably *not* climatic, may be deduced from a consideration of the much less healthy condition of other troops in the same region of the Crimea."

The instructions under which Dr. Lyons proceeded to the East are conceived in a very comprehensive spirit, and are highly creditable to the physician who may have drawn them up; for though signed by Lord Panmure, the subject is scarcely one to which his lordship can be supposed to have devoted any personal attention. The document is itself an interesting one, and belongs to the medical history of the expedition. For these reasons we give it entire.

"1. You will proceed without delay to Scutari, and report yourself, on your arrival, to the commandant, Lord William Paulet, who will be apprised of the objects of your mission, and instructed to place you in communication with the principal medical officers of the hospitals at Scutari.

"2. You will have the entire and uncontrolled direction of the post-mortem researches on the bodies consigned to you for examination; but you will be required to demonstrate the morbid appearances discovered to such of the medical officers as may feel disposed to attend. In order that these gentlemen

may have the full advantage of your pathological researches, you will arrange with the principal medical officer of the hospital as to the time which will be most convenient to the medical officers to attend your demonstrations.

"3. You will have under your directions two able assistants (one first, and one second), and both accustomed to and versed in the operations of the dead-house.

"4. As morbid anatomy is of little value unless studied in connexion with the history of the disease, you and your assistants will require to visit the hospital wards, in order to become acquainted with the symptoms and characters of the diseases during their progress; but you will not interfere with the treatment of the patients. In making notes of the cases, the name of the patient, and the number of his regiment, should always be stated, as by this means the nature of his duties, and the place where he was first attacked by disease, can be more easily ascertained.

"5. The medical officers of the hospitals will be instructed by the principal medical officers to afford you every facility in visiting the wards; and it is hoped that they will be able to supply you with notes of the symptoms and progress of the more urgent cases.

"6. It is expected that you will not content yourself with the simple dissection of the subject, and the demonstration of the morbid parts, but that you will submit them to microscopical examination.

"7. As you are provided with all the appliances necessary for your researches, it is expected that you will take full advantage of the opportunities which present themselves to prosecute your inquiries in this direction to the fullest extent; and as you are also provided with the means of preserving such specimens of disease as you may deem necessary for the illustration of your researches, you will naturally avail yourself of them, and take the necessary steps for having them transported to England.

"8. Although in all probability you will find at Scutari ample opportunities of completing your pathological observations, you are not to confine yourself entirely to the hospitals in that place; should the information which you receive from reliable sources lead you to believe that more favourable opportunities present themselves in the hospitals at Kululee, or elsewhere, on the Bosphorus, you may use your own discretion in transferring your researches for a time to such hospitals, acquainting the commandant of your intention, and the superintendent of the hospital you propose to visit.

"9. With the view of rendering your researches more complete, it is desirable that you should observe the nature of the diseases of the Russian prisoners; and it would further be desirable that you should visit the French Hospitals, and ascertain the results of the researches of the French physicians in the diseases of their sick.

"10. If, after having completed your researches in the hospitals on the Bosphorus, you should be of opinion that you might acquire additional information, to render your researches more complete and useful, by observing the character and effects of the diseases prevalent in their earlier stages and more acute forms in the Crimea, you are at liberty to apply to Lord William Paulet for a passage for yourself and one or both of your assistants and labourers to Balaclava; upon obtaining his approval, you will, upon your arrival, report yourself and explain the object of your mission to the head of the medical department of the army in the Crimea.

"11. Should your health unfortunately suffer so as to render you unable to continue your researches, you are to commit the charge of carrying them out to your first assistant, giving him these instructions as his guide; and if he should require further assistance, application may be made to the principal medical officer at Scutari, or elsewhere, for such assistance, or to the superintendent of any of the civil hospitals in the East.

"12. In the event of either or both of your assistants being unable, from

sickness, to render you the assistance you require for the successful prosecution of your researches, you are to apply to the principal medical officer of the hospital where you may be for one or more assistants, or to the superintendent of the civil hospitals in the East.

"13. If, unfortunately, both yourself and your senior assistant should be attacked by sickness, the circumstance should be immediately communicated to the principal medical officer at Scutari or elsewhere, or to Dr. Parkes, superintendent of the civil hospital on the Bosphorus, who has one assistant-physician at least capable of carrying out the pathological researches on which you will be engaged.

"14. As the office to which you are appointed is new in the medical department of the army, it is possible, although not probable, that some difficulties may arise in the prosecution of your researches. If, by your own prudence and conciliatory conduct, you fail to overcome any such difficulties, you will apply to the commandant of the hospital at Scutari or elsewhere; and should he not afford you the support which you require, you will report the circumstances to the Secretary of State for the War Department with as little delay as possible. You will, however, under any circumstances, report from time to time to the Secretary of State for the War Department the progress made in your researches.

"15. When you shall have completed these researches, you will draw up a full report thereof, for the information of the Secretary of State for the War Department, to whom it should be addressed.

"16. Although not strictly limited to time, it is presumed that a period of about four months will be sufficient to enable you to accomplish the object of your mission; as soon as you have so accomplished it, you will return to England without delay, in order to present the report of your researches to the War Department, it being important that no time should be lost in making known this report, as it may be the means of elucidating the nature of the diseases affecting the army in the East.

"17. On application to the commandant at Scutari, a passage will be afforded you in one of the first Government vessels returning to England; and immediately on your arrival you will report the same to the Secretary of State for the War Department.

"18. Should one or both of your assistants desire to remain in the East, and should it prove that his or their services are required, the superintendent of the civil hospitals will be authorized to employ his or their services, if he should think fit to do so. If, on the contrary, either or both should prefer returning to England with you, a passage will be afforded to him or them on application to the commandant. The same option, with the approval of the principal medical officer, or superintendent of any civil hospital, may be given to your two English porters, if willing to take the duties of orderlies, or if it be found that they can otherwise usefully be employed.

(Signed) "PANMURE.

"London, April 27th, 1855."

BOOKS RECEIVED FOR REVIEW.

An Account of a Mode of Treating the Small-Pox. By Henry George, M.R.C.S.L. Fourth Edition. London, 1856. pp. 116.

The Education of the Imbecile and the Improvement of Invalid Youth. Edinburgh, 1856. pp. 20.

Reply to Dr. M'Gilchrist's 'Remarks' on Professor Bennett's Introductory Lecture. By John Glen, M.A. Edinburgh, 1856. pp. 28.

Throat Ailments, more especially the Enlarged Tonsil and Elongated Uvula. By Thomas Yearsley, M.R.C.S. Sixth Edition. London, 1856. pp. 112.

Report by the Royal College of Surgeons of Edinburgh, on the Bill brought into Parliament by Mr. Headlam, "To alter and amend the laws regulating the medical profession."

Surgical Anatomy. By Joseph Maclise, F.R.C.S. Second Edition. Fasciculus XI.

Medical Anatomy. By Francis Sibson, M.D., F.R.S., Physician to St. Mary's Hospital. Fasciculus IV.

A Description of Improved Hollow Bricks and Brickwork, intended to facilitate the Ventilation of Rooms.

Cell Therapeutics. By William Addison, M.D., F.R.S. London, 1856. pp. 84.

Sixteenth Annual Report of the Registrar-General of Births, Deaths, and Marriages in England. London, 1856.

Reports on Mr. Hooper's Improved Hydrostatic Beds. London, 1856.

A Short Essay upon the Qualifications, Duties, and Remuneration of Union Medical Officers. By Robert Knaggs, of Swindon. London, 1856.

On the Study of Medicine. The Introductory Address at Surgeons' Hall, Session 1855-56. By John Struthers, M.D. Edinb., 1856.

The Asylum Journal of Medical Science. Edited by J. C. Bucknill, M.D. April, 1856.

On the Constitutional and Local Effects of Disease of the Supra-Renal Capsules. By Thomas Addison, M.D. London, 1856. pp. 39. Plates.

Annual Report of the Trustees and Superintendent of the State Lunatic Asylum of Pennsylvania, located at Harrisburg. For 1854. Harrisburg, 1855.

Diseases of the Heart, their Pathology, Diagnosis, and Treatment. By W. O. Markham, M.D. London, 1856. pp. 340.

Clinical Lectures on Surgery. By M. Nélaton. From Notes taken by Walter F. Atlee, M.D. Philadelphia, 1855. pp. 755.

An Inquiry into the Nature of the Simple Bodies of Chemistry. By David Low, Esq., F.R.S.E. Third Edition. Edinburgh. pp. 386.

Sull uso della gommi Resina d'Asafetida. Memoria del Dr. G. La Ferla. Malta, 1855. pp. 29.

A Report of some Cases of Operation. By George M. Humphrey, Esq. London, 1856. pp. 67. (Reprint.)

Letters to a young Physician first entering upon Practice. By James Jackson, M.D., &c. Boston, 1855. pp. 344.

The Hospital System of London. (Reprint.) pp. 53.

Medical Selection of Lives for Assurance. By William Brinton, M.D. London, 1856. pp. 58.

A Classification of Wounds and Injuries received in Action, proposed for use in Military Hospitals. By J. R. Taylor, C.B. 1856.

The Assurance Magazine. No. XXIII. April, 1856.

Observations on the Operative Measures necessary for the Treatment of Hare-lip. By Richard G. H. Butcher, Esq. Dublin, 1856.

On Poisoning by the Root of Aconitum Napellus. By J. W. Headland, M.D. London, 1856. (Reprint.)

Records of Obstetric Consultation Practice, and a Translation of Busch and Moser on Uterine Hæmorrhage. By Edward Copeman, M.D. London, 1856. pp. 223.

Die Ohrenheilkunde in den Jahren 1851-55. Von Dr. W. Kramer. Berlin, 1856. pp. 136.

A Report on the Diet of the Sick, submitted to the American Medical Association. By Charles Hooker, M.D. Philadelphia, 1855. (Reprint.)

A Paper on the Effects of Lead on the Heart. By J. W. Corson, M.D. (Reprint.)

An Essay on Intermittent and Bilious Remittent Fevers, with their Pathological Relation to Ozone. By E. S. Gaillard, M.D. Charlestown, 1856.

Die Krankheiten des Auges für Praktische Aerzte. Von Dr. Ferd. Arlt. Band III. Abth. 2. Prague, 1856.

An Oration delivered before the Hunterian Society, February 13th, 1856. By Thomas Callaway. pp. 29.

Report of the Recent Yellow Fever Epidemic of British Guiana. By Daniel Blair, M.D. pp. 91. (Reprint.)

A Manual of Midwives and Monthly Nurses. Dublin. pp. 170.

On the Medicinal Properties of the Mineral Waters of Vichy. London, 1856. pp. 27.

Progress of Discovery in the Nervous System and in Medical Ethics. pp. 4. (Reprint.)

On the Nature of Cholera as a Guide to Treatment. By William Sedgwick. London, 1856. pp. 182.

Surgical Reports and Miscellaneous Papers on Medical Subjects. By George Hayward, M.D. Boston, 1855. pp. 452.

Beiträge zur Pathologie des Auges. Von Eduard Jäger. Zweite Lieferung. Wien 1855.

Essays on State Medicine. By H. W. Rumsey. London, 1856. pp. 424.

Memoirs of Cholera at Oxford in the year 1854. By H. W. Acland, M.D. London, 1856. pp. 172.

Lectures on the Diseases of Women. By Charles West, M.D. Part I., Diseases of the Uterus. London, 1856. pp. 413

The Obstetric Memoirs and Contributions of James Y. Simpson, M.D. Edited by W. O. Priestley, M.D., and Horatio R. Storer, M.D. Vol. II. Edinburgh, 1856.

Physical Exploration and Diagnosis of Diseases affecting the Respiratory Organs. By Austin Hunt, M.D. Philadelphia, 1856. pp. 636.

Report of the Pennsylvanian Hospital for the Insane, for the year 1855. By Thomas S. Kirkbride, M.D. Philadelphia, 1856.

Physicians and Physic. Three Addresses. By J. Y. Simpson, M.D. Edinburgh, 1856. pp. 133.

The American Journal of Insanity, April, 1856.

A Handbook of Organic Chemistry for the Use of Students. By W. Gregory, M.D. Fourth Edition, corrected and much extended. London, 1856. pp. 627.

Norsk Magazin for Lägevidenskaben. Bind IX., Heft 12; Bind X., Hefte 1, 2, 3.

Om Uterin Deviationen. Af. Dr. F. C. Faye. Christiania, 1856. (Reprint.)

Födselsstiftelsen i Christiania i Aarene, 1853, og 1854. Ved Overlæge Dr. Faye.

Gesammelte Abhandlungen zur Wissenschaftlichen Medicin. Von Rudolph Virchow. Zweite Halfter. Frankfurt, 1856.

Hospitals Meddelelsen. Anden Räkke. Udgivet Af C. E. Fenger, Dr. Med. Förste Bind. Kiobenhavn, 1856. pp. 576.

Lectures on Clinical Medicine. By John H. Bennett, M.D. Nos. IX. and X. Edinburgh, 1856.

Practical Remarks on some Points in the Physical Diagnosis of Phthisis Pulmonalis. By J. H. Bennett, M.D. Edinburgh, 1856. pp. 37.

Annual Report of the Royal Edinburgh Asylum for the Insane, for the year 1855.

On the Defects with reference to the Plan of Construction and Ventilation of most of our Hospitals for the Reception of the Sick and Wounded. By John Robertson. pp. 16. (Reprint.)

Shaw's Medical Remembrancer. Re-written and much enlarged by Jonathan Hutchinson. London, 1856.

A Treatise on Removable and Mitigable Causes of Death. By Norman Chevers, M.D. Vol. I. 1852. pp. 329.

A Manual of Medical Jurisprudence for Bengal and the North-western Provinces. By Norman Chevers, M.D. Published by Order of Government. Calcutta, 1856. pp. 608.

Clinical Researches on Diseases in India. By Charles Morehead, M.D. Two Vols. pp. 687, pp. 766. London, 1856.

History of Medicine, from its Origin to the Nineteenth Century. By P. V. Renouard, M.D. Translated from the French by C. G. Comegys, M.D. London, 1856. pp. 719.

Asiatic Cholera, an Inquiry into its Nature, and how to deal with it. A Popular Exposition by Robert Pavlman. Edinburgh, 1856. pp. 56.

On Calculous Disease and its Consequences, being the Croonian Lectures for the year 1856, delivered before the Royal College of Physicians. By G. O. Rees, M.D. London, 1856. pp. 81.

Compendium der Chirurgischen Operationslehre. Von Dr. Wenzel Linhart. Wien, 1856. Zweiter Theil. pp. 379–752.

Nomos; an Attempt to Demonstrate a Physical Law in Nature. London, 1856. pp. 198.

Life, its Nature, Varieties, and Phenomena; also, Times and Seasons. By Leo H. Grindon, Lecturer on Botany. London, pp. 328.

Index Morborum Internorum Systematicus. Præmissis de Nosogenesi Aphorismis. Auctore O. Baug. Hauniæ, 1855.

Recherches expérimentales sur les Voies de Transmission des Impressions Sensitives. Par M. le Docteur Brown-Séquard. Paris, 1856. pp. 38.

CONTENTS OF No. XXXVI.

OF THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1856.

Analytical and Critical Reviews.

PAGE

§

Bibliographical Record.

Original Communications.

Chronicle of Medical Science.

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1856.

PART FIRST.

Analytical and Critical Reviews.

Review I.

1. *Report of the Sanitary Commission of New Orleans on the Epidemic Yellow Fever of* 1853. Published by Authority of the Council of New Orleans.—*New Orleans*, 1854. pp. 542.
2. *Yellow Fever, Considered in its Historical, Pathological, Etiological, and Therapeutical Relations.* By R. La Roche, M.D., Member of the American Philosophical Society, of the American Medical Association, Fellow of the College of Physicians of Philadelphia, &c.—*Philadelphia*, 1855. 2 vols. pp. 615. pp. 813.

In the wide range of science, there are, it might be supposed, few subjects which more deserve the serious consideration of humane and civilized governments than the causes which influence the spread of epidemic diseases; yet, strange to say, compared with other subjects of far less importance, they have received but little attention. There can be no better proof of this than the stolid indifference with which we regard the late cholera epidemic, which swept away some fifteen thousand of our fellow-creatures from this busy metropolis in the course of a few weeks. While it lasted, we taxed our energies to devise means to stay its progress, and to prevent its recurrence. Brooms and dust-carts were called into requisition, and masses of filth which had been deeply buried in the earth were exhumed and carried to a distance; the nauseous breath of drains and cesspools was stifled by lime, while the parochial authorities waged war against the impurities which for ages had poisoned the dwellings of the poor; but hardly had the last victim of the pestilence been consigned to the grave, when we relapsed into our former state of indifference. Filth of every description has again been allowed to accumulate, while the poor from their washing have returned to wallow in the mire which is almost inseparable from their humble

condition. When a ship founders at sea, or when a train runs off the line and imperils the lives of the passengers, the country cries aloud for an inquiry, and the Government are compelled to grant it. The causes which led to the disaster are examined, and new laws made to prevent the recurrence of any similar accident; but epidemics which destroy thousands and tens of thousands in the course of a few weeks or months, are allowed to pass unnoticed, as if the question of their origin was of less importance than the smoking of a London chimney! This is greatly to be deplored, for although we must confess our ignorance with respect to the real nature of the agencies or germs by which exanthematous and other contagious diseases are produced and propagated, we are well acquainted with their modes of action—we know that their efficiency is reduced or destroyed by dilution or diffusion, by distance and by time. We are thus, by adopting proper precautionary measures, enabled to control the spread of these epidemics at home, and to prevent their extension into lands beyond the sea. This we believe will not be disputed. There are many, however, who are unwilling to admit that the march of cholera and yellow fever may be thus arrested, though the eruption and extension of these maladies obey laws that are identical with those which influence the origin and spread of the exanthemata.

After all that has been written on yellow fever, it could hardly be expected that the writers of the above works would be able to throw much additional light on the subject. The ponderous work of Dr. La Roche contains what was much wanted—a chronological digest of the opinions of most of the principal writers on yellow fever, down to the present time, and we are bound to say he has performed his task with as much fairness as we had any right to expect from a partisan writer. The second book consists of four reports and a mass of testimonial evidence collected from various sources by the Sanitary Commissioners of New Orleans. The first, On the Sanitary Condition of the City, is by Dr. Barton; the second, On the Sewerage of New Orleans, by Dr. Riddell; the third, On the Origin and Spread of the Epidemic in 1853, by Drs. Anson and McNeil; and the fourth, On Quarantine, by Dr. Simonds. To these gentlemen the duty of investigating the subjects referred to appears to have been delegated by the municipal authorities.

From Drs. La Roche and Barton we have a long description of the physical condition of their respective cities. According to the former—

"Philadelphia in its totality is one of the neatest and cleanest towns in America; the streets are wide and well paved. Nevertheless there are in other parts of the town a number of lanes and courts, which are inhabited by blacks and the poorer classes, and often present a melancholy picture of filth and wretchedness. These, as in other towns, have always been infested with yellow fever. . . . . In this almost isolated neighbourhood we find an excess of vagrant population, half fed and half clothed, crowded together in almost untenable houses and crowded cellars. In this forbidding district the half-famished and bloated bodies of a depraved and mixed population, whose constitutions have been undermined through the ravages of intemperance and exposure, the accumulation of filth, and impure food and air, become the fit receptacles, and afford the materials for the growth and reproduction of the morbific germs which produce disease."*

New Orleans in its "totality," on the contrary, is one of the dirtiest,

* La Roche, vol. i. p. 8.

and consequently the sickliest, city in the Union—at least, so says Dr. Barton. It is not only the sickliest city in America, but in the whole civilized world; while the municipality, if we are to believe the same authority, are even a more obstinate, pig-headed race of men than the much-abused rulers of another city on this side of the Atlantic.

Philadelphia appears to have been visited at distant periods by many yellow fever epidemics.

"Scarcely had a few hundred families from the mother country clustered together and provided themselves with comfortable dwellings, before an epidemic fever broke out among them, spreading desolation all around, and producing a mortality fully equal to any that has occurred at future times. The city was then about seventeen years old, and was little more in point of extent than an ordinary country town. Houses were scattered in various directions about the plot, and the creeks, pools, and swamps were such as nature had made them."*

There was then no excess of vagrant population, half-fed and half-clothed, crowded together in narrow streets and pent-up courts, badly ventilated and lighted, offering a fit pabulum for the disease to feed on. Neither were there accumulations of filth or foul drains to give rise to it. Yet it raged as fatally as it did in later years, when the city became filled with every kind of abomination. The chroniclers of this epidemic are silent with respect to its cause; and as it did not extend into the country, Dr. La Roche, for once, wisely abstains from assuming that "*there must have been an epidemic condition of the atmosphere;*" "and nowhere," he says, "so far as I have been able to ascertain, do we find a suggestion that any vessel had arrived from the West Indies in a condition calculated to infect the city;" yet, on the same page, he admits that one Pemberton had positively stated, on information from his father, that the disease was imported in a ship from the island of Barbadoes. The cargo consisted of cotton bags, which were landed on a wharf, and there stored for sale.

"The correctness of the inference," he adds, "will not acquire probability when the reader is told that Pemberton's father was only fifteen years of age at the time of the calamity. Though he was capable of making his remarks on occurrences, and afterwards remembering them, he knew it to be the invariable judgment of the physicians and other citizens that the disease was introduced among them in the manner narrated above, attributing it to no other visible cause."†

For forty years afterwards, yellow fever did not again make its appearance in Philadelphia; but in 1741 it was, according to Lind, introduced by a trunk of wearing apparel belonging to a gentleman who died of the fever at Barbadoes. There was an epidemic in 1747, which was supposed to have been introduced by a vessel from the West Indies; and again in 1762 the disease prevailed in a more intense form. The few writers who have spoken of the latter, ascribe its origin to a foreign source; "for the time had not yet come when it could be admitted to originate from the baneful action of foul exhalations." After an interval of thirty-one years it again broke out with great severity, and "sent thousands to the grave. Nothing, so far as the weather and temperature are concerned, seemed, in the minds of the physicians or the public, to portend the occurrence of a calamity so dreadful;" but Dr. La Roche tells us that "observations collected in subsequent years, and in other places, have taught us to connect the development of yellow fever with the existence of atmospheric peculiarities and conditions of the localities such as were noted at the time."

* La Roche, vol. i. p. 16. † Ibid. p. 53.

Whether the same care was taken to ascertain the atmospheric peculiarities which took place during the preceding thirty years, when the disease did not exist, we are not informed. It is, however, rather amusing to read over the peculiarities so industriously collected by Dr. La Roche. The weather, he says, was sometimes calm, sometimes unusually hot; and there was something (which is not described) in the heat and drought that was uncommon:

"The wharves and docks were more or less filthy; on one of the former there was a quantity of damaged coffee deposited—its smell was highly putrid. Mumps had prevailed, and scarlatina anginosa had also appeared. Peremptory orders were issued to have the streets properly cleaned and purified, and the filth removed. Still, long after this, the disease continued to spread until it prevailed in most quarters of the city, every street appearing to be charged with the miasmata. The inhabitants residing in densely-populated close alleys were the greatest sufferers."

This, most assuredly, would not lead us to infer that the disease sprang from the effluvia arising from putrid coffee and other matters, but rather that it crept from one house to another, and to street after street, by the extension of the exciting cause successively developed in those attacked, until, in the course of about three months, it extended over all the more densely-populated parts of the town, and swept away upwards of 4000 of the inhabitants. That a catastrophe so horrible should be ascribed to an atmospheric cause which has never yet been discovered—to a concourse of circumstances neither described nor understood—or even to filth of the worst kind, are theories which we are unable to adopt.

In 1794, the disease was introduced by a vessel from St. Mark. In 1795 and 1796, there were sporadic cases. In 1797, the fever was more general, but less fatal, than in 1793. The College of Physicians thought it was imported. The non-contagionists, on the other hand, ascribed it to local impurities, though it did not appear in some of the worst parts of the town, near which the foul drains of the city empty themselves.

"As on former occasions, the members of the medical profession suffered severely: out of three- or four-and-twenty who had attended patients ill of the disease, sixteen were attacked, and nine died."

In the following year there was an epidemic, which, in point of malignity and mortality, was the most extensive of any that had yet occurred in the city:

"It afforded the last great manifestation of the epidemic constitution of the atmosphere. There was plentiful rain at one time of the year, and a snow-storm about the middle of April. The intense heat of summer came on suddenly in May; the thermometer rose as high as 84°; but this spell was short. In June, the weather was warm, though variable; in July, the heat became considerable; throughout August and September, the weather was marked by high temperature: but on the whole, it does not appear that the mean temperature for the warm months was more than a degree or so higher than it had been since the days the city was founded; and the extreme heat at any time not greater than on other seasons, both at Philadelphia and at other localities in the same region, and elsewhere, where yellow fever has never been known to exist."

It is utterly inconceivable how a man of Dr. La Roche's good sense could, on such slender grounds, venture to impute to these salutary changes, which are common to every region within or bordering on the

Tropics, the power of originating an epidemic. A conclusion so illogical —irrational, we had almost said—is discreditable to medical science and damaging to the cause of truth. The consecutive eruption of the disease in one part of the town after another, shows that it depended on causes totally distinct from meteoric changes; for they could not have been dissimilar in streets and districts contiguous to each other: "It began about Spruce and Walnut-streets, early in August, but before the close of the month spread to nearly every part of the city." On this occasion, and this only, the disease penetrated into the gaol, situate at the corner of Sixth and Walnut-streets. In a population of 60,000 inhabitants, 4868 were attacked; and of these, 3645 fell victims to the malady. In 1802, 1803, and 1805, there were epidemics, and the usual differences respecting their origin were reiterated. The non-contagionists, to account for the last, pointed to a bed of putrid oysters, which emitted a stench from June to the end of July, when they were removed or destroyed, but the fever did not cease until the cold weather set in.

Sporadic cases occurred during 1806-7-8 and 1809, but after the last-mentioned year, the fever entirely disappeared. In 1812, however, typhus of a malignant character usurped its place; many of the citizens fell victims to it, and, amongst others, the illustrious Rush. Whether this malady was supposed to have originated from the same causes which gave birth to the yellow pestilence, we are not informed, but its communicability was not doubted; and its progress from person to person and from place to place was, in every respect, identical with the latter. During the next fourteen years, many of the populous towns of the Union suffered, but the fever did not again appear in Philadelphia until 1820. Jackson, and others of the non-contagion school, ascribed this invasion to a wet spring and the filthy condition of the town; while the contagionists traced its origin to a vessel which came from St. Jago de Cuba.

A third of a century now passed away before the pestilence again made its appearance; but in 1853, "during the prevalence of an epidemic condition of the atmosphere, which," according to Dr. La Roche, "extended over all the West Indian islands—Launceston, New Orleans, Mobile, Natchez, Vicksburg, and even Brandywine"—wherever the disease appeared, in fact, "it broke out in our midst, and attacked a considerable number of individuals, of whom about 128 died."

Its origin was, by some individuals, ascribed to unwholesome gases arising from putrid masses of animal and vegetable filth; while others, who could not, or would not, admit that any combination of gases had the power of producing yellow fever, traced it to the barque *Mandarin*, which had arrived from Cuba. Such is a brief history of the epidemics of Philadelphia. It will be observed that, with hardly an exception, each epidemic was preceded by the arrival of one or more vessels from an infected port, and to these arrivals the contagionists ascribed the introduction of the disease, which subsequently spread from the ship or person in connexion with which or whom it had been imported; while the non-contagionists were never at a loss to find out accumulations of filth, which, in connexion with an assumed epidemic condition of the air, they considered amply sufficient to account for its origin and also, we must presume, for its continuance for months afterwards, notwith-

standing the removal of filth, and the continual changes which took place in the atmosphere.

From Dr. Barton's Report we do not glean much that is interesting respecting the epidemic which prevailed in New Orleans in 1853. He also attaches great importance to meteorological—if not to astrological—phenomena, as originating causes of the malady; hence he has paid great attention to the state of the weather, the oscillations of the thermometer and barometer, the dew-point, the amount of rain, the winds, and what he terms "solar radiation." February, March, April, and May, so far as we can judge, were not marked by any unusual or unseasonable meteorological occurrence; but he affirms that

"The high combination of heat and moisture with so small a precipitation, together with a remarkable elevation of solar radiation, so early even as January, assured me that the climatic influences were very remarkable; and when I saw the filthy condition in which the city was—the great extent of the exposure of the original soil of the city, for gas, water, and other purposes—the digging of the Carondolet basin, the cleansing of the canals, and the embankments and excavations for railroad purposes; and the reflection on the fatal consequences that these had heretofore always brought on our city, with the chart A. before me,—this early connexion of the atmospheric element with the physical showed in the combination a foreshadow of what was to come, and enabled me to give a warning as early as the middle of May, in the Academy of Sciences in this city, of the disastrous consequences that were to follow, and to some scientific correspondents."*

We give the above passage as it is printed, as we are at a loss to discover the writer's meaning. That a high combination of heat and moisture, with so small a precipitation (of heat and moisture?), together with the elevation of solar radiation occurring in June, enabled Dr. Barton to predict an eruption of yellow fever in July, implies an amount of prophetic wisdom which we cannot accord him; from the state of the weather in January, he could no more predict an epidemic invasion of yellow fever in June, than he could, from the same premises, predict an eruption of Mount Vesuvius on the year following.

The Report on the Origin and Spread of the Epidemic, by Drs. Anson and McNeil, contained in the Report of the Sanitary Commission, is a cleverly written paper. Like Dr. Barton—though in more intelligible language—they endeavour to trace the fever to domestic sources. They say the first cases appear to have been contracted amongst the shipping, though other cases occurred shortly afterwards in three separate localities in the town. These latter cases, the writers think, show the presence of some general and wide-spread agencies operating in that locality, and repeating at different and distant places similar phenomena to those transpiring in the vessels.

"These local influences, it would seem, were more concentrated in one section than in others, and by their action induced a condition which might be properly called one of morbific atmospheric saturation. The locality retained this bad pre-eminence, though it cannot fail to be noticed that the fever seemed to spread thence to other sections of the city. Whether it extended by the migration of certain atmospheric influences, or through the agency of the sick, our evidence is most conflicting. If we look at the epidemic in its totality, at the wide-spread surface of country over which it was diffused, the mind cannot fail to discover a conformity to the well-known habits of epidemic diseases. Nothing that we

* Report, p. 231.

know of the most virulent contagions will favour the opinion that they can, in so short a space of time, commence, progress, culminate, and decline over an extent of country so diverse and separate as that which fell subject to the dominion of our late pestilence."*

Dr. La Roche, on the other hand, as we have elsewhere noticed, considers that the disease being generally confined within narrow limits, is a proof of its not being communicable by personal emanations.

Let us now for a moment glance at the Report on Quarantine by Dr. Simonds, also included in the Report of the Sanitary Commission. The author remarks, that—

"Whatever be thought of the contagiousness of yellow fever in general, or as it has prevailed, here or in other places at various times, it would appear to admit of no doubt, that the epidemic of 1853 was carried by the regular course of travel to the interior. Its progress was steadily directed to points of more direct commercial intercourse throughout the south-west; and it appears that, having once obtained a fast hold in any locality, this served as a new focus, from which it was still farther diffused. It does not appear to have followed any of the known laws of the diffusion of gases, nor to have established any other law of diffusion than that above indicated."

He next refers to the city being dependent on commerce for its existence, and the danger of reporting the presence of malignant disease; for the press, the news-boys, hotel-keepers, and merchants, take the alarm in decrying any one who will dare to announce a case of yellow fever. It is not, therefore, surprising that physicians should hesitate to record early cases; "the only wonder is, that any can be found daring enough to face all these influences with an honest expression of opinion." We fear there is too much truth in this, and that our transatlantic brethren, with all their boasted liberty, are but too frequently compelled to regulate their views and opinions according to the will of a domineering democracy.

On referring to the reports respecting the shipping, we find that a steamboat took the ships *Augusta* and *Camboden Castle* in tow at the mouth of the river; the former was direct from Jamaica, where she had lost seven of her crew by yellow fever; the latter was from Bremen. While coming up the river, the steamer being between them, there was free intercourse between the crews and passengers of both vessels. On the 17th of May, the *Camboden* was anchored near the *Niagara*, *Saxon*, and *Harvest Queen;* while the *Augusta* was carried higher up, and placed near the *Northampton*. On the 23rd of May, an indisputable case broke out in the latter. Shortly afterwards, cases occurred in the three vessels lying near the *Camboden;* and a man named Hart, who had been employed in the *Northampton*, sickened about the 30th of May, and died on the 10th of June. His skin turned yellow; but the absence of black vomit led some to assert that he died of bilious pneumonia. Before going to the hospital, he lay sick in a house near the Mint, which immediately afterwards became a focus of infection. Another man, named Dorrell, after working for many days in the *Northampton*, was attacked in a different district; and in quick succession, five others living in the same house were seized. The fever then spread rapidly over the locality. These,

* Report, p. 493.

so far as we can make out, are the principal facts relative to the origin of the epidemic in New Orleans; but the attempts which have been made to conceal the first cases, and the unwarrantable assumptions relative to the influence of epidemic and other causes, render it difficult to arrive at the truth. The appearance of the disease, however, first amongst the men employed in the vessels which had communication with the *Camboden Castle;* and then, secondly, on shore, in the very spots where the labourers and others who contracted the disease in the vessels subsequently suffered, and not in other parts of the town, accord so well with previous observation, for instance, with the introduction of the fever into the islands of Ascension, Boa Vista, Fernando Po, and Goree, and also into Philadelphia, from the first down to the last epidemic, that we hardly believe it possible for any man who has the courage to think and write according to his own conscience and conviction, to deny that they do not afford strong and reasonable proof of the introduction of the fever by the shipping.

Dr. Barton is evidently well satisfied with the knowledge he possesses of the etiology of yellow fever. He supposes it to depend on the junction or meeting of two causes—the one atmospheric and the other terrestrial; and that it is in the power of man to control these agencies, and so prevent the evolution of the fever.

"If," he says, "the preventives and remedials we have recommended be seasonably and rigidly enforced, they will not only forestall and prevent yellow fever from originating here, but from propagating, should it be brought from abroad."

"Let me," he says, "be understood. I do not say that all the causes to which we assign the production of yellow fever can be forestalled in their coming, or expelled when they do come, by any human agency; for the meteorological conditions of elevated temperature, excessive saturation, great solar radiation, large precipitation, and prevalence of particular winds, or the absence of all winds, may not be entirely preventable or remediable by the art or the power of man. But great as the influence we attribute to the presence of these most deleterious and alarming agencies, we nowhere attribute, nor wish to attribute, to these agencies alone a capacity for originating or propagating that disease. It is only when they are in *combination* with those morbific influences which we have denominated *terrene* (which embrace every species of noxious affluvia which filth of every description and disturbance of the original soil generates and transmits), that the etiological conditions exist for the production and spread of the pestilence. The terrene condition alone is without the power to originate the disease in the absence of the meteorological conditions."

On this we would observe, that yellow fever has raged in towns and localities which were entirely free from every "species of noxious affluvia," or effluvia; while, in other places abounding in filth and corruption, it has never made its appearance. We may instance Sierra Leone, the islands of Ascension and Fernando Po, on the one hand; and the towns and cities on the estuary of the Canton and Yang-tse-Kiang rivers, on the other. In the former, we know, from personal observation, from the paucity of inhabitants, and the total absence of factories, that there never were, nor could have been, accumulations of filth; while in the latter, where the inhabitants are numerous and live closely-packed together, it is carefully stored up as one of their greatest treasures, yet yellow fever has raged with virulence in the former, but has never made its appearance in the latter.

In a chapter dedicated to Applicata, Excreta, &c., Dr. La Roche

hazards some remarks with respect to the influence of localities in producing yellow fever, with which we are unable to agree.

"It is impossible," he says, "to shut our eyes to the fact that the localization of yellow fever takes place only or principally where certain peculiar combinations of the materials appertaining to the soil, or which may have found their way there accidentally or otherwise, are discovered; that it takes place also in houses, rooms, yards, &c., where we meet with certain conditions of circumstances connected with the population, or where the objects by which they are surrounded, and which are known to be inimical to health, exist,—offensive effluvia, putrescent food, foul water, imperfect ventilation, besides some of the agencies already dwelt upon. Bearing this in mind, we arrive at once at the conclusion that the real cause, whatever it may be, meets there certain agencies which so modify the system as render it liable to the morbid impress. In a word, what may be regarded as the active and efficient cause of yellow fever may, after all, be but a predisposing agent."*

That offensive effluvia, putrid food, and bad ventilation are inimical to health, and predispose to disease, no one will deny; but we cannot admit that these are the only or principal conditions under which the localization of yellow fever takes place, or that there is any proof on record that it ever broke out spontaneously in any region or locality, however modified by conditions and circumstances, unless there had been either direct or indirect communication with another locality where the disease existed; and however great the predisposition may be which noxious effluvia and bad water or food impart to the system, no writer of respectability ought to hazard the opinion, that the active and efficient cause of the fever may, after all, be only a predisposing agent. It is to these groundless speculations, apparently advanced from a morbid desire to reason on subjects we do not understand, that the unsatisfactory state of the questions of quarantine and contagion is entirely to be attributed. Whether Dr. La Roche has any correct views with respect to the influence of what he calls the combinations of materials of the soil, or with respect to the certain conditions of circumstances connected with the population, we cannot discover; but it would be well if he and other writers of the same class would abstain from writing in ambiguous terms on questions which they do not and cannot comprehend.

"Yellow fever," we are told, "cannot be generated at a high elevation above the level of the sea, owing to the greater purity of the air, to a diminution of atmospheric pressure, and to more thorough ventilation. . . . . But the main cause is the absence of the degree of heat which is indispensably necessary for the elaboration of the morbific agent; for the same reason, in fact, its limits are restricted within certain bounds north and south by an excess of heat, and a variety of influences of a meteorological and telluric nature. . . . . Everywhere, a high range of thermometric heat has been found necessary to ensure its production, for whatever be the condition of the localities, the occurrence fails unless the temperature be high and continues to give a certain average during some weeks or months. . . . . The connexion of high atmospheric temperature with the prevalence of yellow fever is universally recognised, not as regards one portion of these regions alone—but all."

This may to a certain extent be true, but there is this remarkable distinction to be made—that a high temperature is not invariably connected with yellow fever. We have the same conditions of the soil,

* La Roche, vol. i. p. 86.

similar accumulations of the same kind of filth, with an atmosphere which is universally the same in all parts of the world, yet yellow fever, with the exception of the Island of Ascension, had not, until within the last few years, made its appearance on the south side of the Equator; though it is now, in consequence of the rapid and frequent communication by steam, and the increase and extension of commerce, gradually extending southward along both sides of the American Continent, into regions where it had never before been known to exist. Will this have any effect in causing the rulers of civilized nations to institute an inquiry into the disputed question of contagion, by men—not physicians—who are capable of distinguishing facts from fiction—certainty from uncertainty? We fear not. In the present state of affairs, therefore, there is too much reason to fear that, in spite of the abortive attempts made to exclude the disease by ill-regulated quarantine restrictions, it will go on extending from town to town, and from one country to another, until it overruns the whole of the Polynesian Islands and the densely-populated towns on the shores of India and China. That yellow fever does not reach towns at a high elevation is hardly correct, for according to Humboldt, it has reached an altitude of 2500 feet above the level of the sea; still, the higher the position the greater the immunity. Why it appears less frequently in these localities may be owing to their having less communication with infected spots than those on the sea-level, to their being generally less crowded, and to the absence of heat.

Neither time nor space will permit us to notice a tithe of the causes set forth in Dr. La Roche's work as instrumental in the production of yellow fever. He appears to attach great importance to the opinions of some writers who consider thunder and lightning, shooting stars, and ozone, as probable predisposing causes; and elsewhere he alludes to damaged coffee, putrid melons, and rotten oysters as exciting causes. There is, in fact, hardly any change or phenomena, normal or abnormal, which has ever been observed in the material world, that he has not, in some way or other, connected with the etiology of the malady. For example, if electricity act according to Piclot, Maher, Rutz, and others, as an exciting cause; and according to others as a preventive, he arrives at the conclusion that "it may, and no doubt does, act as an exciting cause by its excess, and as a predisposing one more frequently by its deficiency and modification."

We were not sorry when we came to the tenth chapter of the second volume of Dr. La Roche's work, which treats of the Efficient and Immediate Cause of Yellow Fever; for here at last we thought we might hope to see some light thrown on a subject which deeply concerns so many of our fellow-beings living within the Tropics; for if, in reality, the efficient cause be fortuitously engendered in certain localities by some unknown change in the earth or the air—if it be not a morbid product of the human frame—then away with quarantine restrictions, and leave the arms of commerce unshackled to grapple with the evil as they best may. But if it can be shown, or if there be just grounds for deeming it even probable, that the fever is the product of an infectious virus which emanates from the living frame, then, in the name of humanity, let us endeavour by fair and honest counsel to warn the inhabitants of those regions

who have hitherto escaped, to guard against its introduction by every means which, in accordance with law and humanity, they can adopt. But notwithstanding the attractive title of the chapter, it contains nothing more than a tedious repetition of the opinions of other writers which were noticed in the previous part of the book.

There was, it seems, a time, before men's minds became bewildered and their tempers "*riled*" by the conflicting opinions of medical writers, when the contagious nature of yellow fever was universally admitted throughout the whole extent of the American coast, from the Line up to the Gulf of St. Lawrence. The simple people of those times observing that the fever did not attack the rural population unless they communicated with towns—that it did not originate in the interior and extend towards the coast—that it invariably appeared first in seaport towns, and amongst the population residing near the shipping—and that in almost every outbreak it was traceable to vessels that had arrived from infected ports—that its origin on shore was frequently traced to persons who had resided in or near the dwellings into which strangers had been received ill of the fever, and that these outbreaks followed each other after long "spells" of ten, twenty, and sometimes even of fifty years' duration—observing these occurrences to follow in the manner of cause and effect, and not dreaming that the winds of heaven, through some unknown process or change, could become charged with a specific principle inimical to health; or that their mother earth, pregnant with all that is beautiful and useful in the sight of man, could emit an agent inappreciable by our senses, yet so deadly as that which gives rise to yellow fever;—these simple-minded persons, we say, came to the conclusion that the fever was not indigenous, but that in each succeeding outbreak it had been introduced from abroad. Dr. La Roche, however, informs us that this delusion rapidly declined in America, and that the doctrine of non-contagion, arising from small beginnings, and numbering previously to 1793 amongst its defenders but few names of weight, gradually gained proselytes, and finally, in later years became the predominant doctrine. We are much mistaken, however, if the tide of opinion be not now rapidly setting in an opposite direction. The melancholy catastrophe of the *Eclair*, and the introduction of the fever into Boa Vista, opened the eyes of most men who were not blinded by prejudice or self-interest. Dr. La Roche, we are sorry to observe, finding that he could not dispute the conclusions come to by Dr. McWilliam on this remarkable case, and with a wholesome dread of losing caste amongst his quarantine-abolitionist friends in New Orleans, resorts to the discreditable alternative of affecting to doubt the facts, though they were substantiated by the whole surviving population of the island, by Dr. King, who afterwards went over the same ground, and by documents—the current records of the time—lodged in the archives of the British and Portuguese Governments.

In a chapter on the Contagious Nature of the Disease, a few of the more remarkable facts in support of the doctrine are briefly noticed, and the opinions of the more notorious writers of the Pym school are faintly traced, but in a way which might lead to the conviction that, after all, Dr. La Roche has no settled opinion on the question of contagion. In one place he brings forward the most unexceptionable proofs of the com-

municability of the fever, and, as it were, challenges the reader to deny them; in another he ridicules his credulity, and charges the whole fraternity of contagionists with want of candour and truth. "The communication of the disease from the sick to the well, may," he says, "in general, be traced in a satisfactory manner. In other instances the fever is found to be due to exposure to articles of merchandize, to clothing and bedding impregnated with the seeds of contagion." "The contagion has been carried to distant villages and towns, and to public establishments situated in parts of the city which otherwise remained free from the calamity." A number of cases in point are detailed, convincing enough, one might suppose, to induce those who advocate the abolition of all protective measures to reconsider the question, lest they prove instrumental in upholding a doctrine which, if we are to believe the stories of the *Bann* and *Éclair*, is fraught with an incalculable amount of misery to the human race.

Dr. La Roche contrasts the cessation of yellow fever in cold weather with the cessation of fevers arising from marshes; this coincidence, he conceives, furnishes additional proof of the non-contagious nature of the former. The fevers, he says, though not identical, are closely allied, and are everywhere under the influence of certain hygrometrical conditions of the atmosphere, are due to the action of domestic causes, and are universally admitted to be void of contagious properties. "Every one, from the learned physician to the gossiping granny, knows full well that cold weather is sure to be marked by a cessation of the fevers in question." The extinction of yellow fever by cold we believe to be an undisputed fact, which has not, and perhaps cannot, be explained; but our learned author goes on to state that the same beneficial change is produced through other agencies—copious rains, heavy winds, especially from the north, desiccating and long-continued heat and drought; and that at times it is brought about by a change in the epidemic meteorization, the evolution of ozone, &c. As he does not inform us on what grounds he has formed these opinions, and as he elsewhere attributes the evolution of the disease to the same agencies, we have little hesitation in again ascribing these notions to his not having any settled opinions respecting epidemic meteorization, ozone, or even of the disease itself. There is a looseness in his modes of reasoning, and an abuse of scientific language, which we imagine may be traced to vanity, or a wish to appear learned in matters on which wise men are not ashamed to confess their ignorance. Sydenham, speaking of the kind of atmospheric constitution in which certain morbific influences are supposed to be developed, justly observes that "they are points on which we must be contented to plead ignorance. They are matters, like many others, upon which vain and arrogant philosophy speculates to no purpose."

We confess to our having been rather startled on being informed by Dr. La Roche that he had at hand eighty-nine authorities from which he could quote evidence to prove that yellow fever depended on a distempered condition of the atmosphere, and not on contagion. We can only find room for a few of these valuable proofs: there was a gentleman who informed Dr. Barton "that when yellow fever prevailed, his cauliflowers, cabbages, and radishes would wither and die." "Judge Selby's fig-trees did not produce so many figs as usual." These occur-

rences, he conceives, show that yellow fever allies itself to diseases originating from a general distemperature of the atmosphere—the effects of malarial influence—while it recedes from those of a contagious character.

But still more striking is the effect of the insalubrious atmosphere on animals. "Early in June, 1805, cats began to droop and die. Dogs also were severely and fatally affected. Next year, cats were again affected, as well as rats." "Many of the cats died numb and torpid, while others were seized with delirium and puking."* Even fish and oysters are known at times to participate in the same calamity. In 1798, flies were found dead in great numbers in the unhealthy parts of the city. "At Gibraltar, in addition to dogs and monkeys, a goatherd lost a great part of his flock, and almost the whole ceased to give milk." At New Orleans, in 1833, there was much sickness amongst horses, cattle, and swine. Again, "in 1819, they died with rotten tongues, and sheep and dogs with their hoofs dropping off, and calves with rotten ears."† These are very horrible revelations; and, if our readers do not deem them sufficiently confirmatory of Dr. La Roche's views, we must refer them to the work itself, where they will also find some remarks of equal value in an etiological point of view, on a well-known and fatal disease in the highlands of Scotland among sheep, termed "braxey."

Respecting the local habitation of yellow fever, its sphere of prevalence, we are told, is always somewhat, and on some occasions very, circumscribed, the disease remaining confined within the limits of the localities where it originated. In Philadelphia its ravages were generally confined to a few streets, and the same peculiarity was observed during the epidemics which assailed the inhabitants of New York, of Baltimore, and other cities of the Union; it often seems to affect, in preference, a particular building, a particular side of a street, or a particular part of a house. In Martinique, the patients in the Military Hospital suffered six weeks before the disease extended to the neighbouring houses; it sometimes attacked one vessel in the port, and then jumping over several, committed its ravages in others moored at a distance; even small localities and single houses, situated in the midst of an infected district, sometimes escape. From these circumstances, Dr. La Roche considers he is justified in drawing a conclusion favourable to the doctrine which ascribes the fever to some specific cause existing in the locality, and adverse to its contagious property. We differ from him; in the first place, because these are the characteristics which belong to small-pox and other contagious diseases, and not to the endemic remitting fevers of hot countries; secondly, if the originating cause of yellow fever depended on the combination of atmospheric and terrestrial agencies, the fever would make its appearance in several places in the same locality about the same time, instead of originating in one house or family, and then slowly extending, after the lapse of weeks, to others within a space not exceeding eighty yards. Remitting fevers do not remain within circumscribed limits, but occur scattered over an expanse of country. For instance, throughout the whole extent of the delta of the Niger, or the valleys of the Canton and Yang-tse-Kiang rivers, showing that they depend on a cause peculiar and common to these and similar regions. Contagious fevers break out in isolated spots, and

* La Roche, vol. ii. p. 316. † Ibid., p. 317.

radiate in lines corresponding with the lines of intercourse which are most frequented by men, showing that they depend on a cause which requires the human frame for its elaboration. A gap in the line of communication will arrest the extension of the disease in the same manner as the boundaries of a marsh limit the extension of periodic fevers. The periodic fevers of hot countries are most destructive in fens and jungles—they do not exist in the filthy stews of New Orleans, but the yellow fever germs, when once introduced into these sinks of human depravity, find the fitting soil for their evolution and reproduction.

The late Board of Health of London had a way of arranging isolated facts which led to conclusions that were erroneous and delusive. It presented to the public, in the form of a Blue Book, an account of all the filthy towns throughout England in which cholera prevailed, for the purpose of proving that filth and its products—foul effluvia—were the cause of the disease. Reasoning upon the facts given, the conclusion seemed to be inevitable; but had the names of places in which the disease raged, which were not in a filthy state, been placed in apposition, and if to these had been added the names of other towns into which the disease did not enter, though steeped to the eaves in filth and wretchedness, the conclusion would have been, that, however destructive of health filth may be, it had no direct influence in originating the epidemic. This singular body, with its characteristic modesty, next astonished the legitimate members of the profession by a Report On Yellow Fever, so arranged that, in the whole of the extracts, there was not to be found one grain of evidence that did not weigh in favour of the doctrine of non-contagion. We would hardly have noticed these productions here, were it not that abroad they appear to be received with the respect due to the writings of professional men. They are frequently quoted by Drs. La Roche and Barton; the works of both these authors, in fact, appear to have been drawn up on the same plan, and, as it were, for the purpose of swamping, by a torrent of selected evidence, the facts which establish the communicability of yellow fever on grounds that cannot be disputed. The former has devoted sixteen chapters to proofs against contagion; these proofs consist principally of extracts, tending to show that cases of yellow fever may be introduced into a healthy locality without the disease spreading. Evidence of this kind, unless for the purpose we have mentioned, is of no value, even if it were multiplied a hundredfold, for the same kind of evidence might be adduced to prove that small-pox and scarlatina are not contagious; it simply proves that, in these particular instances, the fever did not extend. It is hardly conceivable that any man of ordinary information could have fallen into an error so palpable as to suppose that, if the fever were contagious, it ought to spread in every instance. As well might we assume that the electric bolt is harmless because it does not always kill, or that the burning brand which falls amidst combustible matter is not fire because it fails to set the latter in a blaze. Speaking of the epidemics of Philadelphia, and entirely ignoring all that he had previously written respecting the contagious nature of the disease, he states that he is not hazarding too much when he affirms, that yellow fever has not been propagated to other towns or cities by the removal of persons labouring under it, or by their effects; and that the result in other cities of the States has been of a kind to confirm fully the

experience there obtained. After detailing a number of instances in which no fever followed, notwithstanding the unrestricted communication with infected districts and persons, he next alludes to others which the contagionists have laid hold of as evidence of undoubted communication. With respect to the former, they are admitted on all hands as facts which have been placed on record by men of unquestioned veracity; even Pym, the most truculent of all the contagionists, dead or living, would respect them for what they are worth. With respect to the latter, he either derides the evidence as not being credible, or sets it aside on the grounds of there having been dirty water, accumulations of filth, or foul effluvia, at hand when the disease made its appearance; or, he affirms that the locality, "under particular meteorological conditions," was prolific of fever. So frequently are the changes rung on these cabalistic phrases, that at last we almost unconsciously admit that there may be some meaning attached to them. But with every respect for the opinions of Sydenham and other distinguished writers, we cannot help thinking that, within the last thirty years, particularly in reference to cholera and yellow fever, they have been used in a most improper manner, and not unfrequently as a cloak to hide our own ignorance. The atmosphere may be warm or cold, damp or dry, but these are the only conditions peculiar to it which have any influence on health. Emanations from the living, labouring under contagious diseases, produce the same kinds of disease, and gases or effluvia of local origin may impair the general health, but they do not give rise to special epidemics.

Writers who, like Drs. La Roche and Barton, affect a kind of transcendental knowledge respecting the causes of disease,—who assume that they *may* be traced to unknown agencies and to physical changes in the elementary conditions of matter, should be prepared to explain the nature of these, or to offer some kind of proof in support of their assertions that such changes really do take place. If, however, we reflect, that for ages it has baffled the skill of the best analytical chemists to detect any cause or agent in the atmosphere to which epidemic diseases might be ascribed, we shall be the better able to appreciate the value which ought to be attached to the hypothetical assumptions with regard to the evolution of a specific aërial cause of yellow fever, put forth by such writers as Drs. Barton and La Roche. The latter affirms that, of all the instances produced in proof of the propagation of yellow fever from cities in America to other cities, towns, and villages, more or less distant, not one has been satisfactorily made out. We have arrived at a contrary opinion; and further, we have an idea that the proofs are so satisfactory, that they can hardly be more conclusive, unless the efficient cause itself be made demonstrable. We have already stated that the question cannot be determined by negative proofs, though these should outnumber the stars in heaven; but one fact of a positive character, such as the communication of the disease to the garrison of Ascension by the crew of the *Bann*, or the communication of the fever by the crew of the *Eclair* to the islanders of Boa Vista, decides the question at once and for ever. The first occurrence shook the fragile structure of the non-contagionists to the base, and the latter scattered it to the winds. They may affect to disbelieve Sir Wm. Burnett on the one hand, and Dr. McWilliam on the other, but they cannot alter the incontestable facts.

The crews of these vessels contracted yellow fever at Sierra Leone; the first ran to Ascension, and landed her sick; the second to Boa Vista, where she did the same. Yellow fever had never existed in either island before, but immediately after their arrival it broke out, and raged with great severity for several months, and then entirely ceased. Twelve years afterwards—namely, in 1837—the *Ætna Raven* and *Forester* arrived at Ascension, with their crews ill of the disease; and shortly afterwards the fever again broke out in the garrison, and swept away a large number of persons; but since that time it has not made its appearance. In 1829, the *Heda* and *Scout*, with yellow fever on board, arrived at Fernando Po, and landed their sick; immediately afterwards it broke out amongst the settlers, though a case of the disease had never occurred in the island before. In 1837, the brig *Forester* landed yellow fever patients at St. Mary's, on the Gambia; the inhabitants were immediately attacked, though the fever had never before been known to exist there. In a community so small it soon became extinct; and has not, up to the present time, again made its appearance. From the Gambia, it was carried by a small trading vessel to the island of Goree, where it attacked a large number of the inhabitants, and then, in the same manner as in the places already mentioned, it entirely ceased; the contagious germs which had escaped from the last cases were scattered by the winds, and from that time to the present day, yellow fever has been unknown in the island. These and numerous other instances, all equally conclusive as to the introduction of the fever into islands and localities where it never before had been known, could not have escaped Dr. La Roche's notice; yet, with a marvellous disregard of the consequences, he coolly advocates the abolition of protective or even precautionary measures, with the view of restricting its extension.

We have already mentioned Dr. Barton's views respecting the cause of yellow fever—namely, that it is an agent which is produced or generated by the junction of two other imaginary agents,—the one of an aërial, the other of a terrestrial nature; these he compares to the blades of shears, which he calls "my shears," or "the shears of fate." When apart, *they* are harmless or inert; but when they meet, without the help even of an encouraging *hey, presto*, the true excitant of yellow fever is produced. Unfortunately for the Doctor and this theory—which is nearly as old as the hills—no evidence can be adduced of the existence either of the generating or generated agents; they are mere myths which do not assist us in accounting for the prevalence of the fever in one part of a town and not in another, or for its sudden appearance in distant ports and places after the arrival of infected ships or persons—occurrences, we conceive, which can only be explained by assuming that it originates and spreads in the same manner as other contagious diseases.

Dr. La Roche has evidently been greatly puzzled in his selection of an efficient cause. He sometimes adopts Dr. Barton's blades, acting in conjunction with "certain conditions and combinations" of other elementary matters; while, at other times, he rejects the idea of any of these agents, whether singly or combined, aërial or terrene, being capable of producing yellow fever. The poison, he says, is not precisely similar to that of other malarial fevers, though it is of miasmal origin. It does not arise

from animal decomposition; but considering the influence of vegetable decomposition in the production of periodic fevers, he finally adopts the views of Dr. Wilson, who refers "the origin of the exciting cause of yellow fever to ligneous decomposition; for though these organic compounds may not be discovered on the surface, they assuredly exist beneath." The facts, he says, adduced by Dr. Wilson, "recommend themselves in many ways, by furnishing evidence amounting, so far as it goes, to prove that ligneous decomposition is, if not the unvariable, at least a frequent, and in many places the only, cause of the disease."

We shall now take leave of this question by merely remarking that yellow fever does not, as a general rule, prevail where the products of vegetable decay are most abundant,—as in the valleys of the Nile, the Niger, the Ganges, the Yang-tse-Kiang, or even of the Nicaragua; while it has prevailed with great virulence in localities which are comparatively free from them—as, for instance, on the island of Boa Vista, at Stony Hill Barracks in Jamaica, and on the Island of Ascension, where there is hardly a particle of vegetable mould or matter, either above or below the surface, the island being one mass of lava and ashes, the products of volcanic fire.

Of the fourteen hundred pages of Dr. La Roche's work, about one hundred are devoted to treatment; they are the pennyworth of bread to the enormous amount of sack which precedes them, in the form of proofs for and against contagion; they, in fact, contain all that is sound and practical in the work.

Adverting to the various modes or fashions which have prevailed at different times, according as the fever was viewed to be of a typhoid, putrid, bilious, or of an inflammatory character, he says, the fever, in a therapeutical view, may be divided into three categories:—The 1st comprises those in whom the poison has produced a deadly impression; 2nd, those who are so mildly affected as to recover without any treatment; 3rd, those in whom the disease assumes an intermediate grade. We have already noticed that these differences are sufficiently distinctive for every practical purpose, whether with respect to the treatment or classification. It is, however, obvious that the treatment suited to each variety, as well as to accidental complications, will vary very considerably, so that rules cannot be laid down as applicable to particular cases, either singly or in series. We must content ourselves with endeavouring, not to neutralize the poison circulating in the system, but to correct the morbid effects it occasions both on solids and fluids; we must guard against mischief being done to vital organs, reduce undue and dangerous excitement, and sustain the powers of life when reduced below the point of safety; but beyond this, art is of little avail. The idea of curing the disease is a delusion. To nature and time the cure must be left; and in cases marked by no organic mischief, the physician will do well to keep his hands off, and restrict his agency to the employment only of such means as are necessary to relieve the symptoms; more danger is to be apprehended from too great than too little interference. There are few who have seen much of yellow fever, who will not admit the salutary counsel contained in these remarks, which we have epitomized from the twenty-third Chapter.

In the inflammatory varieties, antiphlogistics and evacuants are indispensable in the first stage, but care must be taken to graduate their energy to the force of the circulation, the heat of the skin, and the extent of the local inflammation or congestion. In endeavouring to relieve these, we should not lose sight of the necessity of husbanding the strength, while we avoid everything calculated to depress the powers of life. In the more severe forms of the disease, practitioners need not place their principal reliance on the lancet; for, as a general rule, the milder and less debilitating method will be the safer. The rapid and copious abstraction of blood tends to hasten collapse, and thereby places the system in a condition from which recovery is always doubtful. Dr. Barton sometimes drew 70 ounces of blood at one bleeding, and in the course of twelve or eighteen hours afterwards, from 10 to 20 ounces more; but the utility of this practice is justly questioned.

The employment of emetics, whether with the view of cutting short the fever or shaking it out of the system, gains no favour in the sight of Dr. La Roche. "They aggravate the irritation and inflammation of the gastric organs; they foster the disposition to vomit, which it is a primary object to relieve, and which, when once elicited, it is difficult, if not impossible, to control." They have been discarded from the treatment of yellow fever, and are only resorted to occasionally to meet peculiar contingencies. Purgatives from an early period have been used in America, and found advantageous under particular circumstances; they produce comparatively little irritation in the stomach; they expend their action on the intestines, which are but seldom in a state of irritation or inflammation. "As a general rule, it may be regarded as safe and proper to administer purgatives every day until proper and copious evacuations have been procured." Calomel purges are useful when given in moderation; they have a soothing effect, and are supposed to evoke bile, but "the physician must have other objects in view in the treatment of yellow fever, besides the excitement of the biliary secretion. It is not certain that the restoration of that secretion is much more essential than the cutaneous secretion;" and as mischief is often produced by attempts to force the latter, it is but reasonable to suppose that the same may result from heroic efforts to force the former.

We rejoice to find that Dr. La Roche disapproves of the mercurial plan of treatment.

"It is," he says, "acknowledged on all hands, and the fact is too evident to be denied, not only that patients labouring under severe attacks of the yellow fever recover under the use of mercury, but that it is occasionally the instrument of relief. But it is not the less certain also that mercury under all circumstances is an inconvenient and disagreeable remedy; it often produces effects of a highly painful, injurious, and dangerous character. And when we bear in mind that while trusting to this remedy we are allowing moments to pass which might be more profitably employed, we cannot refrain from the conclusion that it is imperative in us, before aiming at mercurializing the system, to ascertain whether the advantages derived from the process are likely to outweigh the objections to its use founded on the circumstances mentioned."*

There are a few remarks on the use of quinine, but they are of little importance; the author does not appear to have a clear conception of the

* La Roche, vol. ii. p. 666.

action of this medicine. Confounding the two diseases, he seems to think that it ought to have the same effect in yellow fever that it has in periodic fevers; but we have yet to learn that it has any influence on the system in the former beyond that of acting as a tonic.

Review II.

1. *Clinical Lectures on Surgery.* By M. Nélaton. From Notes taken by Walter F. Atlee, M.D.—*Philadelphia,* 1855. 8vo, pp. 755.
2. *Surgical Reports, and Miscellaneous Papers on Medical Subjects.* By George Hayward, M.D., President of the Massachusetts Medical Society, Fellow of the American Academy of Arts and Sciences, late Prof. of Surgery in Harvard University, and one of the Consulting Surgeons to the Massachusetts General Hospital.—*Boston,* 1855. 12mo, pp. 452.

"My experience, since the publication of the preceding papers on the anæsthetic agents, has satisfied me of the correctness of the views I then took. If there be any change, it is that I attach more value to sulphuric ether, and have more perfect confidence in the safety of its inhalation than I had at that time. Of its efficacy I have no doubt. I have never in a single instance, during the last five years, failed of producing by it entire insensibility to pain, without causing much inconvenience in its administration, and without any serious or troublesome consequences afterwards. Nor do I find any evidence that it has ever proved fatal, though it has often been exhibited in a reckless and unskilful way.

"Nothing has occurred to change my opinion in relation to chloroform. Many additional cases of death from its inhalation have been published, and scarcely a medical journal comes to us from Europe that does not add one or more to the melancholy catalogue. It is wonderful to me that intelligent and educated men should continue to use an agent of such terrific power." (Hayward, p. 250.)

We have given such prominence to the foregoing statement of Dr. Hayward's opinion because of its weight, its severity, and the unqualified manner in which it is asserted. Dr. Hayward's was the second patient in the United States who inhaled ether for a surgical operation, and the first in whom the narcotism was complete. He has continued to use the same agent ever since, and his experience of its effects must now be much greater than that of any man on this side of the Atlantic. His voice, therefore, has a claim to be authoritative on the subject of its innocuousness. Our experience in Europe abundantly pronounces the harmlessness of chloroform in all healthy persons; and it is unlikely that the opinion we have quoted above will alter the recognised practice of European surgeons. Yet there are certain cases, more particularly those of disease of the heart and emphysema of the lungs, in which many practitioners fear to employ chloroform, but which may bear the administration of the ether without injury.

The work of Dr. Hayward is, in great part, a reprint of papers which have already been published, and some of which appeared as long ago as 1824, 1817, and 1815. The author has, we think, rightly judged that the "facts and tables, if collected together, might be read with advantage by students and the younger members of the profession, and would be consulted by practitioners who have little leisure for the examination of more extended works."

The first two papers are the reports of his surgical practice in the General Hospital at Massachusetts, for a period of sixteen months, and they contain various matters of surgical interest, from which we abridge the following:—

Inflammation of the Hernial Sac.—The author gives four cases of this unusual affection as having occurred to him within a period of eight months. In one of them, the sac was gangrenous; in the second, fibrin was effused in abundance, but no pus formed; in the third, suppuration took place; and in the fourth, the inflammation was so much reduced, that it no doubt terminated in resolution. All the patients recovered, for the sac being no longer of similar structure to the peritonæum, the inflammation did not extend beyond the hernial ring.

"A labourer, aged forty, states that he has had a rupture since childhood, easily and entirely reducible at all times; never any incarceration. Six days since, was attacked with pain, described as colic, about the umbilicus; got some cathartic medicine, which operated. Three days ago, hernia came down, and has not been able to reduce it since. Has been bled twice, and the taxis attempted by several, without success. Has not had much pain in the tumour. No vomiting. Salts operated yesterday. Now, pulse eighty-eight, of moderate strength. Tongue white; coat on lobes moist. No sickness or pain in epigastrium. Some tenderness in left iliac region; less in right. On the left side is an inguinal hernia, the size of two clenched hands, to the touch hard, without resonance. Integuments slightly reddened. External ring tightly girt around the neck of the tumour, which is remarkably large and firm. Some pain upon pressure. A purgative enema came away without operation. The taxis was tried in the warm bath, and the size of the tumour somewhat diminished. Ice was then applied to the tumour, and the bowels freely opened with a pill of jalap and croton oil. The next day, the scrotum was œdematous at the bottom, and the operation for hernia was performed. On opening the sac, a gangrenous odour was emitted, and the sac was in a gangrenous state, but nothing was found in it, and there was no stricture at the ring. In removing the sac, the tunica vaginalis was punctured, and two ounces of the water of a hydrocele escaped. No inflammation took place in the abdomen; the whole scrotum, together with testicle, swelled to five times the natural size; a copious discharge of bright orange fluid from wound, with escape of gas on pressure, probably from the sloughing of the cellular membrane; but the patient recovered completely, and was discharged well fifty-four days after admission." (Hayward, p. 25.)

These papers are followed by a long and interesting report of a committee of the American Medical Association, on the permanent cure of reducible hernia, in which the vast variety of plans for the closure of the unnatural opening is reviewed, and an unfavourable estimate formed of them all. Injection, cauterization, ligature, sutures, excision of the whole or of a part of the sac, seton, acupuncture, scarification, castration, the invagination of a portion of the scrotum into the inguinal canal, plugging the canal with lint, with a pouch of goldbeater's skin, with omentum, or with the inverted sac itself, closing the external ring by suture, by stitching to it a flap of integument, or by forcing the testicle into it, all are reprobated on some ground or other, and the conclusions at which the committee arrived, are embodied in the following three propositions:—

"1. There is no surgical operation at present known which can be relied on with confidence to produce in all instances, or even in a large proportion of cases, a radical cure of reducible hernia.

"2. They regard the operation of injection by the subcutaneous method as the safest and best. This will probably in some cases produce a permanent cure, and in many others will afford great relief.

"3. Compression, when properly employed, is, in the present state of our knowledge, the most likely means of effecting a radical cure in the greatest number of cases." (Hayward, p. 117.)

It is probable that a certain number of recent ruptures in young persons will be permanently cured by any treatment under which the hernial orifice is kept closed for a considerable period; but it is equally probable that there are many cases of inguinal hernia in adults also, which are susceptible of cure by the method of inverting a plug of the scrotum into the inguinal canal. The plan appears to have originated with M. Gerdy, and to have been modified by other surgeons; but no satisfactory mode of securing the plug of scrotum in its inverted position was obtained, until one was suggested by Professor Wutzer, which has been successfully practised and introduced to the notice of English surgeons by Mr. Spencer Wells. The ingenious instrument by which adhesion is secured between the inverted scrotum and sac and the inguinal canal, at a height sufficient to close the internal abdominal ring, is described in the 'Transactions of the Royal Medical and Chirurgical Society,' vol. xxxvii.

Passing over papers on the Diseases of the Knee-Joint, the Statistics of Amputations, and one or two others, we find 9 cases of vesico-vaginal fistula, for which Dr. Hayward adopted the practice of paring the edges, and uniting them by suture. He obtained the complete closure of the fistula in 3 of these cases, and a partial closure, with the restoration of a power of retaining the urine for varying periods, in 4 others. The remaining 2 operations were unsuccessful.

Dr. Hayward is a strenuous opponent of the doctrine that there is anything contagious in the Asiatic cholera, and his paper on that subject will be read with interest still; for although as old as 1832, it is reproduced, and its doctrines are affirmed anew at the date of the publication of the present volume. His arguments are chiefly drawn from the allegation, that the disease did *not* follow the lines of human traffic in its course westward, from the fact of its spontaneous outbreak in ships at sea, and from the immunity from the disease enjoyed by the habitual attendants on the sick. Of the other numerous papers, we need call attention to but one, which contains the statistics of pulmonary consumption in the cities of Boston, New York, and Philadelphia, for forty years (1810—1850), with remarks:

"The most striking fact brought to light by these tables is the great decrease of deaths by consumption in these cities. This decrease has been great in all, and is not only relative but absolute, for the mortality has been somewhat more during the third period of ten years than it was thirty years before. During the first period of ten years, the deaths from consumption were to the total mortality in Boston as 1 in 4·622; while in the fourth decennium, they reached only 1 in 7·011. In like manner, in New York, the improvement was from 1 in 4·467 to 1 in 7·730. In Philadelphia, during the same two periods, the proportionate mortality was 1 in 6·498 against 1 in 8·080. The only exception is, that consumption has somewhat increased in Boston during the last ten years; and that, for this period, this city has been surpassed by Philadelphia in its exemption from that disease." (Hayward, p. 301.)

We look upon the work of Mr. Atlee with much interest, not only as a book full of ingenuities, both in diagnosis and surgical practice, but still more as a specimen of the kind of instruction which awakens the attention of students in the most important department of their duties, clinical study. The indifference which is shown by the great majority of our students to every form of clinical instruction, contrasts strikingly with the attention of the crowds who throng the clinical theatres and wards of the French capital. In none of the hospitals of our metropolis are the medical officers without occasion to regret the ill-success of their schemes to attract students to the wards. Their systematic lectures are regularly, sometimes eagerly, attended; but no amount of professional reputation avails to secure a similar attention to their clinical instructions. Yet clinical teaching is the most valuable of all teaching, and those whose duty it is to undertake it, would find in this volume many valuable hints as to the best mode of pursuing it.

Not only are these lectures evidently the work of a man full of knowledge, and skilful to impart it, but they seem to have been mostly studied beforehand, which our English bedside aphorisms usually are not. It seems also to have been M. Nélaton's invariable practice to announce his opinion or his doubts as to the nature and the prognosis of each case before proceeding to treat it. The students, thus sharing the thoughts and eager to test the judgment of their instructor, follow with watchful zeal the progress of the case, and, whether he were right or wrong, become substantial gainers by every such exercise. In such a case as the following, we have a record which is instructive at once as a surgical fact, as a fact well told (though we have much abridged it), and as a model of ingenious teaching.

"*Veno-arterial Aneurism.*—A woman entered the wards with a very complex affection. Her tongue was large, and deformed by lumps of a bluish colour. Some parts of it were harder than natural; others softer; and in others, again, the consistence was unaltered. At the base, alongside of the papillæ, were large violet-coloured granulations, like mushrooms. There were venous tumours upon the lip and on the inside of the mouth, as well as upon the side of the neck and beneath the jaw. Inside of the mouth, under the tongue, was a projection, bluish, but more pale than would be a simple dilatation of the veins. Under the jaw was another projection, and, by proper pressure, it was found to be the same tumour with that under the tongue. This tumour was supposed to be ranula. Another order of symptoms had caused a belief in the existence of aneurism, and led to the patient being sent to the hospital. In the region of the carotid was a tumour, situated alongside of the larynx, and extending upward as far as the projection caused by the ranula. This tumour, by pressure, was made to disappear with the greatest facility; it pulsated, and presented a *frémissement*, which was very apparent; but in order to appreciate it, the pressure had to be very slight. A peculiar *bruit de souffle* also existed; it was continuous, only at each beat of the heart it was augmented. There was a slight difficulty in eating and speaking, but the condition of the tongue and the tumour in the mouth explained this.

"These things being determined, three diseases could give rise to these symptoms. In the first place, there was a ranula, and, besides that, there were two vascular affections. That in the tongue was a venous erectile tumour, and, as is almost always the case, was congenital. The other affection was either an arterioso-venous aneurism, an aneurism by anastomosis, or else what is called an arterial varix. The continuous *bruit de souffle*, which existed in this tumour, is

only found in three conditions—in chlorosis, and in the two affections just mentioned. The sound was limited to the part, and could not arise from chlorosis. If it were an arterial varix, the sound would be extended everywhere throughout the whole region. It is very rare that the affection does not extend itself to all the arteries of a part; but as the sound was limited, and greatest near the thyroid cartilage, M. Nélaton inclined to admit the existence of a communication between the artery and vein, of an arterioso-venous aneurism.

"About two weeks before coming to the hospital, this patient had had an attack which placed her in great danger. An enormous tumefaction on the left side, by which the whole space between the jaw and the shoulder had been filled, had taken place, and the general symptoms accompanying it had been very severe. When she entered, hard lumps were found here and there throughout that region; the lumps were clots of blood resulting from inflammation of the varicose veins, which had given rise to the alarming symptoms that had before existed.

"In the treatment it was determined to interfere only with the ranula; the venous erectile tumour might remain stationary, and give no trouble, and the only operation suited to the arterioso-venous aneurism was that of tying the artery on either side of the opening in it; such a proceeding could not be pursued in this case.

"When the trocar was plunged into the tumour, nothing but venous blood issued. M. Nélaton said he had opened one of the varicose veins before reaching the ranula. The flow of blood was quite abundant, and interfered with the operation, so that M. Nélaton was content with making as much come out as he could, and only using water as a subsequent injection. The patient did very well until the fourth day after the operation, when she had buzzing in the ears, was delirious at night, and had several liquid stools. She did not complain of the region operated on, when it was touched. On the third day of the attack there had been but one stool since the day before, but the condition of the woman was very alarming; her appearance had changed profoundly. On the sixth day she died. At the autopsy, pus was found under the arachnoid, but nowhere else in the body; there were no signs of purulent absorption in any part. M. Nélaton said the patient had died from an attack of meningitis. It should be mentioned that at that time there were many cases of cholera in the hospital. When a canula was introduced into the opening made under the tongue, and air blown into it, the whole submaxillary venous mass was blown up, and the tongue also, so that it was everywhere crepitant. The dissection of what was supposed to be an *arterioso-venous communication, direct, without any false consecutive aneurism*, was performed with the greatest care; an injection was thrown, with gentleness and slowness, into the primitive carotid; and, at the expiration of a few moments, it flowed back from a small venous branch. This, it is true, can always be accomplished, but it requires generally more force than was employed in this instance. It was, however, impossible to find the place where the communication existed. There was no aneurism of the carotid, nor was there a varicose condition of the arteries." (Nélaton, p. 67.)

It will be quite beyond our power to furnish, in our small space, an adequate idea of the practice and opinions of M. Nélaton, or of the impression of candour and ability conveyed to us by Mr. Atlee's mode of detailing them. While there are some modes of treating diseases to which we should take exception, there are others which are models of ingenuity and skill. We should forbear, for instance, from extirpating the mammary gland, even in a male subject, merely on account of neuralgia. (p. 503.) The serious consequences of practising iodine injections into cavities of all kinds, knee-joints included, are described with sufficient frequency in this volume to deter us from following M. Nélaton

in his ill-judged and hazardous use of them. He makes a distinction, too, between anthrax and diffused phlegmon, which appears to us to lead to wrong practice. He says,

"Let an anthrax alone, there will be openings formed, pus and the core (*bourbillon*) will come out, and after that it will heal; but if you make incisions into it, the edges of the incision will separate very widely, and the healing process will be very long. It must not be imagined that the incisions relieve a strangulation." (Nélaton, p. 35.)

When gangrene of the subjacent tissues has taken place, of course he recommends incisions. But for the most part, though we cannot commend all his practice, M. Nélaton's treatment is characterized by great ingenuity, and directed by a very accurate acquaintance with the various features of the case in hand. Here is an example of his ingenuity, though we cannot but think that it was applied to the wrong limb.

"A young man, aged twenty, had had necrosis of one tibia since he had been five years of age, and the diseased bone was two-fifths of an inch longer than its fellow. The deformity had reacted on the whole skeleton; the pelvis was elevated on the side of the longer limb, and the vertebral column, from being thrown to the opposite side, was curved, and its concavity looked towards the side of the longer limb. M. Nélaton stated that he had made use of this fact to remedy incurvations of the vertebral column, advising a thicker sole to be worn on the side of the concavity. At another part of the book he states that he had found the plan successful." (Nélaton, p. 280.)

Surely, if too long a limb produces lateral curvature of the spine, it is to be cured by lengthening the short limb, or that on the side of the convexity. Mr. Atlee relates three cases of extirpation of the whole tongue for extensive cancer of that organ, prefacing their history with the following quotation from M. Nélaton's remarks:

"As regards the condition of the patient after the extirpation of the tongue, as far as speech is concerned, he gets along very well; the tongue is not indispensably necessary for speech; in congenital absence of that organ, for three or four years the child does not speak, but afterwards he acquires the power of doing so very well."

Nevertheless, we can hardly look upon the operation as likely to commend itself to surgeons in this country. The lower jaw is divided in the mesial line, and the tongue excised from the epiglottis forward; the hæmorrhage is great, and often recurs; œdema of the glottis may soon follow the operation, pneumonia and pyæmia at a later period; and of M. Nélaton's three cases, one died of suppuration in the parotid, and another of pneumonia. In the third case "the wound healed, and the patient left the wards well."

The following extract is worth noting, as illustrating the treatment of the very uncontrollable enlargements of the bursa beneath the annular ligament of the wrist:

"The first of these cases got well almost immediately upon the application of *sinapismes volantes*—i.e., a sinapism for a quarter of an hour at a time, and applied six times a day. The second case had been successfully treated six months before, by applications of alcohol; the affection had, however, returned. They were again made use of, and with marked benefit; in two days the pains had disappeared, and the effusion gradually followed. This application of pure alcohol is painful, the epidermis is raised by it, it produces true vesication. In the third

case, in which perforation had taken place, blisters were made use of, but without producing any great amelioration; after them, cauterizations over the course of the tendons were practised, but without any marked benefit. After the patient had been nearly six months in the wards, with some hesitation from doubts as to the innocuity of the proceeding, injections of iodine were made use of, and under their influence the sac rapidly healed, and the fistula closed." (Nélaton, p. 104.)

In his observations on amputations about the foot, we are somewhat surprised to find the very moderate approbation which M. Nélaton affords to the operations of both Chopart and Syme. The latter he calls good, but good when you cannot do better; that is to say, when you cannot leave the leg longer, perform an operation which is less dangerous, and make your patient independent afterwards of a costly instrument. He seems also to have had trouble with the stumps after Chopart's operation, from the turning of the foot by the traction of the muscles of the calf, the pressure exerted upon the cicatrix against the ground, and the formation of corns, which sometimes force the patient to ask for a new amputation. But then, it does not appear that he is in the habit of removing the lower edge of the os calcis, to which the annoyance is chiefly due. We have practised this on all occasions, and have never failed to be satisfied with it; and in a recent case, in which this was done, and a firm case of starched bandage applied round the stump as soon as the wound was healed, the patient bore confidently on the limb, and walked without a stick at the end of two months from the amputation. M. Nélaton's experience is adverse to the practice of dividing the tendo-Achillis in these cases: he has done it four times in one patient in vain; and our own belief is more and more confirmed in its being unnecessary, whenever the foot is from the first, and for a sufficient time, firmly supported by a tightly-fitting case. The following operation is an important addition to those which are commonly practised in this region of the body:

"The operation of amputating the foot below the astragalus was first proposed in 1839, and it has been found to surpass all expectations. It has been performed in Paris at least fifteen times, by Roux, Malgaigne, and many others. It is tedious, as all operations of the same kind are. The following is the method practised by M. Nélaton:—

"The first step is to form the flap in the soft parts, and this plan, about which there have been so many discussions, was invented, M. Nélaton believes, by M. Roux, of Toulon. The surgeon commences the incision six-fifths of an inch from the point of the heel, on a line drawn from it to the external malleolus, and carries it along to the external edge of the foot, behind the projection formed by the posterior extremity of the fifth metatarsal bone; then obliquely across the sole of the foot and passing over the projection of the scaphoid bone, and over the dorsal surface, an inch and three-fifths in front of the angle it forms with the leg, it is brought round to the point of commencement. This incision encloses a flap of soft parts, by which the bones are covered, to use M. Nélaton's expression, *to perfect perfection.*

"The second step of the operation is to make a movement by which the anterior extremity of the foot is forced in the direction of its internal and plantar surfaces, so as to make the head of the astragalus project; and when this has been done, nothing is more easy than to divide the ligaments by which the bones are held together.

"But now, in order that the operation may be well done, all the soft parts enclosed by the incision made in the first step of the operation, must be preserved, and the bloodvessels must be respected as they pass along the internal side of the

articulation. For this purpose the surgeon must extract the calcaneum *by resecting it*, and this is very laborious; the tendo-Achillis and all the parts must be separated as near as possible to the bone. The surgeon must never expect to do this operation as he does that of Chopart.

"The same flap, made a little smaller, would answer in amputating at the ankle-joint, in case the astragalus should not be found entirely healthy; in which case, also, the malleoli should be removed. These two operations, above and below the astragalus, differ in this, in the length of the limb left afterwards, a difference amounting to one inch." (Nélaton, p. 219—223.)

A stump made in this manner is described as healed in six weeks, round, and well formed; and when pressure was made against it, there was no pain. The shortening was four-fifths of an inch. The under surface of the astragalus is not regular, but by degrees it becomes so, and the projections, from which bad effects might be feared, then disappear.

Of another case it is said: the cicatrization of the wound was quite tedious; it was not completed before the expiration of three months. It was delayed by the formation of two abscesses; one anteriorly, from inflammation in the sheath of the tibialis anticus; the other at the heel. The result was as satisfactory as possible; the limb had lost but four-fifths of an inch in length, and the stump itself was perfect. The patient was able to walk about the hospital very well. In twenty months he returned, walking still very well, and having on one occasion walked a distance of twelve miles, and returned the same day.

It should be stated that one of these patients was twenty years of age, and the other but sixteen. It is not improbable that, in amputations of this kind done at a later period of life, the stump would accommodate itself less readily to the inequalities of the lower surface of the astragalus, and that it might be wise to remove the angles of its posterior and concave articulating surface.

Fibrous Polypus of the Nose.—The following plan of operating commends itself as applicable in some cases to which the present modes of extirpating certain polypi are not suited:

"Some time ago, toward the close of the year 1848, a young man went to M. Marotte, complaining only of epistaxis; M. Marotte thought that there was a polypus of the nose. At a later period, on the patient applying to M. Nélaton, he found the soft palate projected forward, and other signs of a fibrous polypus of the base of the skull, and he ascertained it to be implanted at the basilar process, near the petrous portion of the temporal bone, at the sphenoid, and continuing forward, at almost the whole internal face of the pterygoid process. M. Flobert had already practised an operation that threw some light on the subject; he took away the superior maxillary bone, and was thus enabled to remove the whole polypus and a great part of its roots. He was so happy as to cure his patient. Soon afterwards a surgeon of Lyons did the same, but the details of the case were not published. M. Nélaton was acquainted with these facts, but he was not decided to practise the removal of the maxilla; he thought it better to excise the roof of the mouth, a very simple operation. M. Manne, of Avignon, was the first to have the idea of dividing the velum palati in order to get at these polypi. M. Nélaton was aware of this plan, and determined to go still further. He cut the membrane, hard as leather, covering the roof of the mouth, firmly down to the bone, in the median line; an incision was then made anteriorly from one side of the mouth to the other, meeting the other, so as to form a T-shaped incision; with a spatula the membrane was then detached from the bone on each side; this detachment is easily performed. There are some difficulties, however, in turning the membrane

aside posteriorly, and this is the cause of it: the velum palati is formed of two membranes, one palatine, the other nasal; it is then easy to understand that when you try to drag it aside, no matter how thoroughly it has been detached in the mouth, it still adheres by the posterior layer. This posterior layer of the velum must be cut by a pair of curved scissors. The soft parts being thus turned to the sides, with a small perforator, two holes were made in front, one on the right, the other on the left; a blade of Liston's forceps was then inserted into each hole, and the bone was cut. The bony roof of the mouth was broken to pieces, and thus a large opening was made by which to extract the polypus, the whole of which was then excised; the operation, however, was by no means considered as completed.

"The next day, the whole of the wound was found united from top to bottom; the parts were separated again; the following day they had again reunited, and were again separated; they remained afterwards without uniting. The patient had an attack of pericarditis, which prevented a continuation of the treatment for some time, but when he recovered, the roots of the tumour were scraped away, and Vienna paste applied. All this, as may readily be believed, was not the affair of a week's time, but of two or three months. When all was thought to be destroyed, the patient was still kept several months longer in the wards, and at the expiration of that time, there being no symptom of a return of the affection, staphyloraphy was performed, and the palate reunited. M. Nélaton saw this patient again in 1853; the opening in the roof of the mouth had a diameter of about one line laterally, and three lines antero-posteriorly." (Nélaton, p. 421.)

The propriety of such an operation is found in the fact that by no other means can tumours of this description be entirely removed. They may appear, from the extent to which they have raised the mucous membrane of the pharynx, to originate from the bodies of the upper cervical vertebræ, and they have been supposed to spring as low down as the fourth cervical; but that vertebra is, in fact, opposite the larynx, and the velum palati is in relation with the body of the axis; there is, therefore, less room in the vertebral column than has been imagined for the attachment of the root of a tumour. These fibrous polypi mostly grow from the basilar process of the base of the skull, a little behind the point at which malignant growths usually make their first appearance in this locality. The root of a tumour in such a situation can of course not be reached so as to be extirpated either by ligature, by removing the superior maxillary bone, or dividing the soft palate; for though its various projections may be found in the nostrils, the antrum, the zygomatic fossa, the pharynx, or even—by its pressure causing absorption of the palate—in the mouth, yet its roots are closely mingled with the periosteum of the basilar process, and can only be securely eradicated by direct applications to that part. When the superior maxillary bone is removed, there is but little space at the back of the nostrils to work in; and as the wound is closed at once, there remains no opportunity to repeat applications to the original site of the growth, from which it may be expected to spring forth again. The operation through the palate has at least the advantage in this latter respect of leaving an opening, though it may be doubted whether another allegation in its favour will be found generally true—viz., that when the periosteum below, and the Schneiderian membrane above, are left, these two membranes being placed in contact, the bone is regenerated. The after-treatment of a second case is thus described:

"During eighteen days, rugination was practised, by means of various instruments, upon the remainder of the polypus, the surface of which was more than two

inches in length, and nearly an inch and a half in breadth. At the expiration of that time, cauterization, by means of the electrical apparatus, was had recourse to. Ten days afterwards, it was cauterized by means of mono-hydrous nitric acid; the small platina wire of the electric cautery not destroying fast enough. The acid was used for a long time; at each application being left in contact with the part, four, five, or six minutes. On account of the stifling vapours of this acid, so irritating to the air-passages, the cotton dipped in it was pushed through glass tubes, whose section was more or less oblique, according to the surfaces against which it was to be applied. One inconvenience must also be attended to in this operation, that the ball of cotton does not fill the tube; for if it does, the vapour cannot escape at the sides as you push it with the glass rod, and it is driven into the throat. These applications demanded the greatest patience, both of the surgeon and of the patient; they were constantly made use of for four or five months, when but a small portion, about the size of the end of the thumb, was left. As this nipple-shaped lump was being destroyed, the projection of the eye was much diminished. M. Nélaton thought it to be a prolongation of the polypus into the zygomatic fossa, through the hole of the ganglion of Meckel—a part at which its adhesions were only membranous, not periosteal. In the course of another month, the patient left the ward, cured of his polypus." (Nélaton, p. 424.)

From the description of a third case, which terminated fatally, we should be disposed to think that the disease was, in that instance, of cancerous nature. A prolongation of the tumour entered the cranium, and compressed the optic nerve. The patient had complained of sharp pain in the head, while his right pupil was much larger than the left, and immovable.

Lithotomy through the Rectum.—From some interesting cases of lithotomy we abridge the following:

A man, fourteen years before admission, had passed yellowish urine, mixed each time with a teaspoonful of sand. Within the last eight months, he had stopped passing the sand, and begun to pass blood, to micturate every quarter of an hour, and to become weaker and thinner. No stricture of the urethra was detected with the sound, yet the whole of the membranous and spongy portions were felt to be more narrow than usual. At the prostatic portion, every time the sound was pushed so far, a foreign body was detected; there were evidently one or more calculi there. Whenever the urethra was touched from the rectum, a peculiar sensation, one arising from the rubbing together of many fragments, was distinctly experienced. When the finger was so placed as to touch the anterior extremity of the prostate, and a sound was passed into the urethra as far as the point of the finger, a very slight push at once brought the instrument in contact with a stone. There were calculi, then, in the prostate. The wall between the rectum and urethra was so thin, that it seemed to indicate a dilatation of these parts; and when a straight sound, with a small portion of its extremity, bent at a right angle, was passed as far as the prostate, a movement of rotation was easily made, and the dilatation of that portion of the urethra into a true *prostatic pocket* was ascertained. Moreover, there was a large calculus in the bladder. M. Nélaton believed the small stones in the pocket of the prostate to be fragments which had been deposited there after the *spontaneous fragmentation of the large calculus in the bladder.* The patient had in his possession a small fragment, evidently a shell from a large mass. M. Nélaton said, that calculi in the bladder seem sometimes to be broken up by an *interior* force.

"Lithotrity was rejected in this case, on account of the narrowing of the urethra in front of the dilated prostate, and the diseased condition of the bladder; but the following operation was performed. The thumbs were inserted into the anus, and dragged asunder until they touched the tuberosities of the ischium, and an interior rupture was felt. After this proceeding (which is that recommended by M. Réca-

mier, and adopted by M. Nélaton, for the cure of fissure of the anus), the anterior part of the rectum can usually be seen, as well as the perinæum. However, the struggles of the patient under the influence of the chloroform, occasioned a prolapsus of the rectum through the now useless sphincter ani; and M. Nélaton proceeded by feeling, instead of by sight. With one stroke of the bistoury, he cut down upon the sound through the rectum and urethra; he then enlarged the opening by several strokes of the knife, and then gliding the lithotome through the opening into the bladder, he opened and withdrew it, cutting the lateral parts of the prostate. He found at once the calculus, which was at least two and a half inches in diameter. After it had been extracted, by introducing his finger into the prostatic pocket, he found numerous calculi, or rather fragments of another calculus. M. Leroy d'Etiolles, who was present at the operation, was afraid that all the fragments were not extracted, for, on putting them together, *a whole* was not formed; but M. Nélaton did not participate in his fears, as he thought these fragments had, at one time or other, been detached from the large calculus, upon which again deposits had been formed. At all events, it was impossible to find any more pieces.

"For three or four days the patient went on well; on the evening of the fifth day, he had some inclination to vomit, and tenderness above the pubes. On the seventh day pain came on in the left renal region; then appeared dyspnœa, smallness of the pulse, and cold extremities, and also what the French call *la voix cassée*, the worst sign in all such abdominal cases, as, for instance, strangulated hernia. Opium was the chief treatment, but the patient died. There is no record of the examination of the body." (Nélaton, p. 673.)

The Actual Cautery.—We had expected to find a much more extended account of the value and proper application of this remedy than is contained in Mr. Atlee's work. The cautery is evidently in frequent use in M. Nélaton's wards; and from one passage in this book it is clear that the Professor thinks highly of it. He is reported to use it most frequently in chronic affections of joints, and by repeated applications of it to succeed in their cure. Although the employment of anæsthetic agents enables surgeons now-a-days to apply the cautery both readily and freely, it does not appear to us that we are left to this unpleasant resource in the treatment of chronic diseases of joints, and we are not aware of the adoption of that practice by any English surgeons. But the case is far different with certain acute diseases of the articulations. There is a class or a stage of these diseases, in which joints undergo a very rapid destruction, and in which the characters of severe pain, tenderness, and immobility of the joint, with agonising startings of the limb during sleep, denote the existence of an inflammatory ulceration of the cartilages and of the thin layer of compact articular bone beneath them. Such symptoms occasionally yield to the treatment ordinarily recommended in systematic works, but they yield slowly, and too often, in spite of treatment, they are followed by suppuration and the destruction of the joint. Now it is in these cases that the power of the actual cautery is most strikingly shown. A pale, emaciated boy came under our care at the Middlesex Hospital. He was nine years of age, and had the fretful anxious look and the petulant temper of one who had been reduced and deprived of his rest by acute suffering. All his attention was fixed upon his left knee, which was bent, shining and tender, a little swollen above the patella, and capable of a very slight amount of flexion and extension. The most trifling motion, however, caused pain, which, when the limb started, as it did during his sleep, was very severe. Only four

weeks before his admission, he had been stout and healthy; then he began to limp, and then to suffer pain. After that, severe sympathetic fever came on, and the symptoms with which he was afterwards admitted.

Mercurial and camphor ointment was freely applied, beneath a poultice, to the knee, and he took a sedative draught at night.

On the fourth day there was no relief to the pain, and the knee contained more fluid. His look was still fretful. Chloroform was given him, and the actual cautery freely applied to a large extent of the surface over and about the knee. The limb was wrapped in cotton wool. The sedative draught was continued. The pain was at once relieved, and the boy's tone and appearance speedily improved. In a fortnight, the skin of the knee had entirely recovered from the effects of the cautery, but the fluid in the joint had not diminished, and a little tenderness still remained. A couple of blisters were put on, and when the second had healed, the articulation was of natural size, free from pain and tenderness, and capable of motion. After this, he began to walk about, and was discharged quite well.

A remedy so potent in controlling violent local disease, finds its use under other circumstances also; and we have employed it with satisfaction when under the necessity of making incisions into large articulations, which retained their natural structures. Incisions into joints full of pus, of course afford more relief than injury, whilst natural joints mostly resent incisions by a severe inflammation. Our experience of its effect under such circumstances would lead us to employ it also in the accidental injuries, in which large joints are opened by incision, and to expect that more cases might terminate favourably than now escape the issue in anchylosis, amputation, or death.

The point of greatest importance in the selection of cases for treatment by the cautery is, that suppuration should not be established in or about the joint. When an abscess has formed, the cautery will relieve the pain, but not arrest the disease, and the surgeon will be disappointed in its use. There are, however, cases in which it is impossible to determine whether the disease of the joint has reached the stage of suppuration; the cautery may be used under those circumstances, and may sometimes prove efficacious in stopping suppuration, even when it has actually begun. A strumous lad, aged fourteen, was under our care in the Middlesex Hospital, for necrosis of the femur. He continued tolerably well, until pains came on in his shoulders, and in a few days his right shoulder was considerably swollen by an effusion of fluid underneath the deltoid muscle. The swelling subsided after the application of a large blister to the surface. In a few days more, pain came on about the thigh and fistulous orifice, an ill-developed erysipelatous redness appeared about the fistula, and he became very low. This was followed by pain down the fibula, proceeding from a swollen part on the outer side of the ham, and on pressing that part, some pus, mixed with large yellow globules like synovial fluid, escaped through the sinus. The pain increased in the night, and the next morning the knee-joint was tensely full of fluid, and extremely tender and painful. During the following three days it became larger and yet more tense, and the pain was not relieved. By a long subcutaneous puncture with a trocar and canula, we let out

from the joint four or five ounces of opaque, turbid, light greenish-brown, serous fluid, mixed with many whitish flocculi of puriform character, and some gelatinous lymph. Examined beneath the microscope, the fluid was found to contain a good deal of pus, though the appearance presented to the naked eye did not certainly indicate it. In two days the joint was full of fluid again. An attack of erysipelas came on in the thigh, during which the fluid was in part absorbed from the joint. In three days more, severe pain had begun again, and his state required the adoption of some decided treatment in order to save the limb. The actual cautery was applied, and an opiate given. The effect was immediate. He awoke cheerful, and the pain never returned. Free discharge took place from the charred surfaces, and he had a renewed attack of erysipelas of the thigh. The swelling of the knee subsided, the joint recovered, and the following year he was in excellent health, stout, ruddy, grown, and with his knee well, but there was no change in the state of the femur and sinus.

Review III.

Hospitals-Meddelelser. Anden Række. Udgivet af C. E. Fenger, Dr. med. Professor ved Universitetet, Overlæge ved det kgl. Frederiks Hospital. Første Bind.—*Kjøbenhavn*, 1856.

Hospital Communications. Second Series. Edited by C. E. Fenger, M.D., Professor to the University, Principal Physician to Frederik's Royal Hospital. First volume.—*Copenhagen.* 8vo, pp. 576.

Under the modest title of 'Hospital Communications,' we are presented, in the volume before us, with a series of valuable essays upon some of the most important and interesting subjects which just now engage the attention of the practical physician. A brief analysis of the work will not, we trust, prove unacceptable to our readers, and it will, we think, show, that although the labours of our Danish brethren have hitherto been less known to us than those of our colleagues in many other states of continental Europe, they are not less worthy of our consideration.

In the preface we are informed that the first series of the journal, the numbers of which were issued bi-monthly, was discontinued at the end of the year 1853. The plan of publication is now changed; the present cannot in fact, strictly speaking, be called a periodical, as it does not appear at any stated interval, but is issued only when the editor finds himself in possession of sufficient material to fill a volume. It is argued that this system is better suited to the circumstances of Denmark, where the labourers in the field of medical science are not very numerous, than the plan which compels the editor to have a certain number of sheets ready on a given day; and that it admits of the introduction of longer essays without the necessity of interrupting them once or oftener, and obliging the reader to wait several months for their continuation.

I. The first paper in the present collection is by Dr. E. Fenger, and is on the subject of "echoes" in the human thorax. We may state the case in the words of the author himself, who, after some preliminary observations, remarks that—

"Every one who has paid any attention to auscultation knows, that when at one side of the chest one of the pathological conditions has occurred, by which a strongly bronchial character of the respiratory and vocal sounds is produced, and when this morbid change is situated in the most interior portion of the posterior surface of the chest, close to the vertebral column, these sounds are heard not only at the innermost part of the ribs, but also on the vertebral column, and not only at the affected side of the latter, but also at the sound side. This is simply a phenomenon of the conducting of sound, which admits of a ready explanation; for the diseased part lies in immediate contact with the dorsal vertebræ, consequently the latter must, as good conductors, be able with ease to convey the abnormal sounds to the surface; and as they are situated in the middle line of the body, and do not consist of two distinct lateral portions, but form a connected whole, there is no reason why they should propagate the sound less to one side of their surface than to the other, or why the abnormal sounds should be less distinctly heard at the healthy side of the spinous processes than at the other.

"But in a portion of these cases we will find, that on removing the ear farther out on the healthy side of the chest, a bronchial character of respiration and of the voice is still audible at a certain distance from the vertebral column; we will further find that these phenomena, after having disappeared at a short distance from the spine, again occur or are increased at a greater distance from the same—for example, at the inner edge of the scapula; indeed in some cases we may even observe that they continue much further outwards towards the axilla and the side of the chest." (p. 5.)

It is particularly when the affected parts are situated near from the third to the fifth dorsal vertebræ, and consequently comprise the inner part of the posterior surface of the lung, especially the lower portion of the upper lobe or the upper portion of the lower lobe, that these phenomena are perceived, and they consist in this:

"That bronchial respiration and bronchophony are heard on the healthy side of the chest, where neither percussion nor the symptoms indicate the existence of any abnormity. The bronchial respiration is heard in most cases only as a prolonged and blowing expiratory sound, or as a short though very brisk puff at every expiration; but at other times a very distinct bronchial inspiratory sound is also heard, either with or without superadded vesicular inspiration. The sound of expiration alone is changed in the cases in which the phenomenon is weak; the inspiratory also is modified when it is strong." (p. 9.)

In pleuritis, ægophony is, in like manner, heard on the healthy side.

"The sounds are heard, as already remarked, in the vertebral column, and in the space between this and the scapula. When the phenomenon is well marked, the echo will be heard in the entire of this region, or in the greatest part of its extent from above downwards. But when it occurs in a slighter degree it will be observed that it is not equally distributed over this space, and it will often be found that if the ear be carried from the point of the vertebral column where the sound is strongest, in a horizontal direction outwards towards the scapula, the sound disappears either partially or completely at a short distance from the spine, to become again more audible towards the angle of the ribs, or close to the superior or inferior angle of the inner edge of the scapula. If we now examine more accurately, we shall find that the sounds in such cases seem to follow linear paths, which proceed from a point near the third or fourth dorsal vertebra, and thence diverge towards the inner edge of the scapula, the most superior path running out towards the upper angle of this bone, and the most inferior towards the lower; and that between these there are one, two, or at most three similar paths extending with less deviation from the horizontal direction between the point just mentioned and the scapula; the weaker the phenomenon is, the narrower are these paths; if it becomes stronger they spread and coalesce. Most frequently they disappear at

the inner edge of the scapula; but it is not unusual to find them passing over this, so that they can be heard at the inner part of the bone itself, both above and below its spine, and at its inferior angle; indeed, in some cases I have been able to follow it still further, and then chiefly in two directions—either down over the inferior angle of the scapula and round to the side of the chest towards the inferior edge of the lung, or horizontally along the spine of the scapula to the acromion process, and thence into the axilla." (p. 10.)

The author next proceeds, by the recital of a number of cases, to remove any doubt as to the existence of these echoes which might arise from the suspicion that the sounds in question might have been due to incipient pneumonia or pleuritis in the so-called healthy side. That this was not the case he shows from the clearness of the sound elicited on percussion, the absence of crepitus, and the fact that the echoes occur too frequently to allow of their being with any probability attributed to double pneumonia or pleuritis, which are rare diseases; but lastly, the decisive proof is furnished by the fatal cases and the results of post-mortem examination.

With regard to some of the other physical signs derived from auscultation, the author has never succeeded in hearing the pleuritic friction-sound propagated to the other side of the chest; the finer râles, especially the crepitating, are usually heard only on the side on which they are developed. The author quotes a case to prove that it can in some instances be demonstrated that sounds formed in cavities may give an echo on the opposite side, and from his observations draws the practical inference that the greatest caution is necessary in the diagnosis of double pneumonia or pleuritis, as well as, under certain circumstances, of cavities in both sides of the chest, especially when they manifest themselves posteriorly. In some cases, too, we shall be able, with the aid of the echo, to diagnose a central pneumonia, or a pneumonia which has not yet approached the surface of the lungs, before it can be recognised by means of the characteristic phenomena on the affected side; but the author considers that the greatest importance of the echoes, at least just now, is in a theoretic point of view, especially with reference to the explanation of the bronchial character of the respiration and voice in pneumonia and pleuritis.

Having passed in review the theories by which Laennec and Skoda have sought to account for the production of these phenomena, as well as the objections brought forward against the views of the latter distinguished observer by Dr. Walshe, Dr. Hoppe of Berlin, and Professor Wintrich of Erlangen, the author proceeds to observe, that in the present position of the argument there are three among the questions which have been proposed, the solution of which is of especial importance, namely—

Is the hepatized pulmonary tissue a better conductor of sound than the healthy?

Can the sound formed in the rima glottidis be, under certain circumstances, magnified in the bronchi? and, if this be answered in the affirmative,

Is this strengthening of the sound to be attributed to the occurrence of consonance?

He considers that the first question can as yet scarcely be looked upon as having been satisfactorily answered, and in a note he informs us that

he has himself commenced a series of experiments which he hopes may prove decisive of the point. He is of opinion that in inflammation of the chest the respiratory murmur and the voice are modified and magnified in the bronchi, and that the theory of Laennec, who referred the phenomena of bronchophony and bronchial respiration mainly to improved conduction of the sounds from the bronchi to the surface of the chest, is incorrect. He thinks the least improbable of the explanations which have been advanced, to be that which attributes these phenomena to the reflexion of the acoustic waves from the bronchial walls in the hepatized or compressed pulmonary tissue, and their concentration in the cavity of the tubes; but adds that much remains to be done before this theory can be looked upon as either established or refuted.

In concluding the foregoing very brief abstract of Professor Fenger's valuable paper, we would merely remark that the question will still naturally suggest itself, whether the phenomena described by him may not be the results of a simple conduction of sound, and whether the term "echo" in such a case should not be looked upon as merely expressing the existing state of things, and not as representing a theory by which it may be accounted for? The disappearance of the sound in some places, and its reappearance at a greater distance, would not disprove its conduction, as it might follow the course of denser tissues, which may be much more deeply seated in some parts than in others. If, however, we have succeeded in furnishing our readers with a sufficiently clear view of the author's statements, their practical importance will be obvious; the subject is one which calls for accurate investigation at the bedside; we therefore leave it for the present, and pass to the Essay of F. Howitz, candidate of Frederik's Hospital, On the Behaviour of the Combinations of Chlorine, and especially of Common Salt, in the Urine, under several Pathological Conditions.

II. Herre Howitz has himself made upwards of 600 quantitative analyses of the urine, and he prefaces his paper with a full description of his mode of proceeding; we shall, however, be able to occupy ourselves only with his results. He takes as his starting-point the conditions laid down by A. Hegar, in a treatise published at Giessen, in 1852, as influencing the excretion of chlorides in the urine in healthy individuals: these are, the amount of common salt contained in the food; the mode of life and constitution of the individual; the period of the day, the elimination of chlorides being greatest in the afternoon and least at night; towards morning it again increases, even though no chloride should have been ingested. Exercise favours their excretion, as do copious draughts of water; but after their quantity is increased by the free ingestion of fluid, it again sinks considerably. Something similar takes place during strict abstinence from food, the elimination of the chlorides gradually diminishing; and if much common salt be given after fasting, it increases but slowly, showing that much of the salt has gone to replace what the blood and tissues had lost during abstinence. In the normal state, the excretion of chloride of sodium never entirely ceases; if none be ingested, it is eliminated at the expense of the blood and tissues. If a considerable quantity of common salt be taken, the excretion is immediately largely increased, about in proportion to the ingestion, but it quickly sinks again nearly to

the ordinary amount; an excess of the chloride of sodium must consequently remain in the system, which manifests itself by the peculiar symptoms characteristic of the free use of the salt, as a feeling of fulness, distension of the abdomen, &c.

As a preliminary to his investigations, the author ascertained the amount of salt contained in some of the articles of diet used by the individuals submitted to experiment. Into this part of the subject, however, we need not enter, but it may interest some of our readers to be made acquainted with the nature of the fever dietary of the Danish Hospital. There are two fever diets—the half and the full; the former consists of a "portion" of oatmeal gruel, two spiced biscuits, and one *pægel* (rather more than half a pint) of milk; the full fever diet consists of two small French rolls, half an ounce of butter, a portion of oatmeal gruel, a portion of fish, and one *pægel* of milk.

The amount of chloride of sodium eliminated through the urine depending, in health, in the absence of any peculiar circumstances, on the quantity ingested in or with the food, and being usually about equal to it, the author divides diseases, in reference to the subject of his paper, into three principal classes—namely: 1. Those in which, in a certain time (twenty-four hours), about the same quantity of common salt is excreted with the urine as is, during the same period, ingested with the food, and where the elimination proceeds, in other respects also, as in the healthy individual. 2. Those in which the elimination exceeds the ingestion, none of the causes capable of producing in health a similar disproportion being present. 3. Those in which the amount excreted is less than what is taken in as food.

Among the diseases the author found to be referable to the first class were—typhoid fever, simple bronchitis, capillary bronchitis, gangrene of the lungs, polydipsia, chlorosis, measles, organic diseases of the heart, and several chronic diseases in which nutrition must be looked upon as being considerably affected—as, for example, cancer of the uterus, &c. Ague, hæmorrhage of the brain, traumatic meningitis, tumours in the brain, affections of the spinal marrow (softening), and epileptic fits, were also found to belong to this class. In two cases of rheumatic tetanus, the excretion of chlorides was considerably diminished, but scarcely more so than should have been expected from the small quantity of food the patients could take. During the use of calomel in one of these patients, the elimination rapidly and considerably increased, but sank again immediately after its use was discontinued. The proportion of chlorides was likewise normal in affections of particular nerves—for example, in what appeared to be a rheumatic affection of the seventh pair of cerebral nerves.

In rheumatic fever, the author expected to find a special ratio between the excretion of chloride of sodium and the serous exudations and infiltrations, but this did not appear to exist. When, in the course of this disease, serous effusions took place suddenly, either into the serous cavities or into the areolar tissue, the elimination of common salt usually sank considerably; but this did not occur rapidly, but gradually, and in proportion to the want of appetite. Neither did he observe any sudden great increase of the quantity eliminated to attend the disappearance of these effusions during convalescence.

A. Vogel found the chloride to be proportionate to that contained in the food also in Bright's disease, in calculous affections of both kidneys, and in carcinoma of the liver.

The second class comprises those cases where the elimination of chloride of sodium through the kidneys exceeds the amount of the salt ingested, and is limited to cases of the rapid absorption of serous effusions. A. Vogel had laid down the rule, that, under these circumstances, the quantity of common salt eliminated was augmented in proportion to the increase in the amount of urine excreted. The author reports, from his own observation, the case of a country girl, aged nineteen, of good constitution, in whom extensive serous effusions into the peritoneum and left pleura were rapidly absorbed. The amount of chloride excreted was increased in two days from 6720 to 23,000 milligrammes; nor was the increase merely in proportion to the augmented flow of urine, as stated by Vogel; for on the 11th October, thirteen cubic centimètres of urine contained but five milligrammes of common salt, whereas, on the 18th, the same quantity contained 300 milligrammes. The author remarks that, during the existence of a serous effusion, we may perhaps find in every increase of chloride of sodium in the urine, when this is both relative and absolute, a sign of a rapid absorption of the effusion, and that, *cæteris paribus*, we may, from the more or less abundant elimination of the salt, infer the rate of the absorption. It is evident that a case of pleuritis will, during the rapid absorption of effusion, belong to this class, while during the formation of the effusion, it should be referred to the following.

The third class, as already stated, includes those affections in which the quantity of chlorides eliminated by the kidneys is less than the amount taken into the system by the mouth. The first disease we meet with under this head is pneumonia. The author has quantitatively analysed the urine, by Liebig's method, in a considerable number of cases of various species of pneumonia, and of these investigations he details eight carefully-reported examples. The following are the conclusions he feels himself justified in deducing; they differ in some respects from those arrived at by Dr. Lionel Beale and A. Vogel, who believe that in pneumonia the elimination of chloride of sodium through the kidneys may even altogether cease.

"(*a*.) In individuals labouring under pneumonia, the normal ratio between the amount of chloride of sodium eliminated in the urine, and that brought into the system with the food, is altered; a less quantity being excreted in a given time than is ingested during the same period.

"(*b*.) The elimination of common salt never entirely ceases.

"(*c*.) The diminution in the excretion does not proceed *pari passu* with the hepatization.

"(*d*.) It manifests itself at all ages and in both sexes.

"(*e*.) It takes place whether the patient suffers from the so-called croupy pneumonia, or from lobular, traumatic, or hypostatic pneumonia.

"(*f*.) It occurs without reference to treatment by venesection.

"(*g*.) During pneumonia the administration of large doses of chloride of sodium does not exercise its usual influence on the excretion, as its elimination is not increased nor altered until resolution of the pneumonia sets in.

"(*h*.) The special cause of the remarkable change in the amount of chloride of sodium eliminated in this disease is to be sought in the pneumonia itself, in the circumstance that the pulmonary tissue is attacked in a peculiar manner, and that the respiratory conditions are altered." (p. 118.)

The blood of individuals suffering from pneumonia is found to be unusually deficient in chlorides, consequently it cannot be assumed that a greater quantity than ordinary of chloride of sodium is retained in that fluid; nay, on the contrary, it is evident that the latter must have parted with some of its common salt. A circumstance which demands attention is, that the expectoration in pneumonia is found to be much richer in chlorides than that in other diseases.* Putting all these particulars together, we are led to infer that a special consumption of common salt and of chlorides takes place in pneumonia, and that it is probably in the peculiar exudations in the pulmonary tissue that this consumption occurs, but how or why it happens is still completely a riddle.

The author reports three cases of acute pleuritis in which, during part of the time when effusion was taking place, the elimination was much diminished; contemporaneously with the absorption of the fluid it was largely increased.

In two cases of violent flying rheumatic pains, the result of the exposure of healthy individuals to draughts of cold air, the elimination was greatly diminished during the pain, but rose remarkably on its cessation.

In a patient labouring under erratic erysipelas, a remarkable and sudden diminution took place, and continued for about a week, when the elimination suddenly rose without any alteration in the diet sufficient to account for the change.

III. Herre Howitz's paper is followed by one by H. Krabbe, likewise candidate of Frederik's Hospital, On Some Cases of Hydatids in the Human Subject. Five are reported—three of cysticerci, all in the brain and its membranes, and two of echinococci, in one case located in a cavity of the right lung, in the other in an abscess as large as a duck's egg, situated at the left side of the spine, at about the fifth dorsal vertebra, between the bodies of the vertebræ and the ribs, under the ligamentary apparatus. The author observes that the occurrence of echinococci in the lung is not so very rare, while it is certainly more uncommon to meet them in the spine.

IV. The next is an elaborate paper by E. Silfverberg, Physician Extraordinary (*Reservelæge*) to Frederik's Hospital, on the subject of Gangrene of the Lung, which disease the author divides into the idiopathic or primitive, and the consecutive; the latter may be the result of, 1st, acute or chronic inflammation of the lung; 2nd, of pulmonary tuberculosis; 3rd, of apoplectic foci in the lung; 4th, of acute or chronic dilatation of the bronchial tubes.

The author furnishes us with some interesting statistics of the disease, based entirely upon cases observed in Frederik's Hospital. Thus he found that the gangrene occurred:

Idiopathically in	28 cases.
After acute inflammation of the lung in	3 "
After chronic inflammation of the lung in	2 "
After acute dilatation of the bronchial tubes in . .	2 "
After chronic dilatation of the bronchial tubes in .	3 "
Total	38 cases.

* Beale, Vogel.

He also examined forty-five cases in reference to the seat of the disease, and found that it occupied

The right lung in		22 cases.
And of it the upper lobe was engaged	9 times.	
The middle lobe was engaged	twice.	
And the lower lobe	11 times.	
The disease was situated in the left lung in		14 cases.
The upper lung being engaged in . . .	3 cases.	
And the lower in	11 „	
It occupied several lobes of the same lung in		6 cases.
And both lungs in		3 „
Total		45 cases.

"The disease has consequently exhibited itself more frequently in the right lung than in the left, and in the lower than in the upper lobe of the left; while on the right side it has occurred with about equal frequency in the upper and lowest lobe; the above result does not, however, agree with what is usually stated. It will be seen that it is not very unusual to find several lobes simultaneously affected, while, on the contrary, it is comparatively rare to find gangrene in both lungs." (p. 202.)

As to the etiology of the disease, it occurred among forty-five cases, thirty-three times in men, and twelve times in women—that is, in the proportion of eleven to four, exactly as stated by Laennec. It might be hence inferred that it is especially likely to be met with in those who are dependent on casual labour for their support, and who are consequently much exposed to vicissitudes of temperature; but it must be borne in mind that this class is, more than any other, addicted to the abuse of spirituous liquors, a vice which seems particularly to favour the development of the disease. Of the forty-five patients, no fewer than sixteen were labourers, and three followed professions which compelled them to take exercise in the open air. Besides the abuse of spirits, ordinary catarrhs, and, in a less degree, the puerperal state, appear to be predisposing causes, and the disease also occurs not very rarely in combination with serious chronic abdominal affections.

Gangrene was met with in drunkards in	8 cases.
„ „ in puerperal women	2 „
„ „ in patients with chronic abdominal disease in	3 „
„ „ in patients subject to bronchitis in	5 „

As to age, it was found to occur,

At 15	once.
At 17	once.
Between 20 and 30	12 times.
„ 30 and 40	7 „
„ 40 and 50	12 „
„ 50 and 60	8 „
„ 60 and 68	4 „

"It is usually stated," observes the author, "that age does not seem to have any essential influence as a predisposing cause of gangrene of the lung; but, on

the one hand, the disease is said not to be uncommon in children after exanthematous fevers; and on the other, it would at least appear from the foregoing table, that persons are more liable to it after than before the age of twenty years; on the whole, it might perhaps be assumed that the disease is rarest between the tenth and twentieth years of life." (p. 205.)

Under the head of symptomatology and diagnosis, the author remarks, that pulmonary gangrene may begin with the signs of an affection presenting little danger; it is only in a few cases that its true nature can be recognised immediately after its commencement; it usually exhibits itself in other and different modes, which may be referred to the following forms of disease:—A. Pneumonia; B. Pleuritis; C. Hæmoptysis; D. Pulmonary catarrh.

A. From his observations on the first variety, the author deduces the maxim, that

"When gangrene, proved by dissection to be such, began during life with all the characteristic signs of inflammation of the lung, it has been consecutive to the latter disease; while, on the contrary, in the idiopathic gangrene occurring with the signs of pneumonia, it will be found that one or other of the most important symptoms of the latter disease was wanting." (p. 219.)

B. Of the 45 cases at Frederik's Hospital, the gangrene began in 5 with signs of pleuritis; in reference to such cases, the author infers that

"Gangrene of the lung may begin with precisely the same symptoms as pleuritis, and in such cases cannot be distinguished from the latter disease until its characteristic symptoms have set in." (p. 223.)

C. But one case was met with of gangrene of the lung with symptoms of hæmoptysis, and therefore the author draws no special conclusion with respect to this variety.

D. Lastly, the author infers that "Gangrene of the lung may set in without any other than the apparently unimportant symptoms which characterize a slight pulmonary catarrh."

The author is especially anxious to draw attention to certain characteristic marks of the earlier stages of the disease described in his Essay, particularly the odour of the expectoration, which is, he says—

"At first flat and mawkish, but soon assumes a very peculiar character, to which I attach especial importance, as by its aid we are in a position to diagnose incipient gangrene, and it is a sign which I have not found described elsewhere." "It is characterized," he adds, "as a far from offensive, but, on the contrary, even very agreeable balsamic smell, most closely resembling that of myrrh. It is not very rarely observed, but has by no means been perceived in all cases. This circumstance I am, however, strongly inclined to attribute to attention not having been earlier drawn to it, as, since it was first perceived, it has been found in almost all the more recent cases. It may continue for a very long time." (p. 231.)

The next symptom on which Dr. Silfverberg lays particular stress, is the fact that the putrid cadaverous smell observed in the expired air at a later period of the disease, during ordinary respiration, is, at an earlier stage, perceptible, even simultaneously with the balsamic odour of the expectoration, on expiration during cough.

The third of the characteristic marks alluded to is, that

"After bronchial respiration has first appeared, there has, in all the accurately observed cases, occurred a period in which its strength is much diminished, or

when it may have altogether ceased. . . . But it often happens that the bronchial sound returns with increased intensity, as is seen in the first, and partially in the seventh example." (p. 235.)

Dr. Silfverberg gives the following statistics of the duration of pulmonary gangrene:

From 6	to	90 days in	. . . . .	5 cases.
„ 3	„	4 weeks in	. . . . .	3 „
„ 5	„	6 „	. . . .	4 „
„ 6	„	7 „	. . . .	4 „
„ 60	„	70 days in	. . . . .	3 „
About		90 „	. . . .	1 „
„		130 „	. . . .	1 „

V. Dr. Silfverberg's Essay is followed by a Paper by Dr. Fenger, entitled Practical Observations on Cardialgia and its Treatment. After an extended and interesting review of the several diseases in connexion with which cardialgia may sympathetically exist, Dr. Fenger alludes to its endemic occurrence in the North, and refers, on that subject, to Dr. Huss' excellent work 'On the Endemic Diseases of Sweden,' noticed at length in a former number of this Review.* Dr. Huss attributed the great prevalence of cardialgia in Sweden to the unwholesome and innutritious nature of the ordinary diet of the people, as well as to the abuse of brandy by the men, and of coffee by the women. Dr. Fenger doubts the correctness of this opinion, and argues that in Copenhagen the disease is very prevalent among the female servants, who in general are well fed, and certainly eschew "both sour bread and oaten bread and 'Krösamos.'"† The use of brandy, he adds, is unknown among this class; and coffee is not much more used by them than by other orders of the population, and certainly less than by the proper labouring classes, who diet themselves. Dr. Fenger considers that the cause of this endemic form of the disease, which he proposes to call Idiopathic Cardialgia, is still unknown, and that we are consequently not as yet in possession of any proper rational method of treating it.

The author considers at some length the principal symptoms of this form of disease, as tenderness of the epigastrium and other parts, sometimes including the dorsal vertebræ (spinal irritation), contraction of the recti muscles of the abdomen, epigastric pulsation, epigastric fulness, &c., and concludes his valuable paper with judicious observations on the treatment of the affection, in the course of which he points out the efficacy of quina in the comparatively rare cases where the attacks of pain present a regular daily intermission. In the less regular forms of intermittent cardialgia, in which the attack of pain comes on at an uncertain period of the day, or perhaps altogether omits a day, the author has found much benefit from the use of nitrate of silver, administered in the form of pills, each containing one-eighth of a grain compounded with powdered marsh mallow. It is, in fact, in this particular form of cardialgia only that this remedy appears to be useful. He commences with three pills in the day, increasing the number in the course of a few days to ten or twelve,

* Vol. x. p. 365. The causes of the endemic dyspepsia, as assigned by Dr. Huss, will be found at p. 376 of the Review referred to.

† Sour milk, in which are boiled the berries of the whortleberry or bilberry. (*Vaccinium Vitis Idæa.*)

taking care not to continue their use for longer than a month, lest discoloration of the skin should be produced.

VI. We next meet a Contribution to the Theory of Emboli,* by A. Brünniche, Physician Extraordinary to Frederik's Hospital. The cases on which the author founds his remarks are two in number,—the first, that of a labourer aged fifty-four, is described as one of "dilatation with fatty degeneration of the heart, atheromata in the aorta, plug conveyed into the basilar artery, traces of hæmorrhage in the substance of the pons Varolii and left optic thalamus, abscesses in the perineum;" the leading features of the second, occurring in a servant girl aged twenty-one, were "arthritic fever, endocarditis, and hypertrophy of the left ventricle of the heart, emboli in the arteries of the pia mater, softening of the brain, hypertrophy and fibrinous infarction of the spleen, scurvy and deficiency of blood in all the organs; general dropsy."

"It may," observes Dr. Brünniche, "be considered as established by the foregoing cases, that embolism is really a diseased condition, capable of occurring under certain circumstances, and of giving rise to a series of morbid phenomena, which hitherto we have been in part accustomed to ascribe to other morbid processes, and which often even occasion death. It is therefore important to make ourselves more accurately acquainted with this disease, which has been nearly simultaneously described by Virchow in his 'Handbuch d. Spec. Pathol. und Therapie,' Band 1, Heft i., and by Dr. Senhouse Kirkes in the 'Medico-Chirurgical Transactions,' vol. xxxv., 1852.

"Every foreign substance which is conveyed in the current of the circulation to be deposited in another part of its course, may, in the widest sense of the term, be called embolus. Although, however, the theory of the reception of pus into the blood and its conveyance with its stream, has long played a part in pathology, and Kirkes also has described as a form of embolism the taking up by the blood of the finest molecular matters, which are, according to him, capable of giving rise to diseases of a purely typhoid character, it will yet be most correct for the present only to consider embolus as a more solid body of some palpable volume. Such may of course be any foreign bodies introduced from without, which are carried along with the stream of blood after having entered a vessel, or also natural or pathological products from the vascular system itself. Thus, portions of the valves of the heart, of atheromata, of ossifications, have been shown to act as emboli. Finally, what is most usual, fibrinous exudations or coagula of blood from the cavities of the heart or vessels.

"This transference of solid masses from one part of a vessel to another may naturally take place both in the venous and in the arterial system. As we must assume that the blood in the veins, by reason of its slower and more even course, the weaker impulse, and its more superficial situation, whereby it is more exposed to external influences, is more liable to coagulation, we should also expect that emboli should here more frequently occur. However, it is perhaps owing just to the greater weakness of its current that plugs are here less likely to be carried away, though it may be that this occurs more frequently than we suppose—at all events, this process has as yet been studied only under certain morbid conditions, as dropsies, phlegmasia alba dolens, &c., while it is possible that they may be conveyed from the veins, through the right side of the heart, to the pulmonary artery—a passage, the possibility of which has been demonstrated by experiments, and which might indeed explain certain cases of sudden suffocation; for example, during the puerperal state.†

* Emboli, from ἐμβολὴ, injection, a term applied by Virchow to fibrinous concretions detached from the heart or great vessels, conveyed in the current of the circulation to a distance, and arrested and producing obstruction in remote parts of the vascular system.—Rev.

† M'Clintock: L'Union Médicale. 1853. Dr. M'Clintock's paper here referred to, On Sudden

"In fine, we must look upon emboli as mainly an arterial phenomenon which at one time manifests itself in the pulmonary artery, when the plug comes from the right side of the heart, or through the latter from the veins; at another, in the arteries of the general circulation, when it proceeds from the route of the arterial blood—the pulmonary veins, the left side of the heart, or the larger arteries;—at another, in the ramifications of the vena portæ in the liver, when it arises in the roots of this vessel." (p. 333.)

The author proceeds to examine the courses which emboli are more likely to follow; the situations where they are more liable to be arrested; the predisposing and proximate causes of their manifestation; the immediate effects of their impaction; the changes they may subsequently undergo—either a kind of fatty metamorphosis, by which they are reduced to the state of a loose detritus, capable of being washed away piecemeal by the force of the circulation; or organization in the seat of their impaction, giving rise to ulterior results, some of which have been briefly described in a former number of this Review;* the symptoms to which they give rise according to the part affected; in a word, in a very few pages Dr. Brünniche presents his readers with an extremely lucid view of this interesting subject.

VII. "Some cases of sudden death in women during the puerperal state" (*Forplantningsperioden*) are next contributed by N. E. Ravn, Physician Extraordinary to the Lying-in Institution. The term puerperal state is here applied in its widest sense, comprehending the period of pregnancy, the act of parturition, and the time of subsequent confinement. The cases brought forward by Dr. Ravn are only two in number, and are placed on record as a contribution to the stock of facts on which future investigators of this important subject shall have to base their inferences. In the first case given, death is stated to have occurred suddenly from eclampsia at the time when labour was daily expected, though it had not yet commenced; nothing was found on post-mortem examination to account for the fatal result, except a highly-congested state of the brain; but the situation in which the body was found, with the face buried in the bedclothes, led to the suspicion that the patient had died from suffocation during a convulsive attack, in the absence of her husband, who had gone to seek for help; and in this opinion we ourselves, from a perusal of the case, feel bound to concur. In the second, headache, followed by convulsions and subsequent coma, without stertorous breathing, supervened during labour. About four ounces of blood were found in the ventricles of the brain. The case was therefore one of apoplectic eclampsia.

The subject of sudden death in the puerperal state has been very fully treated of by Dr. M'Clintock, the present Master of the Dublin Lying-in Hospital, in the communications referred to by Dr. Brünniche, and just now quoted. Dr. M'Clintock, in recapitulation, assigns the following as the probable causes which may operate in effecting this result:

"1. Idiopathic asphyxia; 2. The shock of parturition; 3. Syncope; 4. Mental impression; 5. Air in the veins and heart (?); 6. The formation of a coagulum in the heart; 7. Clots in the pulmonary artery: 8. Phlegmasia dolens; and 9. Morbus cordis."

Death in the Puerperal State, originally appeared in the Dublin Medical Press for 1852, in the form of two communications to the Surgical Society of Ireland.

* Vol. xi. p. 382.

It has also been subsequently considered at some length by Dr. Achille Dehous, in an inaugural thesis, published at Paris in 1854; and it was, in 1855, proposed by the Académie Impériale de Médecine, as the subject for a prize essay. To the facts already brought to light by the researches of these observers, no addition is here made by Dr. Ravn. Indeed, his paper does not add anything to the knowledge we had previously possessed upon this subject. In one of his cases, death was simply the result of an apoplectic effusion, and therefore presents no feature peculiar to the state of pregnancy or child-bed. In the other instance, the apparent and most probable cause of death was suffocation.

VIII. "Two cases of typhus observed at Frederik's Hospital by H. R. Magnus, practising physician in Hobro." The author says—

"The last time the question of the identity of typhus and typhoid fever was broached among us was, so far as I know, at the meeting of naturalists held here in 1847, when the matter was brought under discussion in reference to the result arrived at by the Danish Committee. The latter, basing their decision chiefly upon observations made at the General Hospital, had advanced the opinion, 'that similarity in the origin, symptoms, course, result, and ordinary treatment, tended to prevent the cases in which patches (*plaques*) and intestinal ulcerations are found, being attributed to a process essentially different from that on which the typhus fevers, where these anatomico-pathological affections are not found, depend.'" (p. 357.)

This decision of the Danish Committee is in accordance with the views generally held by the Dublin School—who have, unfortunately, peculiar opportunities for studying the severer form, or genuine typhus—as well as with the opinion formed by Dr. Lindwurm, now of Munich, who on two occasions visited Ireland for the special purpose of studying typhus in reference to the two questions of its contagiousness or non-contagiousness, and of the identity or non-identity of its several forms; and who

"Declared unconditionally for the view, that the diseases described as distinct species of typhus are only modifications of one and the same morbid process, of the essence and nature of which we are, however, ignorant."*

A change, nevertheless, caused chiefly by the observations of Dr. Jenner, appears to have come over the Danish mind in respect to this point, for Hr. Magnus continues—

"This opinion was, indeed, opposed; yet, so far as appears from the printed transactions, without the opposite view having been adopted. Since that time, however, our views have undoubtedly become further and further removed from those of the Committee, so that now certainly many, perhaps the majority, are no longer inclined to look upon typhus and typhoid fever as one form of disease. This revolution in opinion is indeed due, scarcely so much to our own observations as to descriptions received from abroad, for the genuine typhus occurs only exceptionally here; thus, so far as I have been able to learn, but five cases of this disease have been met with in Frederik's Hospital since 1846; and of these, three occurred in that year and the remaining two not until the present, consequently after an interval of seven years; and even these two did not originate in this country, but the first was imported by a Finnish sailor, who arrived in Copenhagen, sick, the day before his admission, and from him one of the hospital nurses took the fever." (p. 358.)

The cases were examples of the ordinary exanthematous typhus, but they do not throw much light on the point in question, as the sailor

* Der Typhus in Irland, &c.; and Dublin Quarterly Journal, vol. xvi. p. 129.

recovered, and the less fortunate nurse, on the ninth day, got an attack of Asiatic cholera, of which she died three days later, and there was no post-mortem examination. The author concludes his paper with some remarks on the fact of this disease having been communicated, while in an extensive epidemic of typhoid fever, during which, in the course of a couple of months, 160 typhoid patients were treated in the hospital: to use the words of Professor Trier, "Not an attendant of the sick, whether male or female, nor any one of the physicians, either resident in or visiting the hospital, was attacked by the fever." The extracts we have made from Dr. Magnus's paper appear to us to be interesting, as indicating the feeling of the profession in Denmark, upon the debated point referred to, and also as affording information as to the rarity of the occurrence of exanthematous typhus in Copenhagen:

"How far typhus may perhaps occur more frequently in the provinces, it is," says the author, "impossible to decide; for in the medical reports to the College of Health, the denominations 'typhus' and 'typhoid fever' appear to be used indiscriminately." (p. 359.)

IX. In a paper, entitled Progressive Muscular Atrophy with Fatty Degeneration, Dr. Brünniche, having reviewed the several opinions most recently advanced as to the pathology of this singular lesion, details the case of a man, aged twenty-eight, who was under his observation in hospital for about three months. The affection commenced, upwards of two years before his admission, with a feeling of weakness in the right shoulder-joint, with simultaneously-observed diminution of the bulk of the arm; the wasting and loss of power gradually extended to the fore-arm of the same side, and had, during the last six months, also invaded the left upper extremity. The author was of opinion that benefit was derived from the use of electricity; but the patient, weary of his stay in the hospital, and despairing of recovery, claimed his dismissal in order to return to the country. Dr. Brünniche points out that the loss of power evidently followed the atrophy, and not *vice versâ*, as would have occurred in a case of ordinary paralysis. There was no indication of any disease of the nervous centres, the patient's intelligence and senses were unimpaired, and electricity excited the affected muscles as long as they retained a trace of muscular fibre:

"The following circumstances," continues the author, "are also opposed to the view that the seat of this disease is in the nerves: first, that the muscles are attacked in portions at a time; secondly, the capricious situation of the affected muscles, which neither in my case nor in other similar cases, corresponded to particular distributions of the nerves; and lastly, that the paralysis is subsequent to the atrophy." (p. 384.)

Dr. Brünniche is hence induced to designate the disease simply as a lesion of nutrition of the muscular system.

Our experience does not accord with that of the author so far as relates to the second point referred to in the observations just specially quoted. In a case which came under the notice of the writer, and of which some account is given in the 'Dublin Quarterly Journal of Medical Science,'* the atrophy appeared to follow so accurately the distribution of the portio dura nerve of the left side, that Dr. Charles Johnson, under whose care the patient was,

* Vol. xiv. p. 246.

"Remarked that it would appear to verify the observation of Dr. Darwall, that the nerves of the human body had probably a third function in addition to the production of sensation and motion—viz., that of determining the nutrition of the parts they supplied; and that we might infer that in this case the portio dura had become paralysed as to its office of exciting nutrition, while its motor power continued unaltered."*

In this instance, too, the application of electricity appears to have been beneficial.

X. A case is next related by C. Müllertz of an enormous "hernia prorumpens inguinalis," so called "because it was a hernia in the inguinal canal, which had not extruded through its superior opening, but through an unnatural opening in its inner wall." The patient was twenty years of age; the peculiar course of the hernia was attributed to the presence and development of the undescended testicle in the inguinal canal; this organ was attached by the cord to the internal ring, so that even after death it could not be drawn further down; and although perfectly developed, it was incapable, under these circumstances, of filling the entire canal. A space of an inch in length was thus left between the inferior extremity of the testicle and the external ring, which was occupied merely by a very loose areolar tissue; moreover, it is to be supposed that the abdominal wall of the canal may have been attenuated by the pressure of the gland. The rupture, therefore, took place through the inner wall of the canal, below the testicle, but above the external ring, partly pressing the testicle upwards, and partly descending through the external ring into the scrotum. All efforts at reduction having failed, the operation for strangulated hernia was performed on the 2nd of August, at six P.M. Everything appeared to go on well until the 4th, when erysipelas set in, peritonitis supervened, and death ensued on the 11th at four in the morning.

XI. We have next an elaborate paper by Dr. Fenger, extending to a hundred and twenty-seven pages, upon 'The Masked Forms of Bright's Disease;' or those, according to the author's definition, which are not attended with dropsy. The proper characteristic mark of the affection he considers to be the existence in the urinary deposit, as proved by microscopic examination, of the so-called fibrinous cylinders. We can, of course, in here noticing so lengthy a communication, do little more than briefly point out the line pursued by the author in dealing with his subject. Dr. Fenger first describes the relation existing between uræmia and typhoid fever, a relation which he states to be of a twofold nature; thus we have cases where the symptoms closely resemble those of typhoid fever, while no such affection, but Bright's disease, is present. Again, we meet with instances where both diseases occur simultaneously, and run more or less evidently into one another. The author next passes to those cases which simulate organic disease of the brain, and are attended with a sub-apoplectic condition, epileptic convulsions, delirium, or coma. Dropsy occurring with characteristic urine towards the close of pregnancy he classes among the masked forms of the disease, because dropsy in pregnancy so often depends on other causes that its true nature may easily be overlooked.

* Op. cit., p. 246.

"Among the cerebral symptoms which may occur in the masked forms of Bright's disease, and become of importance for its diagnosis, must still be reckoned several more local nervous affections, of which the most important undoubtedly is a by no means uncommon *amblyopia* or *amaurosis.*" (p. 455.)

The cases of Bright's disease simulating affections of the thoracic organs are next considered. Of some of these the symptoms closely resemble those of œdema glottidis. Such cases, like the disease they imitate, end in suffocation. Œdema of the lung, pneumonia, pleuritis, and bronchitis may also occur as the predominant feature in the latent forms of Bright's disease; pulmonary tubercles are also often present simultaneously with the same, as are diseases of the heart, especially hypertrophy with or without valvular lesion, hæmoptysis, and dyspnœa. Among the abdominal symptoms which may attend these forms of disease may be enumerated vomiting and diarrhœa, cardialgia, and those attending on affections of the liver and spleen.

The author concludes his paper with an examination of the opinions of Dr. George Johnson in reference to chronic Bright's disease, published in former numbers of this Review*—namely, that it presents two principal varieties, which he denominates respectively the desquamative and the non-desquamative form, the first accompanied with urine much less albuminous, and of much lower specific gravity than that secreted in the second variety, while the quantity of the excretion is not so much diminished as is most frequently the case in the latter, and is usually even increased. The kidneys on post-mortem examination are found to be of the normal size, or even smaller; they are moreover very firm, and sometimes even hard in their substance, of a more or less red, though most frequently very pale red or pearl grey colour, and are ordinarily granular on the surface. This form is seldom accompanied with dropsy, and the prognosis is much more favourable than in the non-desquamative variety, in which the kidneys are perceptibly enlarged, softer, and of a dull white colour studded with numerous yellow specks. In the non-desquamative variety, moreover, the quantity of urine is diminished, its specific gravity is often considerable, the albumen is abundant, and the deposit contains fewer epithelial cells and cylindric bodies, while the latter have undergone more or less of fatty change. In this form dropsy is always present, and usually exists to a great degree.

It will be seen that the state of kidney described by Dr. Johnson as existing in his non-desquamative variety is that formerly looked upon as characteristic of the so-called second stage of Bright's disease; while the condition in which it is found to be diminished in size, hard and granular, is that hitherto attributed to the so-called third stage. The author examined the kidneys in seven cases in which the disease proved fatal, with special reference to Dr. Johnson's statement, and from the results of his own investigations, as well as from a review of the observations of others, he concludes in favour of Dr. Johnson's opinion, that when an individual dies of Bright's disease without dropsy having been manifested—that is, in the latent forms—the kidneys will be found to be hard and contracted.

XII. We have next a contribution by Dr. Brünniche, 'To the Elucidation of some Disputed Points in the Theory of Pneumothorax.' The author

* Vol. xi. p. 56; and vol. xv. p. 122.

remarks that emphysema, though of frequent occurrence, seldom produces pneumothorax, notwithstanding that under its influence the walls of the air vesicles are very remarkably distended and attenuated; and he hence infers that when pneumothorax takes place, as a result of emphysema, a further cause of its existence must be sought. This he believes may be found in an effort, by a portion of the lung, to produce an equilibrium, and to compensate for the diminution of volume caused by a partial atrophy of the organ; the theory, in fact, by which Dr. Gairdner accounts in general for the production of dilatation of the heart and emphysema. In support of this view, he details at considerable length a case of "Tuberculosis of the bowels and lungs, progressive in the left lung, retrogressive in the right, with development of vesicular emphysema in the antero-superior portion of the latter, followed by the rupture of an emphysematous vesicle, pneumothorax, and death." The author observes that—

"The presence of phthisis was established, and the physical signs seemed to indicate that it had proceeded farthest in the left side, while the phenomena in the right lung might be taken as indicating an earlier stage of the same disease. Dissection, however, showed that the tuberculosis in the right lung was probably of longer standing; various forms of isolation of the deposited masses were present, and the pulmonary tissue around them was puckered and atrophied. As a consequence probably emphysema arosé, to counterbalance the diminished bulk of the surrounding lung, and as this diminution was progressive, the emphysema continued to increase; some vesicles became over-distended, and burst. This case also affords an example of a peculiar mediate connexion between tuberculosis and pneumothorax, with emphysema as the connecting link, and is rather to be referred to the cases of pneumothorax which owe their origin to emphysema." (p. 542.)

The author shows that in 147 cases of pneumothorax enumerated in a table by Saussier, quoted in Monneret and Fleury's 'Compendium de Médecine,' fluid was absent in about sixteen; consequently, it existed in 89·11 per cent. of the cases, constituting the most usual complications, hydro- and pyo-pneumothorax. Of fourteen cases observed in the medical section of Frederik's Hospital, only two were free from fluid; hence the complication was present in 85·71 per cent., a result closely agreeing, it will be observed, with that given by Saussier.

"With reference to the termination of the disease, the co-existence of fluid does not appear to be very decisive. In the above-mentioned statistical synopsis, only sixteen of the 147 cases were cured, so that the disease may, on the whole, be regarded as very fatal. Of these sixteen, the majority were cases of pyopneumothorax, twelve having had their origin in pleuritic effusions, one from a wound in the thorax, one from rupture of the lung, while two were of doubtful character. The prognostic value of the collection of air is certainly very decided, while the recoveries referred to show that pneumothorax is at least not invariably fatal, as seems almost to be Valleix's opinion in his 'Guide du Médecin praticien.' Probably we shall be nearest the truth by saying that the diseases in the course of which an accumulation of air in the pleura takes place, thereby acquire, in most instances, a fatal complication, that the degree of danger depends scarcely so much on the co-existence of an accumulation of fluid, as on the magnitude and diffusion of the collection of air, and above all, on the nature of the disease whence it has been developed.

"Thus authors are agreed that pneumothorax developed from tuberculosis of the lung is never cured; on the other hand, it would appear, from what has been

mentioned, that that occurring from without, by the rupture of an empyema or from a wound of the thorax, admits of a more favourable prognosis; circumstances which are certainly due to the nature of the disease producing the pneumothorax, its more or less destructive character, the state of exhaustion in which the extravasation of air generally finds the patient, &c. We should consequently be inclined to consider the prognosis to be most favourable in cases in which the affection depends upon a rupture of the lung, without previous disease of the latter. Such examples are, however, so rare, that they cannot be statistically entertained." (p. 545.)

In illustration of these remarks, the author records a case of the sudden occurrence of pneumothorax in the right side in an apparently healthy man, with circumscribed exudative pleuritis, terminating in recovery, and he concludes his paper with lengthened observations upon it, into the consideration of which our space does not permit us to enter.

XIII. The last paper in the volume, On the Abortive Treatment of Zona, by Dr. E. Fenger, is rather suggestive of the possible efficacy of the application of collodion in arresting the development of herpes Zoster, than a report of the author's experience on the subject, this having, at the time he wrote, been confined to a very few cases; and he therefore invites the co-operation of the profession in establishing a series of trials in reference to his proposal.

We have endeavoured, in the foregoing pages, to bring before our readers such a sketch of the "Hospital Communications" of our Danish brethren, as may enable them, each for himself, to form an estimate of the value of the interesting essays contained in the volume we have been reviewing. In thus attempting a survey of the whole, we have been prevented entering as fully into some of the papers as the importance of their subjects would, properly speaking, demand, and we have consequently been unable to render to the writers the full justice we should have wished, under the circumstances, to have extended to each. We can only say, in concluding a task which has been to ourselves a source both of pleasure and of interest, that in our opinion, the first volume of the new series of the 'Hospitals Meddelelser' bears on every page the impress of the ability, sound judgment, accuracy, and truthfulness, we have long admired in our Scandinavian colleagues; and we only hope that the learned editor will not unreasonably avail himself of the latitude afforded by the regulations under which they are published, and too long deprive us of the gratification of noticing his subsequent volumes in the pages of this Review.

Review IV.

Museum Anatomicum Holmiense. Quod auspiciis Augustissimi Regis Oscaris Primi, ediderunt Professores Regiæ Scholæ Medico-Chirurgicæ Carolinensis. Sectio Pathologica. Fasiculus primus, continens casus x., cum xii. tabulis.—*Holmiæ*, 1855.

The Anatomical Museum of Stockholm. Edited under the auspices of His Majesty Oscar I., by the Professors of the Royal Medico-Chirurgical School. Pathological Section. First Part, containing ten cases, with twelve plates.

We have had numerous occasions of drawing the attention of our readers to the valuable productions of our Scandinavian brethren; we are again called upon to express to them our thanks for a work which, as far as it has yet appeared, fully justifies the reputation they already possess; while, on account of its object, and the dress in which it appears, it will prove as useful to the Englishman as the Swede; at least, we would hope that the same facility of reading Latin prevails among ourselves as in the land of the Northmen. Should it not be so, we would express a wish that, with the revision of the whole medical education of this country, some steps may be taken to insure the revival of the vernacular employment of the Latin tongue among medical men, which, as a means of intercourse with scientific men of other countries, and as a means of clinical instruction in the presence of the patient, has great claims upon our consideration.

The 'Museum Anatomicum Holmiense' is a collection of tinted lithographs, taken from important preparations in the Anatomical Museum at Stockholm. It is published at the expense of the King, at the immediate suggestion of the Bishop Genberg, and the President of the Medical College, Dr. Eckströmer. The professors of the Royal Medical School are the parties responsible for the work. Among them our readers will meet with some familiar names; they are—A. Retzius, P. F. Wahlberg, C. G. Mosander, M. Huss, F. Th. Berg, M. C. Retzius, C. Santesson, P. H. Malmsten.

The work is to appear in parts, at irregular intervals; the size of the parts, and apparently the extent of the entire work, is undefined, and will probably depend upon the specimens at hand, and the convenience of the writers. The first number contains twelve excellently executed delineations, representing the subject in natural size, accompanied by an account of the case from which it was obtained, and a minute description of the preparation. They are all valuable illustrations of disease. The most remarkable are probably a case of hypertrophy of a portion of the glandular structure of the stomach (true mammillary hypertrophy), a case of epithelioma of the stomach, and a case of intestinal calculus. A brief description of these three may not be unacceptable to our readers.

The first occurred in a married woman, aged forty-two, who, in spite of great poverty, had, with the exception of frequently recurring pyrosis, always enjoyed good health. Five years before coming under observation, the pyrosis becoming more urgent, she was frequently attacked with

vomiting, when she brought up her food at longer or shorter intervals after it was taken, but without hæmatemesis; her strength gradually failed, chronic dysentery and œdema supervened, and death ensued. The stomach was the only organ which exhibited any marked disorganization. It was of the normal size. The whole of its mucous membrane presented an ashy-green colour, with a tawny hue towards the pylorus. The pyloric portion was covered with papillæ or tuberiform projections, at some parts separated from one another, at others closely packed together; they diminished towards the middle of the stomach, and the fundus exhibited a level surface. A vertical section showed the enlargement to be almost exclusively due to the glandular structure, constituting a genuine hypertrophy of the mucous membrane. There was no trace of pseudo-plasma, induration, ulceration, or erosion. The sub-mucous tissue was normal in the affected parts, but the muscular coat a line and a half in thickness. The apices of the villi were tumefied, giving the membrane the appearance of velvet. The hypertrophied tubes were gorged with epithelium, but the epithelium presented nothing abnormal. The solitary glands of the large intestine were enlarged, and ulcers were found in the descending colon.

The epithelioma occurred in a female, aged fifty-seven, who, up to the year preceding her death, had enjoyed good health; she then was seized with intense headache, lasting day and night, with severe constipation, and loss of appetite. Œdema of the feet, the right hand and eyelid, supervened. She became very cachectic and feeble; but the tongue remained clean and soft; there was no vomiting; the abdomen soft; there was no tenderness, nor could a tumour be discovered. The symptoms pointed almost exclusively to disease of the brain and right lung.

The autopsy showed numerous bony formations in the longitudinal sinus, and the lower portion of the right lung was in a state of grey hepatization. The stomach was contracted, and contained two large pediculated tumours, arising from the posterior wall, and being directed towards the pylorus. The long diameter of the larger tumour was nearly five inches, that of the lesser about one inch. The conical peduncle of the larger tumour proceeded from the lesser curvature near the cardiac orifice, and was almost covered by the tumour itself, which also presented a conical form. The surface of the latter exhibited a cauliflower appearance, and consisted of fimbriæ, folds, and long laciniæ and villi. The folds were in many parts disposed concentrically, so as to look like roses. The petiole of the lesser tumour was also conical, and presented several folds. This tumour resembled a powder-puff, the surface being cut up into long, narrow laciniæ. Both tumours were of a greyish-red hue. A section showed an internal alveolar structure; the alveoli diminished in size in proportion to their proximity to the base. The alveoli contained a mucous fluid, with cylindrical epithelium and nuclei, some of the size of blood-corpuscles, some less. There were but few larger vessels; but a scanty network of minute vessels and capillaries was observed. The superficial portions consisted of epithelium and cell-nuclei, which were in contact with several layers of pellucid corpuscles of a circular or oval shape, four times the size of blood-corpuscles. There were also numerous small pellucid corpuscles, probably the nuclei of unformed epithelium.

The parietes of the alveoli consisted of an almost structureless membrane, around which lay nuclei, cells, and fibres. The more one approached the centre of the tumour, the more it presented a fibrous texture, consisting of flat fibres, which were in many parts manifestly made up of fusiform corpuscles, with and without nuclei. Fat molecules were extensively dispersed throughout all the corpuscles. The authors observe that the case is an illustration of epithelial cancer, or what has been perhaps more appropriately called by Hannover, Epithelioma.

The case of intestinal calculus is a rare specimen of the development of an enormous concretion, which formed in the caput cæcum and appendix vermiformis of a labouring man, aged twenty-two. It was passed per anum after intense suffering, the dislodgment having been apparently effected by the use of seal oil, which the patient prescribed for himself after having been under the hands of medical men to no purpose. The calculus weighed fourteen ounces and a quarter, was nearly seven inches long, and above two broad. Its shape was moulded to the vermiform process and the cæcum, which it had occupied. The surface was granular, exhibiting impressions of the mucous membrane. About the middle of the concretion was a minute cavity, about 0·1 inch (4 millimètres) in diameter, containing a small coagulum, round which the calculous matter was arranged concentrically as far as the surface; the further accession of calculous matter in the direction of the two ends of the concretion also exhibited a generally concentric arrangement round the coagulum, but the circles were necessarily not completed. The layers surrounding the coagulum were alternately tawny and containing a hairy substance, and yellow, more solid and earthy. This alternation was particularly regular for the first six layers, the hairy layers being less broad than the others. The hairs, on microscopic examination, were found to be the hairs that invest the caryopsis of oats.

The following is the chemical analysis of the inner layers:

Matters soluble in ether	1·58
Soapy matters soluble in pure alcohol, the bases of which were soda, lime, and magnesia	0·30
Fatty salts and acids soluble in water	5·20
Water mixed with the above salts	0·72
Sub-phosphate of lime, phosphate of magnesia, with traces of iron and manganese	77·50
Silicic acid	0·70
Hairs of caryopsis of oat	14·00
	100·

Other portions of the concretion were found to contain a small amount of carbonate of lime. No biliary matter could be found in any part.

Before concluding this article, we may also allude to a very interesting specimen of a pedunculated calculus of the bladder. It was discovered in the body of an old woman, of whose previous history nothing was known. The peduncle and the nucleus of the calculus was a fibrous polypus growing from the upper and back part of the bladder; the peduncle from which the calculus was suspended was half turned upon its axis. The base of the calculus was broken off, the fragments probably

having passed off by the urethra. What remained was nearly three inches in its longest, and two inches in its broadest, diameter, and presented an elliptical shape. It nearly half filled the bladder, which exhibited considerable thickening in its muscular and mucous coats.

It remains for us to express a hope that we may soon have occasion to announce to our readers a continuation of a work alike conspicuous on account of its artistic elegance and its scientific merits, and equally creditable to the Government under whose auspices it is commenced, as to the gentlemen more immediately concerned in its execution.

Review V.

1. *A Manual of Medical Jurisprudence for Bengal and the North-Western Provinces.* By Norman Chevers, M.D., Secretary to the Medical Board, Fort William.—*Calcutta,* 1856.
2. *A Treatise on Removable and Mitigable Causes of Death, their Modes of Origin and Means of Prevention; including a Sketch of Vital Statistics and the leading Principles of Public Hygiène in Europe and India.* By Norman Chevers, M.D., Bengal Medical Service. Vol. I.—*Calcutta,* 1852.

Happy is the country the Government of which respects and encourages science, and in which men of science are to be found in the public service respected and rewarded. We make this remark after the perusal of the works which we are about to notice, the titles of which are given above; both by the same individual, both written in Bengal, and one, the former, published by order of the authorities in power.

Nowhere is there ampler scope, a larger field for the beneficial exercise of science, than in our Indian empire, whether we consider the vast tracts of country it comprises, their varied climates, their varied productions, and, what is more important, their various races and phases of society and of civilization. The extension of this empire from its insignificant small beginning to its present magnificent amplitude, with its gradual organization from a trading company of merchants into an imperial government such as it now is, is surely one of the greatest marvels of history, and one of the most memorable triumphs—may we not say of intellect over brute force, of honesty and honour over their contraries; in brief, of a higher principled and more advanced race over an inferior, especially morally and religiously viewed.

Amongst those who have been mainly instrumental in the great cause of improvements in India, no class of the Company's servants has, we believe, deserved better than its medical officers. Most of our knowledge of the country, as regards its natural history, we owe to them. If we have at all gained the affections of the people, it has been chiefly through them. And one of the most promising indications of the spread of sound knowledge, and the substitution of science for ignorance and a degrading and brutalizing superstition, is opening out in the establishment of medical schools and colleges for the education of native youths in the medical profession, conjoined with the privilege granted them of admission, when qualified, into the public service.

The cognate works before us on those great subjects, medical jurisprudence and public health, are good examples of the beneficial exertions we have been alluding to, and, we have pleasure in adding, of the zeal likewise, and industry and ability of their author. He informs us, in the preface to the latter, that the larger portion of the materials of the treatise was collected at Chittagong, "during the unfrequent intervals of leisure allowed by extensive medical duties." Greatly is this to his credit; honour is due to him, and we rejoice to see that his labours have been approved, and that he now has the appointment of Secretary to the Medical Board, an office, for the credit attached to it, hardly second to any in the department to which he belongs; and perhaps superior to any as regards the importance of the duties connected with it, and for which we are sure he has been selected not on account of seniority of standing, but on account of merit and fitness.

Of the author's works which we have undertaken to review, our notice, from their very nature and the limited space only that can be allowed them, must be briefer than we could wish. We shall commence with his 'Manual of Medical Jurisprudence,' which, we need hardly observe, owes its interest and importance to its being written expressly for India, for Bengal, and the North-Western Provinces. In the preface, Dr. Chevers remarks, speaking of his performance, which he modestly calls a sketch, that it is "avowedly a very slight and imperfect one; still, it is believed that it is the first that has been attempted, and it is trusted that it will at least serve to demonstrate the importance which would attach to a thoroughly complete and elaborated history of crime in India." This certainly it does, and till we have such a history, it may well supply its place. It is rich in facts and original observations, the opposite of "a barren epitome," and only requires to be extended to be all, or nearly all, that its author, in his highest aspirations, could wish for; and let us indulge the hope that he may live to complete it to his heart's desire.

In an historical point of view, as throwing light where there is most darkness—the privacies and mysteries of Indian society—this work is specially interesting: interesting and distressing, from displaying so much vice, so much corruption, brutality, and crime, filling the mind with horror, and making one ashamed of our common human nature. Here is the character of the people, given by two distinguished historians, who had lived amongst them, and from their situations and opportunities were very competent to form a correct judgment, nor likely to set down aught in malice:—

"The *Rajpoots* are the representatives of Hinduism. In them are seen all the qualities of the Hindu race unmitigated by foreign mixture, exerted with their original energy, and displayed in the strongest light. They exhibit the genuine form of a Hindu community, formed of the most discordant materials, and combining the most extraordinary contrasts of moral nature: unconquerable adherence to native opinions and

"The physical organization of the *Bengalee* is feeble, even to effeminacy. He lives in a constant vapour-bath. His pursuits are sedentary, his limbs delicate, his movements languid. During many ages he has been trampled upon by men of bolder and more hardy breeds. Courage, independence, veracity, are qualities to which his constitution and his situation are equally unfavourable. His mind bears a singular analogy to

usages, with servile submission to any foreign yoke; an unbelieving priesthood, ready to suffer martyrdom for the most petty observance of their professed faith—a superstition which inspires the resolution to inflict or to suffer the most atrocious barbarities, without cultivating any natural sentiment or infringing any social duty; all the stages in the progress of society brought together in one nation, from some abject castes more brutal than the savages of New Zealand, to the polish of manners and refinement of character conspicuous in the upper ranks; attachments to kindred and to home, with no friendship and no love of country; good temper and gentle disposition; little active cruelty, except when stimulated by superstition; but little sensibility, little compassion, scarcely any disposition to relieve suffering or relieve wrong done to themselves or others; timidity, with its natural attendants, falsehood and meanness, in the ordinary relations of life, joined with a capability of becoming incited to courage in the field, to military enthusiasm, to heroic self-devotion. Abstemiousness, in some respects more rigorous than that of a western hermit, in a life of intoxication; austerities and self-tortures almost incredible, practised by those who, otherwise, wallow in gross sensuality; childish levity, barefaced falsehood, no faith, no constancy, no shame, no belief in the existence of justice." (Mackintosh.)

his body. It is weak, even to helplessness, for purposes of manly resistance, but its suppleness and tact move the children of sterner climates to admiration, not unmingled with contempt. All those arts which are the natural defence of the weak are more familiar to this subtle race than to the Ionian of the time of Juvenal, or to the Jew of the dark ages. What the horns are to the buffalo, what the paw is to the tiger, what beauty, according to the old Greek song, is to woman, deceit is to the Bengalee. Large promises, smooth excuses, elaborate tissues of circumstantial falsehood, chicanery, perjury, forgery, are the weapons offensive and defensive of the people of the Lower Ganges. All those millions do not furnish one Sepoy to the armies of the Company. But as usurers, as money-changers, as sharp legal practitioners, no class of human beings can bear a comparison with them. With all his softness, the Bengalee is by no means placable in his enmities, or prone to pity. The pertinacity with which he adheres to his purposes yields only to the immediate pressure of fear. Nor does he lack a certain kind of courage, which is often wanting in his masters. To inevitable evils he is sometimes found to oppose a passive fortitude, such as the stories attributed to their ideal sage. An European warrior, who rushes on a battery of cannon with a loud hurrah, will sometimes shriek under the surgeon's knife, and fall into an agony of despair at the sentence of death. But the Bengalee, who would see his country overrun, his house laid in ashes, his children murdered or dishonoured, without having the spirit to strike one blow, has yet been known to endure torture with the firmness of Mucius, and to mount the scaffold with the steady step and even pulse of Algernon Sydney." (Macaulay.)

In these descriptions of the character of the people, the antithesis of style and fine writing might make one doubt their correctness, were it not confirmed by the prevailing vices and by the crimes committed, as evidenced in every page of the criminal reports.

"Theft, perjury, personation, torture, child-stealing, the murder of women and aged men, assassination, arson, the butchery of children for the sake of their ornaments, drugging and poisoning, adultery, rape, unnatural crime, the procuration of

abortion, are among the leading villanies of these ingenious, calm-tempered, indolently-pertinacious sensualists."

These are the words of the author; and he adds, in corroboration, that—

"It is only by thoroughly knowing the people, and by fixing the mind sedulously upon the records of their crimes, that an European can learn how strange a combination of sensuality, jealousy, wild and ineradicable superstition, absolute untruthfulness, and ruthless disregard of the value of human life, lie below the placid, civil, timid, forbearing exterior of the native of India."

He further states "that the women are even more ignorant and brutalized than the men; that the belief in woman's virtue or man's honesty does not exist amongst them;" and that between the Hindus and the Mussulmans there is little difference in the characters of the offences recorded against them. The darkest period of the night, and its most chilling, is that nearest the dawn. Amidst this intensity, as it seems, of national depravity, there is one peculiarity, which we would fain hope may be viewed as a qualifying and extenuating, if not a redeeming circumstance. The Committee on Prison Discipline remark in their Report (that of 1838):

"An Indian criminal is probably a better man than any other criminal of the same sort. His general character certainly differs less from that of the mass of his countrymen than would be the case in more civilized and moral countries."

Adding:

"A large proportion of the crimes in this country are committed by persons whose tribe have done the same time out of mind; and they are almost as naturally the result of birth as another man's honest trade. Many more are committed, as it were, professionally, by members of immense confederations, who are not much worse than other people in matters unconnected with their profession, owing to feelings which we can never [?] comprehend. There is little or no consciousness of moral guilt amongst these classes, on account of the exercise of what they regard as their proper business."

If this statement be correct—tending to show how weak is conscience opposed to habit; and it is in accordance with all our experience of mankind, whether we direct our attention to the usages of the early Britons and their human sacrifices, or to those of the New Zealanders in our own times, so recently cannibals—we ought not to despair of the people of India, nor consider them incapable of acquiring a better mental condition under an efficient moral and religious training and education, such as have been so successful amongst the savages just mentioned, and in our ragged schools and our reformatory institutions. Without this hope, *ad meliora*, ought we not to consider the possession of the country an evil, and the ruling over it a temptation and a curse, pregnant with evil to ourselves? But far be from us this despair! Let us keep in mind that man everywhere is very much the creature of circumstance; under good influences having the virtues of humanity elaborated, and *vice versâ*. Of these influences, none are more powerful than just laws, having for their end the suppression of crime; and to such laws no help is greater than the science comprised in medical jurisprudence.

Written as Dr. Chevers' work professedly is, for India, its arrangement, the subjects treated of, accord with the prevailing vices and crimes of the

people. Referring to the table of contents, the topics which meet the eye as most peculiar are—*poisoning*, in its multifarious ways, and for diverse purposes (no less than forty-three different poisons are named which are procurable in the bazaars of the country); wounds, including *torture*, variously administered; *human sacrifice;* the bites of venomous serpents; under the head of asphyxia, *burying alive;* under insanity, *running amok*, *fanaticism*, and *religious monomania*. These, and the ordinary matters belonging to medical jurisprudence, are preceded by some judicious remarks on the character of the people—to which we have adverted; on the transmission and examination of wounded persons and of dead bodies; on medical evidence, and on the uncertainty of general evidence in India; on the search for the bodies of missing persons; and on the declaration of the dying. The rules accompanying, and the remarks themselves, cannot fail of being useful to those for whom they are chiefly intended—the officer of police and the medical officer, whose assistance may be required in the cause of justice; and what is very valuable, they are in most instances illustrated by cases, some of them of a very curious and extraordinary kind. We shall give a specimen: it is under the head of Identity of Bodies, and is very Indian and characteristic.

"A very prevalent crime amongst the natives of Bengal, is that of causing a person to disappear, and of charging some obnoxious individual with his murder; a putrid corpse, readily procurable from the river, and disfigured with wounds, being perhaps brought forward as that of the lost individual. Upon the examination of this body the medical officer can probably do little more than report that the remains are those of a male or female, young or old, upon which he observes certain wounds, regarding which he cannot venture any opinion as to whether they were inflicted before or after death. Here the safety of the accused is, of course, imperilled; unless, as has not unfrequently happened, his alleged victim be speedily produced in full life and vigour. The most recent case of the kind is, I believe, that recorded in the 'Nazamut Adamlut Reports,' Part I. of 1853, p. 259, in which 'certain of the prisoners were convicted, on their own confessions, of perjury, in having falsely deposed to the fact of a murder, and burial of the corpse, in a trial, at the conclusion of which the alleged deceased made his appearance in court.' 'It would be impossible,' writes the sessions judge in his report of this trial, 'to imagine a case more completely satisfactory, as regards, at least, the guilt of Abdost Kurrem [the unfortunate accused], than this became when the daragah's report was completed, and as, in fact, it remained, until the appearance of Pertaubnarain [the murdered man] brought to light its real character. The prosecutrix was the mother of the missing man; the principal witness was his wife, Shearasattee, and his cousin, Kanaram; while the prisoner's own servants detailed at length the circumstances attending the burial of the body. There were no inconsistencies and no contradictions in the evidence, which from first to last gave the hearers the impression, that a heinous crime had indeed at last been brought to light, in spite of a powerful combination to conceal it.'"

Under the head of Search for the Bodies of Missing Persons, are many striking instances given in illustration. The following remarks, though specially applicable in a hot climate, are in their bearing worthy of attention in every climate; and we quote them on that account, and as affording a good example of the author's style and acumen. As in most other instances, they are supported by cases, many of them of a remarkable kind, confirmatory of the belief that

"Murder hath speech, and will declare itself
With most miraculous organ."

"'Wheresoever the body is, there will the eagles be gathered together,' is a fact which has daily illustration in India, and which has often been turned to good account in searches for the remains of missing individuals. . . . . I have long thought," he continues, "that the common pariah dog of the country, and very possibly the vulture also, could be trained for the discovery of missing bodies. The dog would probably be found tolerably manageable, but could rarely be useful in cases where the corpse lay at a considerable distance. The keener sense and the wider visual range of the vulture would tell far more advantageously; and although this bird appears to be absolutely untameable, small supplies of food will generally induce it to resort to a particular locality, where its movements can nearly always be under observation. Indeed, a tree resorted to by vultures, will be found in the neighbourhood of every station; and a look-out for the direction in which the birds left or returned thither, might often assist a search."

In the chapter On Poisoning—a crime, it would appear, extremely rife in India, from the earliest recorded period to the present time—much valuable information is given. According to Strabo, the author remarks, the burning of Indian widows was enforced to check the women's practice of poisoning their husbands; and Captain Hamilton, who traded in India between 1688 and 1723, notices a legend in which the custom is similarly accounted for; how, before the law was enacted, poisoning was so well known and practised, that the least quarrel that happened between a married couple cost the husband his life.

"Thuggee of travellers by poison" is one of the peculiar crimes of the country, and practised, we are assured by the author, throughout the three Presidencies, in which it is pursued systematically as a trade, and is not merely the device of a stray criminal—especially since the check given to Thuggee,—Burking (to use our synonyme) by strangulation. Amongst the evidences the author brings forward in proof of the frequency of the crime, he quotes Colonel Sleeman, who, writing in 1844, "expressed his belief that no road was free from poisoners; and that throughout India there must be many hundreds who gained their subsistence by that trade alone."

Poisonous snakes, formidable enough in themselves, but how much less so than those inhuman poisoners just referred to, have very properly had the attention of the author as a medical jurist. He makes mention of twelve different species known in Bengal and the Bay of Bengal—some terrestrial, some pelagic—the latter, there is reason to infer, more ferocious, that is, more disposed to attack man, than the former, which seldom or never, we believe, strike, except on the defensive. It is conjectured that a vast amount of undiscovered crime is concealed in India under the always plausible and not generally controvertible report of "Died by snake-bite"—a belief confirmed by the fact of the disparity in the number of reported cases of the kind, and of well-authenticated cases. Thus, whilst in one district of Bengal alone, the number of deaths attributed to this cause by the magistrate amounted to 402 in twenty-one months, not a single case of snake-bite had been admitted into the General Civil Hospital in Calcutta during the preceding thirty years:—a contrast the more remarkable, since poisonous snakes are nowise unfrequent in that town and its vicinity.

In treating of the effects of this animal poison, we perceive that Dr. Chevers has adopted the common opinion, that they are much the same in kind, irrespective of the species of snake inflicting the poisoned wound.

This we are satisfied is not the case, and we found our conviction of the contrary on our own experience, derived from trials made with the three poisonous species which occur in Ceylon;—trials which we instituted whilst stationed in that island many years ago, and which we published at the time.* The results decidedly proved that the poison of each kind acted differently on the animal experimented on. Should the author's work come to a second edition, as we have no doubt it will, we venture to express the hope that he will reconsider the subject, as an error of this kind must vitiate and render useless, and worse than useless, all inquiry in doubtful cases of "snake-bite."

Torture in India has but lately, indeed only within the last twelve months, excited a home interest, and has been in a manner ignored, yet, strange to say, the practice of it has never been discontinued.

"We have abundant evidence," says the author, "that this atrocity has now become intimately blended with the customs of all sects and classes of natives throughout India. The poor practise torture on each other; robbers on their victims, and *vice versâ*; masters upon their servants; zumindars upon their ryots; schoolmasters upon their pupils; husbands upon their wives; and even parents upon their children."

A like remark applies to "human sacrifice," which, we are assured by Dr. Chevers, is still perpetrated, notwithstanding all the exertions of Government. "This crime," he says, "doubtlessly is less prevalent than formerly; but there are strong reasons for believing that there is scarcely a district in India in which human sacrifice is not still practised as a superstitious rite," and this "altogether apart from those of suttee and female infanticide." He adduces instances in proof of a terribly revolting kind.

On the complicated subject of wounds and injuries—whether inflicted before or after death, whether or not self-inflicted, whether by wild animals, whether by weapons in common use; further, as to the manner in which inflicted—as, by hacking the neck, decapitation, cutting the throat, fractures and dislocations, gun-shot wounds, mutilation—deaths from beating, &c.—much varied and curious information is given, alike illustrative of the criminal propensities of the natives and their debased moral nature—information which every medical officer serving in India should be familiar with, and portions of which may be advantageously known to the medical practitioner at home, to secure him, if called on to give evidence in court, from serious mistakes; such, for instance, as the remarks on the colour of cicatrices in the persons of negroes, and those on the causes of death in cases of injuries not in themselves of an essentially fatal character, followed by death. Regarding the first, it has been asserted recently, and even by medical jurists of deservedly high reputation, that in the healing process of wounds, in the instance of the dark-coloured races, the *rete mucosum* is not restored, and consequently that the cicatrix remains white; whilst in fact, as Dr. Chevers has satisfied himself, confirming the observations of Mr. Lawrence made long ago, that in process of time it (the cicatrix) becomes darker even than the adjoining skin. Regarding the second, he states,

* See Researches, Physiological and Anatomical, vol. i., by John Davy, M.D.

"About ten years since I went carefully over the whole of the records at Guy's Hospital of cases in which injuries and surgical operations, not in themselves of an essentially fatal character, had been followed by death at periods of a few hours or days, during a period of fifteen years. The results were, out of 153 cases which had occurred in the hospital, death had resulted from inflammation of internal organs or secreting surfaces (including the liver and spleen) in 143 instances. In the remaining 19 the patients died from other causes, such as tetanus, sloughing, hæmorrhage, suppuration, gangrene, erysipelas, diarrhœa, and total deficiency of reparative action in the wound. In one only the precise cause of death was not ascertained. Out of these 153 cases, there was marked disease of the spleen, liver, and kidneys in 93 cases. In the 143 cases of death from internal inflammations, there was also superadded marked disease of the spleen, liver, and kidneys, or of all these organs at once, in 90 cases."

Had such a return as this been called for in the House of Commons when, some years ago, a death after flogging in the instance of a soldier at Hounslow was discussed, the decision arrived at might have been more just; but then, as too often is the case, public clamour stood in the way of sober inquiry. And here we would remark, that were an analysis made, after the manner followed by Dr. Chevers, as to the cause of death in cases proving fatal in hospital—comprising a large number admitted with slight ailments—the results could hardly fail proving instructive and highly useful, were it merely in their tendency to check rash and hasty conclusions.

The chapter On Infanticide, the last we must notice, is peculiarly interesting; and in the facts adduced, connected with sex, specially characteristic. The author remarks,

"The murder of female children, whether by the direct employment of homicidal means, or by the more inhuman and not less certain measures of exposure, privation, and neglect, has for ages been the chief and most characteristic crime of six-sevenths of the inhabitants of British India."

What a contrast is presented in the following passage, and in the statistical statements in the subsequent one! And how grateful we should be that, as a people, we are not exposed to the same terrible influences and temptations! More than that, how earnest should be the efforts of all who are officially employed in India, to oppose and abolish a system fatal to all natural affections, and in its reflex action no doubt contaminating society, and degrading it to the very depths of brutality.

"By the Hindu the advent of a female child is superstitiously regarded as a curse, and is practically viewed as a tax and a misfortune. The daughter, so welcome in the English peasant's homestead, so fondly greeted as the crowning honour and presiding grace of every European family of gentle blood, is viewed alike by Hindustani, Rayot, and the Rajput thakoor, as a certain presage either of poverty or of shame hereafter. The daughter of the Hindu must always be dependent upon others for her support; she must be suitably married, and a crime will be involved in the postponement of her nuptials beyond the age of childhood. At her husband's death she must trust solely to the support of others; and her conduct must be watched with unceasing vigilance, lest shame, with all its direct accompaniments—feud, revenge, and murder—should be entailed on her house. . .

"The results of this condition of things," the author continues, "may be set forth in a single paragraph. It is clearly established that in every country in Europe there is an excess of females. The census of 1851 showed that throughout Great Britain and Ireland the number of males then amounted to only 48·2 of

the inhabitants. The recent census of the North-West Provinces of India gave 53·4 as the per-centage of males in a population of 30,271,885; while the official census of Mysore, for 1852, showed that in a population of 3,410,382, the number of adult males exceeded that of females by nearly ten per cent., while the excess of male infants was sixteen per cent!"

We have marked other passages for quotation, but restricted as we are for space, we must pass them over; and this we do the more readily, feeling confident that such of our readers who are specially interested in medical jurisprudence will consult the work itself, which, we have no hesitation in saying, is not only valuable for use in our Indian possessions, but—to bestow on it higher praise—is valuable also in contributing, by the original matter it contains, to the advancement of science.

We have spoken of medical jurisprudence and public hygiène as cognate sciences: so, truly, they are. The limits between them are hardly sensible, their differences consisting mainly in the circumstance that, in the instances, the cases coming under the one, there is a presumed criminal intent, whilst in those of the other, that intent is absent; and further, accordingly, that whilst the one is concerned chiefly in endeavouring to prevent and detect offences committed by individuals on individuals, the other is occupied in discovering and removing noxious causes endangering the health and lives of the many. Both, in relation to the interests of society, are of the highest importance—are, indeed, vitally important; a consciousness of which, we are happy to think, is daily becoming more and more acknowledged; allowing us to indulge in the hope, that the time is not far distant when this feeling of their importance will be so strongly impressed on the public mind, that each will be held to be a fit topic for instruction, and enter into the curriculum, not only of our colleges but also of our public schools—at least, the elementary principles of hygiène. Both of them, as sciences, are of modern origin. Half a century ago, even medical jurisprudence was not taught in any of our medical schools. It is little more than forty years ago that we attended the first course of lectures ever given on this subject in a British university; and at present we are not aware that, in any one of our universities, there is a chair solely devoted to hygiène. The connexion between the two is well shown in the works of Dr. Chevers, now before us. In the perusal of the latter, we have had much the same satisfaction as we experienced from the former. The subject-matter being more popular, affecting the interests of all, the author, in his treatment of it, has addressed himself, and very properly, as much to the public as to the members of his own profession, and in a more discursive manner than he has observed in the preceding, and more supported by statistics (for most part given in foot-notes); using a style always vigorous and animated, and often eloquent. The first of the two promised volumes only has yet reached us – that relating to Public Health in Europe, mainly, we may say, at home. This home view, he informs us, he has thought it necessary to enter upon, as preliminary and in preparation for his special subject, the matter of the second volume—viz., the Hygiène of India. He says, and says justly, in explanation, after passing in rapid review the progress of sanitary reform in the West:

"It will not, we trust, be considered that the above literally 'musty' records of

Old-World barbarism have been called up without a motive. The principal object of these pages is to suggest the necessity for an active sanitary reform throughout the whole of our possessions in the East; and, at present, it is only by knowing what has yet been achieved at home, that we can adapt our measures to the mighty task before us. India is, at this moment, decidedly behind England of the fifteenth century, in respect to the sanitary condition of its towns and villages; and is, of course, in natural advantages of climate, many degrees less favourably situated as regards the probable success of hygiènic laws. Still, it has become the principle of this age to seek out difficulties for the glory of overcoming them; and the field which lies open to English scientific enterprise in the sanitary improvements of this noble country, may well engage the energies of the best intellects during the remaining half century. It may never come within the power of man to remove those infinite sources of pestilence which lie in the vast jungles and marshy plains of Bengal; still, it may be some encouragement to modern enterprise to remember that, in our own country, two hundred years ago, intermittent fevers and their allied disorders were as prevalent as they now are in Lower India, and scarcely less destructive in their ravages. In 1652, the casements of Windsor and Whitehall admitted, from the reeking flats of Eton and Lambeth, marsh vapours as poisonous as those which now arise on every side around the Anglo-Asiatic palaces of Garden Reach. If the narrow and neglected streets of old London could bring the seeds of the Great Plague into full development at a season when the fresh autumnal breezes were rushing down upon the city, laden with the scents of gardens, and harvest-fields, and pleasant waters, much more will cholera and dysentery rise paramount out of the fœtor and decomposition which everywhere prevail in Indian bazaars, during the intermission of the rains, when the sun scorches like a furnace, and the air is as still as the death which is impending. The records of the past direct us unmistakeably in the course which leads to the attainment of that most precious of the Divine gifts—long life; and energy and liberality are alone required to secure that blessing, if not for ourselves, assuredly for the fruition of those who surround us, and of those who are to follow us."

The great argument in this volume—that of a comprehensive hygiène, that of sanitary reform generally—is comprised in the best modes of encountering the removable causes of death, including of course therein the causes productive of feeble health and disease in all their varieties. After, in an introductory chapter, broadly treating of the modes of origin and the means of prevention of the causes of death which are either removable or mitigable, he proceeds, sketching the plan of his work:—

"We have seen that a large proportion of the deadliest and most prevailing diseases are the results of circumstances which it is in the power of humanity to control, and, perhaps, eventually to remove altogether; that, although the universal gift of old age can be looked for only among the blessings of the promised millennium, the power of considerably extending the term of his existence lies tangibly within the grasp of man; and that this faculty of moderating the great causes of death rests with the lawgiver rather than with the physician.

"We have now to consider," he continues, "some of the legislative measures and social rules which are obviously necessary for the diminution of the most prevalent causes of disease, and for the general prolongation of human life. The following appear to be the most requisite:—

"The encouragement of emigration from over-populated districts to healthy and productive colonies.

"The embankment of rivers, and the draining and cultivation of marsh and waste lands.

"The infliction of heavy penalties upon all persons found guilty of adulterating

any medical drug or any article of sustenance, or of vending the flesh or milk of ill-fed or diseased animals.

"Restriction in the sale of ardent spirits and of other intoxicating drinks.

"The proper building, ventilation, lighting, and draining of houses,—particularly those of the labouring population.

"The abundant supply of pure water to towns.

"The proper cleansing of all streets and thoroughfares.

"The clearing, regardless of opposition or expense, of all confined and notoriously unhealthy districts of cities, and the partition of the spaces of ground thus obtained as the sites of appropriate dwellings for the poor.

"Prohibition of intramural burial of the dead.

"The removal of all cattle-markets, slaughter-houses, piggeries, tan-yards, gas-works, &c., beyond the confines of towns.

"The erection of all extensive manufactories at the distance of at least two miles from the confines of large towns, with the provision of their being constructed in healthy situations, with proper regard to security, ventilation, warming, &c.

"Prevention of the retail sale of poisons.

"The suppression of all those trades which, while they produce no substantial benefit to the community at large, entail almost certain destruction of life or health on those who practise them; and the careful modification of all those useful trades or occupations which are attended with danger to health or risk of life.

"The due remuneration of the working-classes (especially in the manufacturing districts), and the limitation of their hours of labour.

"The opening of baths, washhouses, and places of exercise, for the use of the working classes in the vicinity of crowded cities, and in manufacturing districts.

"The establishment upon an extensive scale, throughout the country, of houses of temporary refuge for the destitute, where medical aid may be received, as well as assistance in obtaining proper employment.

"The introduction of better and more liberal rules than are at present in operation for the medical relief of the destitute sick, and for the support of incurable patients.

"The establishment of judicious systems for the reduction of the mortality in lunatic asylums, jails, and workhouses.

"The adoption of measures encouraging the poor to bring their children to be vaccinated.

"The employment of means tending to reduce the mortality among the children of the poor.

"The maintenance of a strict hygienic system among sailors and soldiers, at home and abroad.

"The enforcement of a well-conducted system of quarantine, whenever the introduction of pestilential disease is to be apprehended.

"The strict prohibition of the practice of medicine and surgery, as well as the sale and compounding of drugs, by unqualified persons.

"The gradual introduction of regulations calculated to improve the *morale* of populous districts, and to diffuse religious and useful instruction among all classes of the people."

Before separately considering each of these requirements, the author remarks:—

"Some of these provisions have long been in active, though partial operation; and their beneficial effects are daily becoming more and more strikingly apparent; others are just beginning to work, and will have to be much extended in their application before they can produce their intended and destined good; the remainder, it is to be regretted, have yet to be introduced; but the time is not distant at which the absolute necessity for their adoption must become evident to all."

On all these important matters the author gives, seriatim, much valuable and trustworthy information—such as might be expected from him. It is seldom that we find occasion to differ from him, or call in question the soundness of his views; and when we do, it is chiefly on points unsettled and open to discussion, such as the contagious nature of certain diseases, the propriety of enforcing quarantine, the extent to which it should be carried, and its efficacy—not to mention some other and minor things.

In our special notice of parts, for obvious reasons of time and space, we must restrict ourselves to one or two. We shall first select that entitled "The Employment of Means tending to Reduce the Mortality among the Children of the Poor."

We believe that it may be laid down as a principle, without exception, applicable equally to man and the brute, that the younger the animal the more helpless, the more exposed it is to noxious and destructive agents, and the more susceptible it is of their influence, and the more feeble its tenure of life. This may be one of Nature's checks against an undue increase of any one species—the weaker perishing, the stronger individuals surviving and reaching maturity to continue the race.

Be this as it may, as regards the final cause, certain it is that the preservation of infant life is very much in proportion to the judicious care that is taken of it. We have extreme examples afforded in the Foundling Hospital in its worst condition, and in a well-regulated family residing in a healthy locality in easy circumstances:—in the one, the great majority of the hapless innocents perishing in infancy; in the other, the greater number, it may be the whole, attaining the adult age.

Amongst the labouring poor, we have neither the one nor the other extreme—the ratio of infant mortality varying according to circumstances, from more than one-half of the whole registered mortality—as in the large towns, such as Manchester, Liverpool, Leeds; to less than one-third, as in the agricultural counties of Lancashire (north of Marcomb Bay), Westmoreland, Cumberland, and Northumberland. Nor is it at all surprising that there should be such differences of mortality, when we reflect on the principal causes to which it is attributable.

The following table (p. 348), extracted by the author from the 'London Medical Gazette,' November, 1846, displaying these causes, is an instructive document, as showing how, of the total 18,435 deaths at all ages in the metropolis, during one year, from the diseases specified, no less than 15,019, or 81·55 per cent., occurred in children under five years of age. It is true, the majority of the diseases given are infantile ones; yet there is one exception—viz., pneumonia, the proportional mortality from which, it will be seen, is only a degree less than from the whole, being as 71·85 to 81·55 per cent.:

Table of the principal Diseases causing Death in Children under Five Years, in the Metropolis, during 1844.

	Total deaths at all ages.			Under five years of age.	
Specific Contagion—					
Scarlatina	M. 1545, F. 1484	3029	...	M. 1126, F. 1025	2151, or 71·02 per cent.
Small-pox	M. 942, F. 862	1804	...	M. 633, F. 575	1208, or 66·96 "
Measles	M. 627, F. 555	1182	...	M. 586, F. 511	1097, or 92·80 "
Cold, or Atmospheric Influences—					
Pneumonia	M. 2149, F. 1915	4064	...	M. 1534, F. 1386	2920, or 71·85 "
Hooping-cough	M. 565, F. 727	1292	...	M. 534, F. 687	1221, or 94·50 "
Croup	M. 218, F. 193	411	...	M. 189, F. 163	352, or 85·64 "
Improper Feeding—					
Teething	M. 395, F. 333	728	...	M. 392, F. 333	725, or 99·58 "
Diarrhœa	M. 353, F. 352	705	...	M. 257, F. 242	499, or 70·78 "
Tabes mesenterica	M. 261, F. 201	462	...	M. 229, F. 178	407 or 88·09 "
After the above, or in consequence of other Diseases—					
Thrush	M. 129, F. 130	259	...	M. 127, F. 129	256, or 98·84 "
Convulsions	M. 1545, F. 1191	2736	...	M. 1512, F. 1146	2658, or 87·11 "
Hydrocephalus	M. 982, F. 781	1763	...	M. 865, F. 660	1525, or 86·48 "
Total		18,435			15,019, or 81·55 per cent.

The author refers to the following heads the chief destructive avoidable causes of disease and death in infancy and childhood:

"1. The crowding and injudicious management of infants in foundling hospitals and orphanages.
"2. Neglect on the part of parents.
"3. The systematic administration of narcotics.
"4. The confinement of children in ill-ventilated school-rooms."

The facts which he has brought forward on these several points may most of them be familiar to our professional readers; they are of a very striking kind, and admirably adapted to make an impression on the public, and to rouse to exertion all who have it in their power to check the evils. Some of the instances he adduces are almost of a kind to come under the consideration of the medical jurist, such as the fatal practice of drugging infants with laudanum, sold under the cant term of "quietness," and the nefarious tempting premium to the neglect of offspring, if not of their wilful murder, by having them in burial clubs; or the infamous practice of druggists administering to the destruction of infant life by the unstinted sale of the deadly drops. The author raises an indignant voice in reprobation, urging that there should be severe penalties on those

"Who render themselves accessories to systematic and wholesale murder by the sale of pennyworths of 'venomous distilment' to ignorant and inexperienced wretches who, in nine hundred and ninety-nine cases out of a thousand, have not the slightest conception that in thus drugging they have called *Death* to stand and watch beside the crib."

Let us pass to another and very different class—soldiers and sailors, those employed on active service—ranging in age from eighteen on an average to thirty-six (few of them are older), all of them chosen men—and who may, in consequence, as regards power of endurance, power of resisting noxious agents productive of disease and death, be considered as ranking highest in degree, as much so as infants must be admitted to rank lowest. Should it not follow, then, that were just sanitary rules observed in our armies and navies, the health of the men in each service, as marked by disease and death, ought to exceed that of the people generally including all ages? Now, what are the facts? In the instance of the home-station, it would appear that amongst our troops the mortality has never been lower than that of the mixed population of our healthiest districts (about 14 per 1000); whilst in some regiments, as the Foot Guards, it has been considerably higher, as much as 21·6 per 1000; and taking our foreign stations, it has ranged from 15·5 in the healthiest, as the Cape of Good Hope, to 483 in the least healthy, as in Sierra Leone, "the white man's grave." Statistics might be given to a great extent, and of a reliable kind, in proof of the unhealthiness of our troops, and their losses from sickness at different periods and in various climates, vastly exceeding the greatest sustained in action from the fire of the enemy; and the same remark applies to our navy. It is as well proved that the more attention has been given to their sanitary condition, the better has been their health, the greater their efficiency, and the less their mortality. The royal navy, as a whole, is a striking example of the good resulting from judicious sanitary measures. There was a time when our fleets were more than decimated by scurvy and crippled by disease; now, scurvy is almost banished from the navy; now, on the healthiest stations the mortality amongst the seamen is reduced below that of the mixed population at home. It also affords notable examples in particular instances, and even at present, of the direful effects of negligence of sanitary laws, such as are afforded by the outbreak of destructive fevers in ships, the holds and bilges of which have been allowed to become foul. Our army too, as a whole, and in particular instances, affords equally good examples of the benefit derived from attention to sanitary measures, and of the evils following their neglect. In the West India command and in Jamaica the mortality at one time varied from 80 to 250 per 1000; of late years it has seldom reached 80, and has been more commonly as low as 40: a change not connected with any alteration of climate, but with improvements in the men's barracks, less crowding, better ventilation; improvements in their diet, diminution of salt rations—still affording scope for improvement—and in other matters bearing on health. In the Crimea—to take the latest instance of the extremes—whilst in the winter of 1854 our troops before Sebastopol sustained, it is reported, from sickness the enormous loss of 350 per 1000 in a few months, the crews of the ships anchored off the same shore, and employed on the same service, retained their ordinary good health,

and experienced no increase of mortality. In brief, both services may be viewed as experiments made on the grandest scale, as if for the express purpose of testing the human constitution in various climates and under various circumstances, and of illustrating the principles of Hygiène. The experience obtained has been dearly purchased; but what is most to be regretted is, that it has so often been neglected or turned to so little account. Were not the destitute state of our army in the Crimea so well authenticated during the terrible winter alluded to, it could not be credited. Could it be believed that with a noble fleet of men-of-war and steam transports at hand, with the capital of the Turkish empire within two days' sail, with productive countries not more remote capable of furnishing cattle, corn, forage, fuel, and wood for hutting, a small force not exceeding 35,000 of all arms should have almost perished from want of wholesome food, shelter, and clothing! Even though so well authenticated, it is hardly credible! Is it not a most demonstrative proof of the want of sound knowledge of what constitutes the science of Hygiène on the part of the authorities who conducted the war, both in the Cabinet and in the field; and also of the little influence exercised by the medical department of the army in its special function, that of attending to and preserving the health of the troops under its care? Let us hope that the disasters before Sebastopol from negligence of sanitary laws and measures, will be a warning for the future, and lead both to a more efficient organization of the medical department in all its branches, and to the securing it more influence in an administrative capacity; for what is the use of knowledge without authority and the means of making it practical? We have no hesitation in expressing our opinion, founded on pretty wide experience, that were sanitary provisions carried out as they might be under ordinary circumstances, and as far as possible under circumstances of difficulty such as are likely to occur in any campaign, a vast saving of human life might be effected, especially in our colonies and at home, with increased efficiency and contentment, with less malingering, desertion, and suicide—chiefly by the selection of healthier quarters, by the adoption of a more varied and wholesome diet, and the use of more suitable clothing. Space does not permit us to enter into details. Many of the particulars we could relate in illustration would hardly come short of those which have been so perfectly authenticated as having occurred in the Crimea. But in reflecting on our own experience, we are forgetting our author. That part of his work relating to "The maintenance of a strict hygiènic system among sailors and soldiers at home and abroad," will well repay perusal; it furnishes ample facts demonstrative of the evils of neglect of a sanitary system, whether in the army, or navy, or the merchant service, and of the marvellous and blessed effects following its trustful and careful adoption. Regretting that we must pass over unnoticed so much that is valuable and impressive in this work, and indulging the hope that we shall soon see the second volume, we shall finish with one more extract—his concluding paragraph—in all the aspirations expressed in which we heartily join him:

"It is trusted that enough has now been said to prove that, mortal and transitory as the condition of man inevitably is, he has been endowed with a power of lengthening as well as of improving his earthly existence; that this remarkable

power has hitherto been cast aside with a strange and wilful carelessness; and that the means by which it may be exercised, although but very imperfectly developed at present, are beginning to assume the form of a great and beautiful science. Let all who possess the benefits of intellect and education—the divine in his study and in the house of prayer, the general in his camp, the legislator and the magistrate in their constant ordering of the people, and the physician in his daily conversance with scenes of every kind and intercourse with men of every class—let each to the fullest measure of his ability join earnestly in the development of this inestimable science—

> 'Still educing good,
> And better still, and better thence again,
> In infinite progression,'

and we may venture to predict that the end of this century will find the civilized nations of the world twice as happy—nay, it may be, twice as virtuous—as they were at the opening of its sixth decade!"

Review VI.

Mémoires de l'Académie Impériale de Médecine. Tome XIX. *Paris,* 1855. 4to.

The preliminary pages of this volume are occupied with—1. Éloges of Désormeaux, Capuron, Déneux, and Baudelocque, from the pen of the Secretary, M. F. Dubois, who, in this species of composition, bids fair to rival the successful Pariset; 2. A Report on the Prizes adjudged by the Academy, by M. Gibert; and 3. A Report, by M. Gaultier de Claubry, upon the Epidemics which prevailed in France during 1853. These we pass over, and proceed to the memoirs contained in the volume; the first calling for notice is

I. *On the Obliteration of the Umbilical Arteries, and on Umbilical Arteritis.* By M. Notta.

M. Notta first alludes to his former researches* upon the obliteration of arteries after ligature. From these he concluded that the portion of the artery comprised between the ligature and the first collateral does not become transformed into a fibrous cord, but only undergoes an atrophy that still allows the various elements of its structure to be recognised. A persistent coagulum fills this space of the artery, but undergoes no transformation. To this statement, the fibrous transformation of the umbilical arteries was objected, and the present paper is devoted to considering the validity of such objection.

In the first place, the results of M. Robin's researches upon the *structure* of the umbilical artery are brought forward. According to these, the middle tunic is almost exclusively composed of the muscular fibres of organic life, while, in place of obtaining its blood by imbibition, as in the case of other arteries, it is probably vascular; for, although it has never been injected, it is very prone to inflammation, which is followed by the same results as are seen in vascular parts. Under the ligature, however, this coat is divided exactly as in other arteries.

Examined within the first three days after birth, the umbilical arteries

* Gazette Médicale, 1850.

may be found completely devoid of coagula, or, if these exist, they are of variable length, soft, and only slightly adherent. The walls of the artery, by reason of their great contractility, may be in contact, but they are easily separated by means of a probe. Near the umbilicus the arteries are almost always tumefied to double or triple their volume. From the eleventh to the twenty-first day a fine probe can still be introduced by the hypogastric. The parietes are brought into contact, so as to prevent the access of blood, but they are not adherent, the lining membrane retaining all its smoothness and polish. The arteries participating in the development of the parietes of the abdomen are somewhat larger than at birth, but they exhibit no thickening or hypertrophy in their course, beyond the tumefaction near the umbilicus. As to the clot, when it exists, it is now adherent, dense, and reddish; but, later, gradually disappears, so that not a trace of it remains when the fibrous transformation is completed. Although farther observation is required, it is certain that a longer period is required to effect this obliteration than is usually supposed, since M. E. Dubois has found it unaffected in six or seven weeks. The inconstancy of the formation of the clot, and its variable extent, are due to the variable amount of blood that continues to pass, proportionate to the greater or less amount of contractility of the middle coat.

As already stated, about the third day the umbilical arteries present a dilatation near the umbilicus, which is obviously connected with the fall of the funis. There is always, in fact, a certain amount of inflammation attendant upon this fall, although it does not always induce suppuration. This inflammation invades the umbilical arteries to a certain extent, and according to the intensity of the phlegmasia, the tumefaction is more or less considerable. It is always confined to the vicinity of the umbilicus, the conditions being pathological under which it extends to two or three centimètres beyond; while M. Notta has never found it implicating the entire length of the arteries. M. Notta thus sums up these various changes:

"Thus, a less considerable quantity of blood in the umbilical artery, contraction of the artery throughout its entire extent, formation of a coagulum (which, however, is not essential, the contraction of the artery sufficing in a great number of cases for the obliteration of its calibre), inflammatory tumefaction of the walls of the artery at the umbilicus only, then resorption of the coagulum when this exists, and the ulterior transformation of the parietes into a fibrous cord, which participates in the development of the abdomen; these are the various phases through which the umbilical arteries pass. It is evident how much these differ from the mode of the obliteration of arteries after ligature, such as I have observed it, and as I will recapitulate it to facilitate comparison. Immediately after the ligature, the column of blood continues to fill the vessel (which does not contract) to its extremity. A coagulum is formed, and fills the artery as far as the first collateral, whatever be its size, providing it be permeable. The clot and the portion of the artery enclosing it persist, and are never transformed into a fibrous cord." (p. 7.)

The inflammatory tumefaction of the arteries near the umbilicus, which may, from its intimate connexion with the fall of the funis, be termed physiological, is not always restrained by these limits, but may increase in intensity, so as to give rise to suppuration of the arterial walls and its consequences. So little attention has been excited by *umbilical arteritis*,

that there are few cases on record, although the affection cannot be so rare, as the author has himself been enabled to deposit five examples of it in the Dupuytren Museum,* the particulars of which he now furnishes. In four of these the funis had become detached, and in the other it still adhered; and in all there was pus in the umbilical depression. In two cases the umbilicus was surrounded by a rose-coloured areola, but it was never much tumefied. In all the cases, the arteries near the umbilicus presented the characters of violent phlegmasia, the middle coat, contrary to what takes places in other arteries, actively participating in this. In all, the pus was separated on the side of the hypogastric artery by a small, adherent, fibrinous clot, that varied from two to fifteen millimètres in length, but never reached to the hypogastric artery. In two of the cases, the cavity of the peritoneum contained serosity, and the portion covering the arteries was injected. In two other cases, there was no peritoneal injection; and nothing besides the tumefaction could give rise to the suspicion of the arterial lesion, which would certainly have been overlooked had not attention been already directed to the subject. In all these cases the umbilical veins were healthy. Nevertheless, in thirteen cases of umbilical phlebitis, the author has found accounts in three of them of a coexisting arteritis.

In regard to the question whether umbilical arteritis is always fatal, it is to be observed, that in the author's cases this constituted the principal lesion, so that we may conclude that it alone may induce death, especially when the phlegmasia invades the peritoneum. Still, as a diagnosis of the disease has never been made during life, we cannot state that it has never been cured. It is even probable that the prolonged umbilical suppurations which terminate in recovery, are sometimes due to an arteritis of small extent. However this may be, it is a most dangerous affection, both from the vicinity to the peritoneum, and because it may become the point of departure of infantile erysipelas, the seat of which is always near the navel.

The diagnosis is difficult, for often there is nothing externally that reveals the arterial inflammation. In two of the cases only was there a little redness around the navel; nothing existing in the others beyond a little pus at the bottom of the umbilicus, which being covered with a dry crust, might escape careless examination. The treatment should be especially preventive, avoiding all friction of the umbilicus by hard napkins, observing the strictest cleanliness, and washing away, by marsh-mallow lotions, any pus that may remain beneath the crust which succeeds the fall of the funis. In this way we may moderate any inflammation that exists, and prevent its propagation to the arteries. If a collection of pus is recognised, we should open it sufficiently to allow of free issue.

II. *Case of Successful Ligature of the Arteria Innominata.* By M. Peixoto.

Among several interesting "Cases in Surgery" communicated by M. Peixoto to the Academy, we select this one for transcription. M. Moura,

* Dr. Schöller has met with inflammation of the umbilical arteries in 15 out of 18 cases of trismus neonatorum. Dr. Collis, of Dublin, also attributes this affection to umbilical arteritis. See Jones and Sieveking's Manual of Pathological Anatomy, p. 345.—Ed.

a distinguished Portuguese doctor of medicine, aged thirty-three, was the subject of it. An erectile tumour of the right ear began to develop itself in 1832, and in 1845, M. Nélaton tied the posterior auricular, considerable hæmorrhage following the fall of the ligature. After temporary amendment, the tumour again made great progress, and frequently gave rise to serious hæmorrhage; the patient being then at Rio Janeiro, M. Peixoto tied the common carotid in the middle of its course, 14th November, 1851, and on the 27th, surrounded the tumour itself by a ligature, which induced its separation by sloughing. On the 4th December, some bleeding was observed where the carotid had been tied, the ligature not having yet come away; and as the hæmorrhage recurred again, it was resolved to apply a precautionary ligature (*d'attente*) lower down. On the 8th, this was executed on the trunk of the innominata; and in a later communication to the Academy, the author states the cure was completed in two months.

M. Velpeau, reporting upon this case,* observes, that as far as he is aware, this is the first example of a cure resulting from the artificial obliteration of this artery; although the cases of accidental occlusion published by Pelletan, Martin-Solon, and Darrach had already shown that its occlusion did not deprive either the arm or the brain of a sufficient supply of blood. The cases of operation have hitherto all terminated fatally. Mott, who first practised the operation in 1818, lost his patient on the twenty-sixth day. Gräfe's patient died on the sixty-eighth day, Bland's on the eighteenth, and Hall's on the sixth. In Lizars' case, death occurred at the end of three weeks. After M. Kühl's operation, in which the ligature comprised the subclavian and the carotid close to the innominata, death took place on the third day. A patient of M. Arendt's died on the eighth day; and two operations performed by M. Bujalski were followed by death in two or three days. Finally, M. Hutin lost his patient on the eighth day. So that ten operations have furnished as many deaths.

After all, M. Velpeau adds, this is not an example of ligature of the innominata, properly so called; for although a ligature (*d'attente*) was applied to and flattened the vessel, this was not tightened. The patient was cured, but nothing allows us to affirm that the ligature bore rather upon the common trunk than upon the origin of the carotid alone. Nor is there anything that absolutely proves the closure of the innominata, if closed it be, not to have taken place as a consequence of the first ligature, rather than under the mere influence of a ligature *d'attente*.

III. *Reduction of a Complete Inversion of the Uterus at the end of Fifteen Months.* By M. Barrier.

Marie Michaud, a scrofulous subject, aged twenty-four, was confined, after a natural labour of ten or twelve hours' duration, 14th September, 1851. The removal of the placenta was accompanied by traction, and followed by hæmorrhage, and three days afterwards a large tumour protruded externally, which, however, was returned within the vagina. From that time severe hæmorrhage had recurred every few days, which much

* Bull. de l'Acad., tom. xix.

exhausted her strength. She entered the Hôtel-Dieu at Lyons, of which M. Barrier is surgeon, 13th February, 1852. A tumour was found just within the vagina, consisting of the completely inverted uterus, somewhat larger than normal. Tonics were administered to her, and on the 9th March, reduction attempted. This was easily effected, under etherization, and with little loss of blood. A bladder of vulcanized caoutchouc was introduced into the vagina, and insufflated, removing it morning and evening, in order to inject the vagina, and discontinuing it after the 11th. The patient continued to improve, was enabled to walk about a little by the beginning of April, by the 15th of which month she was considered as cured.

We can only give the title of the next paper, which is—

IV. *A Critical Examination of the Efficacy of Emollients.* By Dr. Delioux;

And pass on to—

V. *The Pathological Anatomy of the various Forms of Goître, and the Treatment suitable for them.* By M. Bach.

This memoir forms a portion of M. Bach's essay, that recently obtained the Academy prize. He prefixes to it an account of the normal histological characters of the thyroid gland; but as he adds nothing to what is already known, we shall merely give a summary of the author's description of the varieties presented by the affection. He establishes three principal forms, accordingly as the vascular, glandular, or connective tissues are the seat of the hypertrophy which constitutes the essential feature of the disease.

1. *Vascular Goître.*

(1) *Congestion.*—The thyroid, owing to its abundant supply of large vessels distributed in lax cellular tissue, is perhaps the organ, next to the spleen, most liable to considerable changes of size from congestion. As, however, this is rarely fatal, there have been few opportunities of observing its anatomy. M. Bach has met with two. The one occurred in a fœtus delivered by the forceps, and the thyroid was found dark and resistent, like a piece of apoplectic lung. The vessels were gorged with blood. The other thyroid was taken from a woman who died in puerperal convulsions. It had the same appearance, and was reduced to about half the volume, when incisions had evacuated abundance of blood. The gland was only gorged, for no coagula were found in its substance. After long maceration, its texture was found quite normal. Instances of great turgescence of the gland are often observed during life, and M. Bach alludes to one such occurring in a child born with the forceps, who presented an enormous goître, which felt rather hard. Next day it had diminished one-half, and in four days had disappeared. About puberty, turgescence is frequent; and if menstruation is ill-developed, it may become permanent. In young persons, masturbation is a frequent cause of such congestion.

(2) *Thyroidean Apoplexy.*—Sometimes excessive turgescence or ulceration may give rise to rupture of a vessel, and then effusion of blood takes place. In two thyroids, M. Bach has found a coagulum of blood enclosed in a kind of cellular pouch. In one, this pouch had become very

dense, and almost identified with the coagulum, which was about the size of a walnut. No trace of vessels could be discovered in the coagulum, and the capsules of the gland in its immediate vicinity seemed destroyed; while at some points they were deformed and heaped together, presenting a fibrous appearance. In both cases, the gland was well developed, without being hypertrophied, and the arteries were not ossified. The coagulum undergoes the same changes as in other apoplexies, and may lay the foundation to one form of cystic goître. In place of this, however, a cicatricial tissue may be produced, which may extend in different directions, destroying the glandular capsules. The vessels of these capsules and the finer capillaries become atrophied, so that goîtres of this kind are the least vascular, and, though small in size, extremely hard, owing to the retractile power exerted by the cicatricial tissue. The cicatrix, by continuity of tissue with the envelope of the gland, may exert a certain retractile effect upon that membrane, which may injuriously compress the larynx and trachea. Two examples are given in which such compression gave rise to serious symptoms. In this cicatricial tissue, calcareous deposit easily occurs.

(3) *True Aneurismal Goître* has been noticed by Walther and others, but only incompletely described. A case has never occurred to M. Bach; and he suspects that some of those described and treated as aneurismal have been really examples of permanent congestion with great arterial activity, and capable of relief by other means than operation.

(4) *Vascular Parenchymatous Goître.*—In this form, the description of which the author for the most part borrows from Ecker, certain metamorphoses occur under the influence of hyperæmia, affecting at first only small portions of a lobe, but eventually involving the whole gland. The part affected becomes isolated from the sound parts by condensed cellular tissue, so that the degenerated lobules may be easily enucleated. When seen at an early stage of the change, the lobule is of a deep red colour, the granular appearance having almost disappeared at the surface of the section. In certain parts, Ecker found the glandular capsules in a state of integrity; vessels somewhat dilated and gorged with blood, ramifying in the compact tissue, separating these. Towards the centre, the vessels had undergone more dilatation, the surrounding tissue had become denser, and the capsules had well-nigh disappeared. The dilatations of the minute vessels assume an ampullar form, of varying diameter; on opening these ampullæ, which offer a considerable resistance to pressure, amidst globules that are in a normal state, others, altered in form and deprived of colour, are observed, strongly adherent to each other. Another change that takes place as a consequence of this altered condition of the capillary system is, the calcareous incrustation of the minutest vessels, these being much smaller than those undergoing the ampullar dilatation. Still, the latter vessels are also sometimes the seat of incrustation also. These incrustations are to be distinguished from those met with in cystic or other forms of goître, and which have another origin.

2. *Parenchymatous Glandular or Lymphatic Goître.*

This is due to hypertrophy of the substance of the gland itself, or, to speak more exactly, to the abnormal development of the glandular

capsules distended by a fluid of a gelatinous consistency. It is not secondary to vascular goître, already described, but may coexist with it. After describing the views held by Beck and Ecker upon this transformation, Dr. Bach gives the result of his own observations. He found

"Agglomerations of normal capsules, some capsules filled with blood, and lastly, capsules of half a millimètre in diameter filled with fluid. These larger capsules present an anhistous envelope, containing a smaller plaited one, and separated from the first by a hyaline fluid, and a little punctuated mass. The inner capsule is formed of cells like those constituting the normal contents of the capsules, almost all having a nucleus, some containing one or two young cells, and all containing granules analogous to those of the surrounding gelatinous mass. The centre of the capsule appears hollow, and contains young capsules, which themselves enclose less compact cells, not forming a complete epithelial layer, and destitute of nuclei.

"These corollaries result from my researches. Capsules of half a millimètre in diameter are already diseased. Gelatinous substance enters the capsules by endosmosis, and distends the anhistous membrane. The epithelial covering, formed of the characteristic cells, shrivels up, and is repelled towards the centre. The gelatinous matter is also introduced into the internal cyst, but without distending it. The inner cyst is larger than a normal capsule, and may become the seat of new capsules. The young capsules are formed of cells by endogenous generation, and are clothed later by an anhistous membrane. The young cells of the young cysts only acquire nuclei at a later period. In portions of these thyroids, cells are observed, and the capsules are wanting, and in other portions more degenerated there is only a punctuated mass representing the *débris* of cells. These researches prove the accuracy of the observations of Rokitansky; and, with that author, I admit that the thyroid gland is endowed with great power of production. It would seem probable, from the researches of Beck, Ecker, Rokitansky, and myself, that the change in the capsule does not always occur in the same manner, but the production of gelatinous matter in the primary glandular capsule, is always the definitive result." (p. 368.)

It is this glandular parenchymatous goître that is oftenest found endemic, manifesting itself soon after birth, and increasing with the individual. The *goître of new-born children* has been but little observed. The subjects are plump and well-looking, and the neck seems merely somewhat large, or the seat of an accumulation of fat. Obstruction of respiration, which in a few hours may lead to death, may, however, occur. The inspirations are long and deep, and are accompanied by a peculiar plaintive tone, that is heard at a considerable distance. Cries indicative of suffering attend expiration. In bad cases the child is unable, owing to the dyspnœa caused, either to drink or suck. It struggles with the difficulty of respiration, and at last perhaps perishes almost unexpectedly. In a case witnessed by the author, one of the most remarkable circumstances was the excretion of an enormous quantity of bronchial foam, resembling that observed after division of the pneumogastric nerve in experiments, and he supposes the external part of the gland induced irritation in that nerve. The author does not agree with Betz, that there is no alteration observed in the gland in these cases. He found the glandular capsules increased to 0·150 or 160 millimètres, in place of 0·075 millimètres, the extreme normal limit, while great hyperæmia was also present. He believes that some cases of thymic asthma are really due to a posterior development of the thyroid. In very urgent cases no treatment is of avail, but in milder ones, leeching and iodine ointment may be resorted to.

3. *Metamorphoses which the Stroma and Cellular Tissue of the Thyroid undergo.*

The stroma of this gland is a fibro-cellular tissue, in which ramifies a rich network of bloodvessels. The newly-formed tissue most frequently met with is the fibrous, the product of an amorphous blastema due to the stasis of the blood in the vessels, to its extravasation, or to its exudation through the capillary tissue; and its primordial texture is the same as that of normal fibrin. One of the characters of accidental fibrous tissue is its retractility, as already observed when treating of apoplectic goîtres; and it may undergo the fatty, osseous, and cretaceous transformation. So also, air may be effused amidst the cellular tissue: and new products may be there developed which are not met with in the healthy economy.

(1) *Cellular Goître.*—A species of goître is not infrequently met with that takes on a rapid and very large development, and has been well described by Heidenreich. Its progress is too rapid to be due to the development of the gland capsules, and remedies suitable for glandular goître fail here. M. Bach has carefully examined such a goître, as it occurred in a man aged sixty. He found cells of an irregular shape, rather angular than rounded, some of them intercommunicating. Their walls were thickish, and sometimes contained cartilaginous or osseous matter. The fluids in the cells were sometimes serous, and at others gelatinous or bloody. The glandular element was not to be recognised; and although the capsules were in some points unaltered, they were separated by layers of degenerated cellular tissue. The arteries could not be injected, and the venous plexuses were much dilated. The microscope showed the walls of the cells to consist of fibrous tissue of new formation, composed of bundles of homogeneous fibres. Between these were scattered here and there some granules, fusiform cells, and muscular fibres of organic life. In other portions the elements of cellular tissue, together with some fibres, were observed.

(2) *Emphysematous Goître.*—This form has been described by Larrey under the name of *aërien;* it is due to no primary change in the thyroid gland, but is produced by a fissure or rupture taking place in some portion of the air-tube. Sometimes, however, the air penetrates into the tissue of the gland, and induces alterations due to inflammation and changes in the glandular capsules.

(3) *Scirrhus and Encephaloid of the Thyroid.*—This is so rare that M. Bach is unable to refer to any well-authenticated recorded example; the disease so termed by Larrey, being really a *ramollissement* of lymphatic glands. So, too, there is no example of *tubercle* of the gland on record, and the few instances of *hydatids* may have been really examples of capsular cysts.

(4) *Cystic Goître.*—This name is given to a tumour formed by a cyst developed in the midst of the normal elements of the thyroid gland, and containing a more or less liquid or sometimes solid product of new formation. This was at one time termed *false goître,* but it is as properly goître as is the parenchymatous form. It may originate in various modes, as from a pathological blastema, the product of inflammation, the

metamorphosis of an apoplectic clot, or the degeneration of one or more glandular capsules. To these primary transformations others succeed secondarily.

Whatever be the origin of the cyst, its walls consist of a layer of cellular tissue, amidst which fibres are deposited. Vessels are developed within the walls, most of these being of new formation; and all the internal surface of the cyst is lined with epithelium. In some rare cases, the walls are so thin as to be nearly transparent; but they are usually thick, and especially in front. Posteriorly, however, they are generally as thin as paper, and the vessels when distended project into the cavity containing the fluid. This liquid forms a support to the vessels, and rupture of their walls, with hæmorrhage, may be the result of evacuating it. These projecting vessels are easily ruptured by pressure with a blunt body, explaining one of the causes of the hæmorrhages that are so frequent in operations on these parts. The walls not infrequently undergo cartilaginous transformation, which has been called *enchondroma*, and which consists in varying proportions of hyaline tissue and cartilaginous fibres. After more or less time, this has a tendency to pass into *osseous* tissue, years, however, being required before the entire cyst is so metamorphosed.

The cyst encloses either serosity or colloid matter. The first proceeds from the serum of effused blood that has not been taken up by the absorbents, and its quantity is increased by endosmosis from the afflux of blood which takes place towards the cyst. It contains no fibrin, and is incapable of organization. It is sometimes absorbed. Inflammation may sometimes induce such absorption, or it may convert the internal wall into a pyogenic membrane. Frequently, in proportion as the serosity disappears, it is replaced by products of new formation—such as crystals of cholesterine or the salts of lime, these being deposited in mass, or in the walls of the sac; the *colloid* matter is a colourless or yellowish gelatinous mass, of the chemical composition of which different accounts are given. It is, however, amorphous, and does not contain cells, those met with being detached from the epithelium.

Treatment.—We do not find much of novelty in the author's directions for the preventive and medical treatment. He thinks, as a general rule, practitioners too eagerly resort to specific remedies, without previously attempting to subdue by bloodletting the congestion upon which the affection so often depends. Of the use of *iodine* he expresses himself as follows:—

"It is not suited to congestive or any of the varieties of vascular goître, and should not be resorted to until all congestion and vascular action have been subdued, supposing the antiphlogistic treatment has not triumphed over the disease. It will then prove of use, because our object is to destroy either a product of exudation, or a commencement of the transformation of the capsules into colloid matter. We must not rely upon it in certain encysted goîtres. It will not cause the disappearance of a voluminous cystic goître containing colloid matter, nor exert any action upon those containing serosity. No advantage will be derived from it when the cysts have undergone tertiary transformations. It will partially disperse the parenchymatous cystic goître, on condition that this is principally due to the degeneration of the capsules, and that the vascular element does not predominate. Iodine will almost always triumph over parenchymatous

goître, however large this may be, and especially where the capsules have not taken on excessive development. It exerts no action upon cellular goître, or in that accompanied by carcinomatous or other morbid products." (p. 417.)

With respect to the *surgical treatment*, as this is often attended with considerable danger, M. Bach lays down the rule that it should never be had recourse to except when the patient is much inconvenienced by the disease, and his life is or may soon be placed in danger. Of the various operations, *puncture* is only to be practised when suppuration exists, or as a means of emptying a cyst prior to its injection. Not infrequently, after evacuating the fluid, a distended artery may give way, and rapidly fill the cyst with blood, the tumour afterwards enlarging more than ever. Of all the *injections*, iodine is the only one that should be employed, and this not only from its greater innocuity, but from the specific influence it may exert. Even with this, inflammation and suppuration of the cyst are sometimes induced. It is often, too, very difficult to detect fluctuation, and when it is evident, we cannot decide whether the cyst be unilocular or multilocular. If the glandular capsules are very dilated, they may also give rise to fluctuation, when puncture discharges very little fluid, and the goître subsides only incompletely. The *seton* is unsuited for the vascular, the glandular, the parenchymatous cystic, or in voluminous cystic with thickened walls. It should be reserved for cystic goître of moderate size, and presumably thin walls. The double suture, impregnated with chloride of zinc, and passed at right angles, is the best form. *Incision* should be confined to cystic tumours of large size, puncture and injection being preferable when the walls are thin. As with the seton, the danger of hæmorrhage or inflammation is considerable. *Cauterization*, though discountenanced by most surgeons, the author has seen advantageously employed in M. Bonnet's mode in voluminous cysts containing concretions; but its effects are mischievous when it is applied to tumours not containing fluid. The *ligature of the superior thyroid arteries* alone, in the author's opinion, is of little avail, while the difficulty of securing the whole of them must ever render the operation quite exceptional. On the total or partial *excision* of the thyroid, M. Bach adds nothing to our present information, but he states that his own success in the operation of extirpation by *ligature en masse*, has caused him to feel surprised at the little favour surgeons have shown to it. Certain goîtres are not amenable to it, as those of immense size, cellular goître without defined limits, and goître closely adherent to the larynx or trachea; but as great numbers of goîtres are not found in these conditions, in the majority of cases the ligature is applicable. The nature of the tumour is never a contra-indication, although the operation is better suited to some forms than others. The absence of pedicle is not the objection it is supposed by some to be, the author having applied the ligature around a portion of the gland five or six centimètres in diameter, without any ill effects. The constriction must be made very gradually, or dyspnœa or nervous irritation may result; and it is not until twenty-four or thirty hours that the complete arrest of the circulation in the tumour should be effected. After the fourth day, the constriction should be temporarily discontinued, for at this time there is a tendency to consolidation of the exudation. Three days later, the ligature is again tightened, so as to slowly induce the gangrene of the part embraced.

VI. *The Pathological Anatomy of the Cicatrices of the Various Tissues.* By Dr. F. Hutin.

In this prize essay, the subject of the reunion of divided parts is pursued in detail through the various tissues, but the only portion we deem it desirable to lay before our readers is that treating of the *Pathological Affections of Cicatrices*, a subject which the author's position, as surgeon to the *Invalides*, has given him ample opportunity of studying.

In general, small cicatrices are not painful, although they are sometimes the seat of pruritus, and a troublesome sense of dryness, for which no causes can be assigned beyond the absence of local transpiration, and the imperfection of their structure. The subjects of large and deep-seated cicatrices, however, are very liable to severe pains proceeding from the inodular texture itself, or from the neighbouring tissues, and produced by the changed relations of parts, new adhesions, injury to nerves, &c. Severe lancinating pains seem to be due to certain filaments of nerves which run contiguous to the inodular substance, or terminating with more or less swelling near its circumference. The pains are always aggravated in wet and stormy weather, and the patients complain of a sensation as if the cicatrices were tense or swollen; but the most careful measurement detects no difference. These sensations disappear if we cover the cicatrix with wadding or other warm substance, while they persist if we cover the surrounding parts and leave the cicatrix exposed. When adherent cicatrices are situated over very moveable parts, the movements induce tractions that are very painful; and if there be osseous inequalities beneath, ulceration or laceration of the cicatrix may occur.

Cicatrices may easily become the seat of inflammation, and an erythema may readily pass into ulceration; while, if the inflammation be severe, gangrene may result. After a certain period, only rare capillary vessels ramify through the inodular tissue, the vessels which traversed it at the time of its formation having become converted into fibrous cords, so that the finest injection does not penetrate. This operates as some protection against inflammation. We find varicose veins ramifying in extensive cicatrices, although, for the most part, they are seated beneath the cicatricial tissue, through which they are visible. Œdema sometimes raises cicatrices as it does the rest of the skin, although less easily when there are adhesions. Ecchymosis is also met with, but rarely without excoriation. Cicatrices may be the seat of hypertrophy, and M. Hutin gives an account of the dissection of one, resulting from an abscess in the thigh, which had been submitted to pressure during the occupation of a shoemaker. It was a simple hypertrophy, in which the surrounding skin did not participate, the cicatrix being triple its proper thickness, projecting a centimètre above the level of the skin. Hypertrophy of the neighbouring skin, as in elephantiasis, may sometimes extend to the cicatrices, but in other instances does not do so.

Sometimes cicatrices become covered with small conical or nipple-like elevations, which in consistence and colour much resemble corns, and, in certain cases, large, hard, and lamellated, present some analogy to horns of the skin. They are, however, but the result of dirtiness and

prolonged pressure, occurring especially in cicatrices possessed of depressions and furrows, in which epidermis, dust, and other bodies accumulate and become adherent to the skin through the agency of its transpiration. The projections are not implanted in the cicatrix, but adhere to it, and fall off at various periods, from some weeks to a year. Cleanliness is the remedy. In other persons, however, really adherent eminences are seen, and are true excrescences from the cicatricial tissue, or they may arise from the surrounding normal parts. Indeed, it is very rare to meet them on the cicatrix, without any participation of the skin. Sometimes, again, cicatrices serve for the implantation of horny substances of various forms, which sometimes acquire a large size. Two examples are given by M. Hutin, in one of which the horn, of a spiral form, was ten centimètres in length; and in the other, reached a length of five centimètres, with a base of three. Usually, however, these horn-like substances are of much less size, always being hard at their free extremity, and becoming softer as they approach the point of implantation. The especial seat of those of a small size seems to be the extremity of the stump after amputation, and chiefly amputation of the thigh—the cicatrix in these cases being subjected to much and constant pressure.

The author has twice met with the *warty* affection of cicatrices described by Hawkins. In the first case, the growth was the size of a small nut, and resembled the warts observed on the fingers. The other more resembled fungus hæmatodes, and grew from a portion of the cicatrix of a large ulcer of the leg; seeming, however, more intimately united to the surrounding skin than to this. The author only made a transient examination of this growth; but he suggests that both it and the examples described by Hawkins may be varieties of cancer. The latter and other malignant diseases sometimes attack the cicatrix, and especially when this is large, and situated on the lower extremity. The most curious and rare accidental production M. Hutin has met with, was a kind of *keloid*, in the person of a soldier, who, at the age of twenty-six, received (November, 1839) many blows with a yatagan on various parts of his body, one of these striking the left ear, and another the point of the left shoulder. While reparation of the last two wounds which resulted was going on, vegetations sprang from the bottom, which were mistaken for ordinary granulations, and were kept down by nitrate of silver. In forty days the wounds were quite closed, but the vegetations, covered with epidermis, continued to make progress, and after a while they became so large and troublesome, that M. Gimelle removed those of the shoulder in 1842. Those of the submastoidean region were removed in 1843. These excrescences were reproduced with the same activity in the new cicatrices, while nothing of the sort was observed in those of the thirteen other wounds. The excrescences still remain, although somewhat diminished in size, the result of the operations of Nature, after the failure of every application. *Cysts* of various kinds not infrequently are developed on cicatrices, but they are rather formed at the expense of persistent or neighbouring sebaceous follicles, than of the inodular substance itself. In old cicatrices, especially when large, *cartilaginous and osseous deposits* may occur. Ossification is of more common appearance than is that of accidental cartilage. A cellular layer, a kind of periosteum, surrounds this

bony deposit, the existence of which is only demonstrable after maceration, and its function as periosteum is problematical.

Slight and oblique *contusion* may do little mischief to a cicatrix, but when this is violent, it almost always leads to ulceration. In all cases, there is a tumefaction and thickening of the tissue, due to the effusion of fluid. The cicatrix is easily destroyed, either wholly or in part; and then the tissues which it had retracted separate again by their own elasticity and muscularity. Ulceration takes place rapidly in inodular tissue, so that the wound may speedily resume its original dimensions. The secondary reparation is much slower, and its different phases are liable to interruption by various accidents. Inflammation and rupture of cicatrices, as a general rule, are more likely to occur at an early period of their formation. This depends upon their retractility being more active, and their organization less complete; while the surrounding textures, still nearly approaching a pathological condition, are very susceptible of undergoing alterations. At a maturer stage, the cicatrix will acquire a greater power of resistance. But when it is thin, when it covers an extensive surface, and when there is much loss of substance in parts subjected to frequent or extensive motion, we occasionally find an old cicatrix giving way as a consequence of slight external violence. The wounds of cicatrices made by pointed instruments of small size are not of much importance; and thus we daily find healing without difficulty the bites made by leeches, and the punctures made by a needle or a lancet, though these may traverse the entire substance. The action of a cutting instrument, applied to a large surface, is less inoffensive, reparation then requiring more care and time. Healing by the first intention is observed every day; and certainly the excessive care some surgeons take in avoiding old cicatrices is far from being always justified. Still, as this mode of reparation is sometimes defective, it behoves the operator to avoid such cicatrices as far as possible. If suppuration occur, it is very rare that the old cicatrix is not entirely destroyed, and that especially in the case of wounds from contusion or fire-arms. The ordinary phenomena of wounds of cicatrices are always more energetic than in the solutions of continuity of other tissues.

VII. *On the Treatment of Chorea by Gymnastic Exercises.* By Dr. Blache.

M. Blache observes, that two indications should guide us in our treatment of this affection: 1. To restore to the will its empire over the muscular contractions—i.e., regularize the movements; and 2. So to say, reform the constitution of the patients.

M. Blache regarded the methodical use of sulphur baths as the best constitutional remedy for the affection, until he had recourse to gymnastics. The great success which attended the application of these in 1847, under the skilful direction of M. Laisné, to scrofulous subjects at the *Hôpital des Enfans*, induced the directors to erect large gymnasia, and extend their employment to various other diseases, among which was chorea. In the present paper, M. Blache gives an account of 108 cases so treated, 100 being first attacks, and 8 only relapses—an important distinction, as the ordinary duration of a case of chorea is diminished by a number of relapses.

These 108 were divided into two categories, according to the severity of the disease; one of these being composed of 34 cases, of mean intensity, and the other of 74, in which the agitation was as violent as possible. The whole of the 34 were cured in a mean period of twenty-six days and eighteen *séances;* of the 74, 68 were cured in forty-five days with thirty-one *séances.* Therefore there remained but 6 cases which may be regarded as failures. These were examples of chronic chorea, which in the end was also cured, requiring 122 days and 63 *séances.* Calculating in another mode, we have 102 cases in 39 days, and 6 in 122 days.

For the description of the procedure that is had recourse to, M. Blache takes an aggravated case, in which the movements are violent and speech impossible. Here the will of the subject being powerless, nothing can be demanded of him; and the gymnastics must be entirely passive. M. Laisné, aided by three or four intelligent pupils, fixes the patient on his back in bed, and retains him thus motionless for ten or fifteen minutes. He then shampoos with the open hand the chest and limbs for a long time, following this by brisk friction. Similar manipulation is pursued at the back parts of the body, and especially at the nape, and over the muscular masses alongside the spine. A *séance* of this kind lasts about an hour, and is repeated for three or four days in succession. An amendment in the disordered contractions is observed after each; the child evidences its contentment, and calm sleep is restored. The following days, without entirely discontinuing the shampooing, the child is taught to execute very regular and perfectly rhythmical movements. Thus, suppose the arms are hanging in a state of supination by the side of the body, the operator takes hold of them by the wrist, bends the fore-arm upon the arm, carries the latter directly upward and forward, and then replaces the fore-arm in a state of extension. The hands are now raised in a parallel manner above the head, and from thence they are brought down to their point of departure, always following a well-marked ternary measure. This manœuvre is executed a great number of times, with much regularity. The inferior extremities are submitted in their turn to analogous movements: the leg is bent rapidly on the thigh, and this on the pelvis, when both are brought into extension, following a binary measure.

It is clear that the manipulations employed must impart remarkable activity to the capillary system of the skin and subjacent tissues, and through this to the intimate process of nutrition. The movements are so combined, that muscles whose motions are synergetical are brought into regular and simultaneous action. Unable to contract spontaneously and with regularity, they seem quite passive, so that the limbs may be bent or extended without the will of the patient contributing to the effect. Indeed, this generally opposes it, and it is only obtainable by employing a certain amount of force. But after one or two *séances*, the hand of the operator is enabled to follow the contractions which come to his aid with regularity. Every day the command of the will over the muscular system is strengthened, the abnormal movements at the same time diminishing in frequency and intensity. Not infrequently, during the first days, pains are excited in some of the joints by movements at all strong; but these, which some have considered of a rheumatic character, disappear after a few *séances.* After the employment of these passive movements for eight

or ten days, very marked improvement is observed, for the child can now speak intelligibly, feed itself, and, in some instances, walk about the ward. He now joins the gymnasium, and takes part in the exercises, under the surveillance of the master or a monitor-pupil. These exercises are graduated, and have in view the production of regular and easy physiological movements of the trunk and limbs—movements in which the will and the attention are called into play as much as are the physical powers. A great number of the manœuvres are performed in common, and during their execution the master and his pupils sing an air in two or three well-marked times, according as the exercises are performed in binary or ternary measure. The little patients, ranged in groups, are led away by the rhythm and by imitation. Other exercises are individual, and executed by each child according to its strength, all having for object the rousing the attention, and bringing the muscular contractions under the empire of the will.

The spirit of order and discipline exerts upon these children the most salutary influence. The attention, zeal, and great address of M. Laisné, aided by the means for ensuring safety in the gymnasium, have prevented any kind of accident occurring. During the first ten days, the children pursue the exercises with ardour. They are desirous of doing well, and their disposition seems to undergo a favourable change as they become more lively and open, and at the same time more docile. The organic functions are remarkably influenced from the first, the appetite becoming very keen, and requiring a proportionate supply of aliment; the muscular power increasing, and even already some increase of flesh being apparent. From the tenth and twelfth day, this amendment is subjected to some check; and we must now support the will and courage of the patient, and the more so, because the children endowed with most courage, determination, and docility, make the most rapid progress. After some days of this resistance, renewed improvement is observed, and we may be sure that the cure will now prove prompt and radical. Whatever the future may reveal, hitherto no relapse has been met with.

M. Blache enters into a comparison of the relative efficacy of gymnastics and sulphurous baths; and although employed alone, the former mode of treatment is to be preferred, yet the combination of the two modes is often desirable.

Besides the papers which we have noticed, the volume contains the following articles:—On the Culture of the Poppy in France, by M. Aubergier; On the Medicinal Properties of Saline Waters, by M. Carrière; and On Potable Waters, by M. Marchand.

Review VII.

1. *Das Medicinal Wesen des Preussischen Staates.* Dargestellt von LUDWIG VON RÖNNE, Kammergerichtsrathe, und HEINRICH SIMON, Stadtgerichtsrathe. Zwei Theile.—*Breslau*, 1844. pp. 786 und pp. 628.

The Medical Politics of Prussia. By L. VON RÖNNE and H. VON SIMON. Two Vols., 1844, and Supplement 1855.

2. *Dictionnaire d'Hygiène Publique et de Salubrité.* Par A. TARDIEU. Tomes III.—*Paris*, 1854. pp. 567, pp. 532, pp. 727.

Dictionary of Public Hygiène. By A. TARDIEU. 3 vols.

3. *Médecine Légale, Théorique et Pratique.* Par ALPH. DEVERGIE, Professeur, &c., Membre de Conseil de Salubrité, &c., &c. *Avec le Texte et l'Interprétation des Lois relative à la Médecine Légale.* Revus et Annotés par J. B. F. DEBAUSSY DE ROBECOURT, Conseiller à la Cour de Cassation, Chevallier, &c., &c. Troisième Édition. Tomes III. —*Paris*, 1852. pp. 743, 840, and 846.

The Theory and Practice of Forensic Medicine. By ALPH. DEVERGIE, Member of the Council of Health. *Containing the Laws bearing upon Forensic Medicine, with their Interpretation.* Revised and annotated by J. B. F. DEBAUSSY DE ROBECOURT. 3 vols.

4. *Code Médical, ou Recueils de Lois, Décrets, et Réglements sur l'Etude, l'Enseignement, et l'Exercice, de la Médecine Civile et Militaire en France.* Par AMÉDÉE AMETTE, Secrétaire de la Faculté de Médecine de Paris, &c.—*Paris*, 1855. pp. 470.

The Medical Code; a Collection of the Laws, Orders, and Regulations relating to the Study, Instruction, and Practice of Civil and Military Medicine in France. By AMÉDÉE AMETTE, Secretary of the Faculty of Medicine in Paris.

5. *Loi et Réglement sur l'Administration Générale de l'Assistance Publique à Paris.*—1849. pp. 7.

Law and Regulation regarding the General Administration of Public Succour in Paris.

6. *Manuel de la Cour d'Assises dans les Questions d'Empoisonnement.* Par M. JULES BARSE.—*Paris*, 1845. pp. 104.

Manual of the Court of Assizes regarding the Questions of Poisoning. By M. J. BARSE.

7. *On the Law of the Coroner; and on Medical Evidence in the preliminary Investigation of Criminal Cases in Scotland.* By JAMES CRAIG, Esq., F.R.C.S.E., &c., &c.—*Edinburgh*, 1855.

8. *Illustrations of Medical Evidence and Trial by Jury in Scotland.—Edinburgh*, 1855.

9. *Essays on State Medicine.* By HENRY WILDBORE RUMSEY. *London*, 1856. 8vo, pp. 424.

THE "Metropolis Local Management Act" must be regarded as one of the highest achievements of modern civilization. Indeed, it may be

doubted whether a more important measure has passed the British legislature within the two last centuries; for of all the objects of legislation, none can be of higher or more paramount importance than the protection of the public health, the diminution of mortality, the prevention of disease, the prolongation of life. Having for its more immediate and primary aim the sanitary regulations affecting the health of nearly three millions of persons collected together within the Metropolitan Districts, the ultimate operation of this Act must be the conferring of incalculable social benefits upon the inhabitants of every city, town, and hamlet in the kingdom.

The good that shall flow from this source will, however, nearly wholly depend upon the manner in which its provisions are carried out. Among these the most essential is the institution of medical officers of health. It is not without reason, therefore, that the qualifications and duties of this office have occupied, and will continue to occupy, the anxious attention of all who have at heart the usefulness, the honour, and the dignity of the profession of medicine, thus entrusted to a few of its members, whose noble aim it should be to lead, not to follow the legislature, in matters sanitary.

Influenced by these considerations, we have thought that a small space of this Journal might not be unprofitably occupied by a comparison of the sanitary and medico-legal functions of the English officer of health, as compared with those of the German *physicus* and the French *expert*. To this end we have extracted, from the works above-named, the principal features of the sanitary and medico-legal arrangements now in force in France and Germany.

Germany.—A complete system of medical organization exists in Austria, Prussia, and the other German states. The principles of this system are the same throughout; modifications in details are, however, met with, as may be supposed, in different parts of so extensive a range of Europe as that over which the German language is spoken.

A Supreme Medical and Sanitary Council or College exists in the capital of each kingdom or state, forming part of the office of Ministry for the Interior, and is presided over by the Minister of Public Instruction. This central council, at the seat of government, superintends all medical affairs, and has the supervision of all the provincial and district medical colleges or sanitary boards. To take an example, we may state that the Supreme Medical Council of Berlin consists of certain members, appointed for three years, and eligible for reappointment. Of these, the majority are medical men, the following nine well-known names being those of the medical members of the Supreme College of Medical and Sanitary Affairs in Berlin: Klug, Kōnen, Horn, Link, Kluge, Wagner, Mitscherlich, Casper, and Froriep.

In the principal city or town of each province there is established a provincial medical college, consisting of a president (the governor or principal councillor of state), two physicians, one surgeon, one accoucheur, one apothecary, and one veterinarian. This provincial council has to forward periodical reports to the supreme college, and is empowered to require the aid and co-operation of the councillors of state and the police authorities, in carrying out its objects.

In every city, also, having more than five thousand inhabitants, there is, in addition to the provincial council, a special sanitary commission.

The efficient instrument, the right hand of these councils, without whose active and skilled co-operation their sanitary regulations would be so many dead letters, is the medical officer—the *physicus*, who, according as he resides in a capital or a city, or in a rural district, is entitled *stadt-physicus*, or *kreis-physicus*. The qualifications, duties, and obligations of this official, we propose here to lay before our readers.

The physicus is subordinate to his own medical council, and is amenable to the jurisdiction of the Minister of the Interior. He is charged with the execution of all the laws regarding the public health, and medical affairs in general. He is expected to follow the advances of science, that he may be enabled to make them bear upon questions relating to the public health. It is the more incumbent upon him to excel in acquirements and accomplishments, inasmuch as, having the precedence of all other medical men, he should be able to gain their respect and esteem, both by his personal conduct towards them, and by his counsel and assistance in promoting the common welfare.

The general qualifications required for the appointment of the physicus, are good moral character, and the profession of the Christian religion; no Jew being allowed to receive the appointment. The special qualifications of a candidate for this office are attested by the Supreme Medical College, before whom he has to undergo an oral and a written examination in the principles and practice of medicine, distinct from the examination *ad licentiam practicandi*, and to whom he is required to submit a thesis upon some medico-legal question.

The appointment is made by the Government, and is accepted under the obligation of an oath of loyalty to the Crown, and of faithfulness in the discharge of the duties of the office, which confers a certain rank and title, that of "Councillor of Health," equivalent to that of "Councillor of Justice" (*Gerichts-rath*).

The remuneration of the physicus is very much below what has been given to the English officers of health—averaging only about thirty pounds per annum, while the obligation and duties arising out of the appointment are extensive and onerous. The *stadt-physicus* or *kreis-physicus* is required to reside within his specified district or city, and, so far as practicable, to be within call on all occasions on which his services may be required. He may not absent himself without the permission of the local authorities, and is then bound to find a competent substitute. The physicus is exempt from serving on juries. He is required to wear a particular uniform when appearing in the performance of his public duties. The physicus has the superintendence or oversight of all other physicians, surgeons, apothecaries, midwives, barber-surgeons, and other medical persons resident within his city or district; it is his duty to see that they perform their duties efficiently, or restrict themselves within their several limits, as prescribed by law. He is required to report to the Medical College the fact of any unqualified person undertaking the practice of medicine or surgery. It is the duty of the physicus to see that the medicines of the apothecary are pure, for which purpose he is required to make special visitations and inspection. He must report

to the police authorities, periodically, any alterations in the prices of drugs. He has to take care that the business of the apothecary be not interfered with by medical men dispensing their own medicines; and on the other hand, he has authority to protect medical practitioners from encroachments by the apothecary. In general, however, the limits of practice are so distinctly observed, that little occasion occurs for the exercise of this authority.

An acquaintance with prevailing diseases, whether in man or in animals, is expected of the physicus. To gain this, he is empowered to require reports of cases attended by other practitioners. It may be noticed that this forms one of the immediate advantages of the appointment of officers of health in the British metropolis, other practitioners having in most of its parishes received instructions to give notice to the officer of health, of the existence of epidemic disease, or of any other circumstances affecting the public health. With the same view, the physicus is directed to make himself familiar with the occupations and habits of life of the residents in his city or district, as they may affect their health; he must also inform himself upon the topographical and meteorological features of his district; he must on all occasions be ready to give his advice to the authorities in adopting measures for the prevention of the spread of disease. With the same object, the kreisphysicus must furnish to the Provincial Medical College periodical reports upon the state of the public health in his district, giving a full account of the prevailing diseases, their probable causes, &c. He may demand the aid or consultation of other civil or military medical officers when in doubt upon any question affecting the public welfare, or when from spread of disease the cases are more numerous than he can himself attend to.

The physicus cannot refuse his medical services to any one who may require them. He is permitted to receive remuneration from the rich; for his attendance upon the poor he has a claim upon the public purse He has medical care of all paupers, prisoners, or soldiers, not under the special charge of any other medical officer. Our own health officers have not this onerous charge.

Throughout Germany, the physici are the special medico-legal officers, to whom is confided the investigation of all medico-forensic matters. As already stated, each country is divided into districts, having severally a physicus, or "*Gerichts-Arzt,*" with his associate surgical officer (*der gerichtliche Wundarzt*). These officers are paid by the Government. The respective tribunals or judges may require their services in all inquiries touching sudden or violent deaths. A legal dissection is made by them, in the presence of the judge, and a written statement of what is then found in the body is dictated by the physicus. In all deaths from poisoning, the œsophagus, stomach, and duodenum having been carefully tied, are removed, and separately examined by the physicus in conjunction with an apothecary.* A report of the result is made to the tribunal. The "physicus" is required to investigate all cases of rape, simulated disease, or mental diseases, and to report thereon to the judge.

* The German apothecary corresponds to our pharmaceutical chemist; he passes a rigorous examination, and is debarred from medical practice.—ED.

If the case be very complicated, or if doubts still remain, the tribunal send the report of the medical officer to a provincial college, together with all other depositions. These colleges are required to report thereon to the tribunal. If still any doubt or difficulty should present itself to the judge or tribunal, there is yet a higher authority to which application can be made,—viz., the "Superior Medical College," the central authority for the whole kingdom, having its seat in the metropolis. To this highest authority the reports of the medical officers, and of the provincial colleges, are referred for the purpose of obtaining a "*super arbitrium.*"

Within the last few years, the proceedings are carried on publicly and before juries, in all important criminal trials. The "physicus" is required to appear before the jury, and *vivâ voce* to explain his report and opinion. The accused may also call other medical opinion. "Not seldom, an opposition between the public and the private medical men takes place, the public being present; which I cannot find very advantageous for the dignity of our state and science;" observes the celebrated Casper, of Berlin, in a communication with which he has favoured the writer. Dr. Casper has been thirty years a member of the "Superior College" at Berlin, and upwards of fifteen years "Stadt-physicus" of Berlin.

The forensic duties of the *physicus* are under the direction of the supreme judicial courts and of the police authorities of the district, or of a local magistrate. With the assistant forensic surgeon, the physicus, in the event of sudden or violent death, is required to repair, without loss of time, to the spot where the body is to be examined. The judicial inspection is required to be made according to special instructions issued to that end. In cases of poisoning or adulteration of food, the physicus shall very carefully and scrupulously investigate the case with the assistance of a qualified apothecary. A conjoint report shall be signed by these three officers—viz., the physicus, the surgeon, and the apothecary—not only to verify the truth thereof, but also to divide the responsibility of the consequences that may thence follow.

When his presence at a legal dissection is required by a magistrate, the physicus is ordered to do so with all possible expedition, and shall see that the forensic surgeon be provided with the requisite instruments in proper condition for use. If the body to be examined be that of a still-born infant, all outward appearances from head to foot must be recorded; the degree of its development; the state of the tongue as to protrusion from the mouth; the condition of the latter, whether containing mucus or any foreign substances; the state of the navel string, whether tied, cut, or torn. In examining a body, the state of the heart and large vessels, whether full or empty, as well as the exact condition of the larynx, lungs, &c., should be accurately noted. The organs in all the cavities should also be accurately observed. All wounds of the internal organ should be closely compared with those discovered externally, in order to determine whether the latter have been mortal.

In large cities or towns, there are provided also, for the especial purpose of facilitating the ends of justice, one or more police *physici*, whose functions are in some measure indicated by the epithet, and will further appear in our subsequent remarks. The distinction is, however, one

rather arising out of the practical requirements of cities, than one established by law. The police physicus is more directly connected with the police department, and is under the jurisdiction of the President of Police, to whom he must communicate all matters relative to the public health that may come under his notice. The police physicus is invested with authority to make inquiries, in the discharge of his duties, without the attendance of a police officer; in so doing he is, however, required to appear in a police uniform. If any case of illness should come under his notice, having originated in a quarrel or fight, the police physicus shall take notice particularly whether life is endangered by the injuries that have ensued; in which case he is expected to give information thereof to a magistrate. If it should come to his knowledge that any individual has died suddenly, without previous illness, and there be any ground to suspect that death has not arisen from natural causes, it is the duty of the police physicus to investigate the case, and ascertain whether there be sufficient reason to demand a magistrate's order for dissection. Any traces or indications of poisoning must be carefully looked for by him. The carcases of animals that have died of epidemic disease, or from any prevalent or similar cause, shall be opened before the police physicus, who shall make a written statement of the internal appearances, particularly of the stomach, noting at the same time what medicinal or therapeutic measures have been adopted during the life of the animal.

If in the course of his dissections the police physicus shall meet with any unusual specimen in natural history, or any monstrosity, he shall transmit the same to an Academy of Sciences, or to a Professor of Anatomy.

The police physicus must prepare for the President of Police, a quarterly report of all the judicial investigations and dissections performed by him during the quarter. In the discharge of his duty of examining the bodies of still-born children, he has opportunity of controlling the practice of midwives, and noticing any neglect of duty or transgression of the legitimate limits of their practice.

The police physicus has the superintendence of all the insane, of all prisoners, the surgeons of jails, to see that these latter act humanely towards prisoners, in discharge of his duty. He has the particular supervision of, and control over the forensic surgeon, and is required to ascertain that the latter performs his duty, in taking such measures for the prevention of syphilis as are indicated by science. For this purpose he is to call for monthly oral and written reports of all such cases, and from time to time to make a personal inspection of the manner in which this duty is performed. By these means he will become acquainted with the degree to which venereal poison is spread; he shall make experiments thereon, and shall embody the results of these reports and observations in his quarterly reports; wherein, also, he has to state the number of prostitutes, either in or out of brothels, together with the number of cures of venereal disease.

The objects of sanitary police which become the immediate duty of the medical colleges and the physici, are all those measures which may be necessary for the removal or suppression of the causes of disease, either as affecting individuals, or as spreading through a community. For this

purpose, the physicus has authority, in the case of infectious maladies, to enforce the separation of the sick from the healthy. The inspection of dwelling-houses, as to their ventilation, &c., also forms part of their duty. The sanitary regulations are directed to the prevention of hereditary disease, by the prohibition of unequal or premature marriages, and marriage between near relations; by the care of infants, and by the training of youth. The Prussian laws in these last matters are far more arbitrary than we in England should deem consistent with the liberty of the subject. The laws regarding pregnancy, abortion, and infanticide are among the subjects that come under the consideration of the sanitary police, as are also the examination of articles of food, the inspection of slaughter-houses, breweries, &c.

The police physicus is required to take cognisance of all poisonous substances employed in arts, trades, confectionary, &c., or in the manufacture of earthenware utensils. The regulations respecting interment are under the control of the sanitary police; as are also the dead-houses.

Vaccination comes within the province of the police physicus, who must superintend its performance by other medical men, and use every means to remove prejudices or other obstacles to its extension.

Upon these and all other matters coming within the scope of his public functions, the physicus must present to the Medical College a quarterly report, besides periodical and special reports to the police authorities when called for. These reports must record the occurrence of epidemic disease, with suggestions on the means of their prevention. Instances of malpraxis must also be reported.

The preceding summary includes, we believe, all the most important or most essential features in the functions, duties, and responsibilities of the German physicus, and the legal enactments respecting medical, sanitary, and medico-legal affairs in the German States. We proceed, in the next place, to lay before our readers a sketch of the same subjects as at present existing in the French empire.

France.—We are accustomed in England to hear and speak of the French "*experts*" as of a distinct class of medical men: as such, however, they do not exist in any formal or legal sense; although, practically, *experts*, distinguished as such from their medical brethren, are to be found in nearly all large towns or cities. The great care and attention bestowed by the State upon the public health has created a demand for the special skill and attainments required to cope with questions of hygiène and medical jurisprudence: the demand has been very fully met. The assumption, however, of the character and functions of an *expert* is entirely voluntary; but having been undertaken, their non-performance incurs penalties. Although in practice the medico-legal relations of the profession in France have thus come to assume an apparently positive form, these are by no means so certainly defined as are the duties and obligations of medical men in general, with reference to matters of public hygiène.

The attention that has been bestowed upon the care of the public health in France may be inferred from the number and high character of the works that have emanated from that country upon hygiène. The following remarks, from a leading article in the 'Medical Times and Gazette'

(Sept. 8, 1855), having reference to Dr. Waller Lewis's valuable Report upon Unhealthy Trades in Paris, very forcibly and correctly puts this matter before us:

"We learn from Dr. Lewis's Report that, at a period when France had barely recovered from the fury of the first Revolution—namely, on the 12th of February, 1806—the prefect of police prohibited the establishment in Paris of any workshop, manufactory, or laboratory, which could endanger health, without a statement of its nature being made to the prefecture, and a strict examination being instituted as to the mode in which it was to be conducted, and its probable influence upon the physical condition of the population. But as these regulations were imperfectly carried into execution, the Minister of the Interior consulted the Academy of Sciences on the measures necessary for the regulation of manufactures in regard to their effect on public health. Now, mark the persons chiefly concerned in reporting on sanitary measures—not lawyers, nor placemen, nor politicians, even at a time of intense political excitement—but men eminent in science—namely, Guyton Morveau, Chaptal, and George Cuvier; and upon *their* Report was based a decree, dated the 15th of October, 1810, and an ordonnance, dated the 14th of January, 1815, which two documents regulate all sanitary subjects up to the present day. Thus, while France was distracted by political excitement, while she was emerging from her first Revolution, while she was under the empire of the first Napoleon, and while she was struggling with the military forces of all Europe, she still was attending to the physical welfare of her people, and establishing laws for the preservation of public health, based upon scientific data. During the stormy periods of French history which have succeeded the memorable year 1815—during the reign of the Bourbons—a second Revolution—an Orleanist dynasty—a third Revolution—a second Republic—a second Empire—no attempt has been made to reverse the laws and ordonnances relating to the public health in France. The changeable population of that great country, fickle in almost all other respects, have yet learned to regard the sanitary condition of the nation as one of the chief objects of every form of government, whether imperial, monarchical, or democratic, and have cheerfully submitted to the necessary and wholesome restraints imposed by hygiènic laws and enforced by executive authority.

"All dangerous, unhealthy, or inconvenient establishments in France are divided into three classes, and Councils of Health are appointed in different localities to regulate and control the formation and the operations of such establishments. The establishments of the *first* class are those which must be kept at a distance from private habitations, and which require for their legalization the authority of the Government: those of the *second* class are such as do not rigorously require to be kept at a distance from habitations, but are compelled to give an assurance that the operations proposed to be carried on in them are executed in such a manner *as not to be a nuisance to the neighbourhood, and not to cause damage;* those of the *third* class are such as may remain without inconvenience near dwellings, but are subject to the *surveillance* of the police."

The existing organization of Councils of Hygiène and Public Salubrity, is based upon the decree of December, 1840, and of additional decrees, dating 1849 and 1851. In the chief city or town of every *arrondissement* in France a council of hygiène exists, and in every canton a committee of public health.

The Councils of Hygiène consist of not less than seven nor more than fifteen members, appointed for four years, one-half retiring every two years, but eligible for re-election. The members are medical men, agriculturalists, commercial men, proprietors, mayors, engineers, magistrates, and others who, by education and social position, are regarded as capable of judging of matters of hygiène. The medical elements of these councils are

distributed as follows:—In a council consisting of ten members, there will be four doctors of medicine or surgery, two *pharmaciens*, and one *vétérinaire;* in a council of twelve members, there will be five doctors, three *pharmaciens*, and one *vétérinaire;* in a council of fifteen members, there will be six doctors, four *pharmaciens*, and two *vétérinaires.* The advice and assistance of civil and military engineers, official architects, and of the chiefs of the police departments, may be called for if required by the councils, although they may not be members thereof.

The really local character of these councils of health is evident from the fact that, out of 1742 members thereof, 1544 are resident in the chief towns of the several *arrondissements* and departments, while the remaining 198 reside at greater or less distances within the department or *arrondissement*, and include the most important and most distinguished of their inhabitants.

The proceedings of the several councils of the *arrondissements* are subjected to the consideration of the councils for the departments, whence they are annually transmitted, through the Central Council of Hygiène in Paris, to the Minister of Commerce.

Paris has its own special arrangements relative to public hygiène, known as the Council of Hygiène and Salubrity of the Department of the Seine. In each of its *arrondissements*, a commission of nine members, presided over by the mayor of the *arrondissements*, in the city, and by the sub-prefect, in the suburban districts. Besides certain of the principal inhabitants, there shall always be, in each commission, at least two physicians, a *pharmacien*, a *vétérinaire*, an architect, and an engineer. These members are nominated by the prefect of police, from a list of candidates prepared by the mayor or sub-prefect of each *arrondissement* or rural district. The members are elected for six years, one-third going out every two years, the retiring members being eligible to re-election.

These councils and commissions meet not less frequently than once a month, and more frequently if the public service require it. They shall point out to the prefect of police all causes of insalubrity existing in their districts, and shall give their advice on the means of their removal; and may be required to give their advice also to the departmental councils. They may be called upon to execute extraordinary measures for the suppression of epidemic disease.

Among the duties of the councils of hygiène are, cleansing of localities and habitations; the adoption of measures to prevent the spread of epidemic and infectious maladies; the extension of vaccination; the organization and supply of medical assistance to the poor; the means of improving the sanitary condition of industrial and agricultural populations; the salubrity of factories, schools, hospitals, asylums, barracks, prisons, &c.; questions relating to foundlings; the quality of food; the improvement of public mineral waters, and the rendering these available to the poor; the removal or suppression of dangerous or insalubrious establishments, or nuisances; the supervision of public works, such as the construction of prisons, schools, canals, reservoirs, fountains, cemeteries, sewerage, &c., &c.

These councils shall also collect the statistics of mortality and its causes, together with the topography of each *arrondissement;* and shall

regularly transmit all such documents to the prefect, who shall forward them to the Minister of Commerce.

A central council of hygiène and public health, at the seat of Government, presides over all the other councils, and over medical affairs in general, and is charged with the examination of all questions on hygiène referred by these, or put before them by the Minister of Commerce and Agriculture. The members, seven in number, are nominated by the same functionary; they consist of four doctors of medicine, a civil engineer, an architect, and a secretary having a consultative voice. They may require also the attendance of one member respectively of the Military and Marine Councils of Hygiène, of the perpetual secretary of the Academy of Medicine, and of certain public functionaries—e.g., the chief of the sanitary police department, the architect, the chief of the post-office packet department, of the administration of tolls, &c., &c.

The *Criminal Code* (Art. 44) directs that in the event of a violent death, or of one to the cause of which suspicion may attach, the procureur shall call in the aid of an *officier de santé*,* who shall submit to him a report upon the condition of the body, and the cause of death. In the *Civil Code* it is directed that when suspicion exists of violent death, interment shall not take place until a police officer, assisted by a doctor of medicine or surgery, shall have prepared a *procès verbal* as to the state of the body, and other circumstances, such as the name, age, residence, &c. of the deceased. The choice of the medical officer is left to the magistrate, who, although the matter is of equal weight in either case, may call upon a physician, being an *expert*, or upon an *officier de santé*, who in the medical hierarchy has no rank, or only the lowest. The education of the *officier de santé* is inferior to that of the physician or surgeon, his functions are restricted, surgical operations not being performed by him. The *officier de santé* seems, in fact, to occupy a position in some respects similar to our now obsolete "apothecaries," but the former does not practise pharmacy.

It must be supposed that the framers of the above-cited clauses of the criminal and civil codes regarded the mere skill in making a technically expressed report as being of higher value than the scientific qualifications of the individual to whom an important public duty was to be assigned by the magistrate. Or, they may have considered that a mere *officier de santé*, on the spot, in the communes or rural districts, would be more suitable for these investigations than a physician residing at a greater distance. The result, however, is that the opinion of an *expert* is frequently required in a subsequent stage of proceedings; Article 43 of the Criminal Code giving the procureur the power to summon the assistance of whomsoever he may deem the most skilled in his profession.

The official reports of the *expert* must contain all the information which his experience shall enable him to suggest relative to the presumed intention or premeditation of an alleged crime, so far as inferences may be drawn from the appearances of the body, of wounds, or of the characters of weapons found.

* In the words of M. Devergie: "L'expression, *officier de santé*, qualifie un homme apte à donner des soins en cas de maladie, et pas autre chose. On n'y entend pas un grade, un rang dans l'hierarchie médicale." (Tom. i. p. 4.)

An autopsy is performed upon the authority of the procureur or his deputy. Exhumations are ordered only in extreme cases. The autopsy is to be performed without delay, and the authorities are required to see that the investigation is closely conducted, and that traces of crime are not thereby obliterated.

The reports of *experts* are of three kinds—viz., judicial, administrative, and estimative. The *judicial* or *official* have for their object the elucidation or discovery of an alleged crime. The *administrative* have reference to questions touching public health. The *estimative* refer to disputed remuneration. Besides these reports, the *expert* is frequently called upon to give a simple certificate or statement of a fact, not in behalf of justice, or attested by an oath, but for inaccuracy of which he is, nevertheless, amenable to punishment.

We may represent by an imaginary case the mode of proceedings and position of the *expert* in France.

Supposing that a man is found dead in a room, the police requires the attendance of a doctor, or *officier de santé*, to attest the death, and to state the probable cause of death. Should any wounds or other indications of violence be apparent, these must be noted; and simply confining himself to the facts before him, the medical man must, in a *procès-verbal*, state his suspicions, and indicate whether or not these require that the body be opened.

By a police ordinance of 1801, every medical man is required immediately to report to the police the particulars of every violent or accidental death to which he may have been summoned.

This primary report is forwarded by the police to the *procureur*, who, if he consider the suspicions of a crime to be sufficiently strong to call for further proceedings, appoints a *juge d'instruction*, who then nominates two physicians to inspect the body in the presence of either himself or his deputy. These physicians draw up an official report of what they observe, with their interpretation of the facts, and the conclusions thence to be drawn.

These two reports may, however, fail to explain with certainty the cause of death, or they may raise difficulties not previously contemplated. For the solution of these, the *juge d'instruction* shall charge two or more physicians with the duty of examining and advising upon the preceding reports; at the same time he shall submit to their consideration all other documents that may tend to throw light upon the inquiry. All these are digested and discussed in a *medico-legal consultation*, in which the last *experts* examine, in all their bearings, the facts and conclusions drawn by previous reporters, either confirming or reversing these. This "consultation" is not the subject of a special law, but is governed by those which rule the production of the "reports;" the several *experts* being convened by the *procureur* or magistrate, in the regular form of summons for a report.

The medico-legal "consultations" may have two different sources—they may be demanded either by the accused, or by the judicial authorities. They are usually held before judgment is passed; but if the condemned have an opportunity of appeal, he may demand a "consultation" subsequently—sometimes with the effect of reversing the sentence. The

strictest impartiality is enjoined upon the *experts*, whether engaged by the defence or the accused, with the proviso that in case of doubt the benefit be given to the accused.

The experts thus called in "consultation" do not necessarily reside in the locality where the alleged crime was committed, but may, if advisable or necessary, be summoned from a distance. Or it may happen in more grave cases, such as poisoning, assassination, &c., that there may be a difference of opinion among the "experts" who have investigated the affair on the spot. Under these circumstances, the magistrate addresses to the local *juge d'instruction* a *commission rogatoire*, by which he is authorized to require the opinions of certain "experts," the choice of the latter being frequently left to his discretion. The limits of the "consultation" are much less restricted than are those of the "reports," which consist simply of a statement of facts and conclusions. In the consultation every fact must be discussed and fully commented upon, the commentary being strengthened by all suitable arguments, and illustrated by reference to the statements and opinions of authors. The names of the previous "reporters" are in all cases concealed from the consulting "experts," lest the authority or insignificance of a name should exert its undue influence upon their judgments. The several parts of the evidence are separately examined by each expert, previously to their joint consultation. The result of the consultation is delivered in four distinct parts:—1. The preamble, a simple enumeration of the points submitted for deliberation. 2. The exposition of facts, in which all the circumstances and events are set forth in their exact order. 3. The discussion of the facts, which is the most difficult portion of the duty of the experts, requiring much sagacity and discrimination, and demanding research, experiment, scrutiny of proofs, and the collection of facts, for the guidance of the magistrate or judge. 4. The conclusion, in which the results must be briefly and clearly stated, together with the grounds of difference (if existing) from the conclusion of previous reporters.

It is apparent that the "expert" must possess not only practical skill, but should have also an extensive and ready acquaintance with the recorded facts and opinions of medico-legal writers. Their reports constitute the ground of action determining the prosecution or abandonment of legal proceedings; and in the event of trial, they are in the position of witnesses, although, as observed by M. Devergie, they are there in a false position, since, as representatives of science, they should not be called upon to advocate any particular interest. At the tribunals, the "experts" are required to depose to all that they have observed, and recorded in their reports; they have, moreover, to respond to questions put either by the judge, the jury, or the procureur. Their replies may give rise to further explanation, and the demand for additional evidence, and occasionally lead to controversy and discussion in the court between experts on the side of the prosecution and of the defence. To this M. Devergie very justly objects, and urges that the duty of the expert should be confined to the statement and the interpretation of facts and their legitimate conclusions, irrespective of any civil or criminal questions.

In order to meet these objections, M. Devergie suggests that there should be three grades of public or official experts, liable to be called upon

by the judges, mayors, justices of the peace, prefects, and sub-prefects: the first to be attached to the *Cour d'Appel;* the second, to the tribunals of each *arrondissements;* the third, to the local courts of the *cantons.*

It has also been proposed by M. Barse, that a college of experts should be established, to which reference should be made in all difficult cases, and in which institution he considers that society would have all the guarantees it could require for the unbiassed and exact application of science to all medico-legal questions, while experts themselves would acquire increased confidence in their conclusions, from the weight and dignity with which they would be invested as the reports of the college. M. Barse proposes that the institution be divided into two sections, chemical and medical; directed by president, vice-president, secretary, &c., chosen from its own body. Every investigation to be submitted to not less than three members of this college. The proceedings of the college to be published at regular intervals; the council having authority also to publish original articles by any members of the college.

The "reports" which have been mentioned as "administrative reports," are those which relate especially to matters affecting the public health. They call for as much care and exactness as is demanded for the preparation of criminal reports, inasmuch as the comfort or even the existence of many individuals or of a neighbourhood may be involved therein. The duty, obviously, should not be undertaken by those who do not possess the requisite knowledge of chemistry and manufactures. In large towns, these functions are performed by the Councils of Hygiène and Salubrity.

The duties and qualifications of the English "Officers of Health" are now generally known, and have been fully stated in this Journal (April). They are of no light character; they are not restricted to any narrow or special field of sanitary quackery; but will demand a practical knowledge of medicine, and something more than a superficial acquaintance with collateral sciences. Sir B. Hall has well summed up these in the following remark to a deputation that waited upon him to learn his views on this subject:

> "He desired the appointment of men of such high position and acknowledged qualification that, in case of a return of epidemic, they might meet as a general medical council for the whole metropolis, and draw out a system of sanitary regulations which, bearing the authority of their names, would be universally respected."

The combined weight of the experience and attainments of the Officers of Health would not only, in the times of danger referred to by Sir B. Hall, but at all times, constitute such a general medical council as shall be "universally respected." From the close connexion of this council with the central council of vestrymen, the necessity for other non-medical Boards of Health would cease. All their functions would be absorbed by the more efficient medical council.

The Boards of Health that have existed hitherto have been proved to have been powerless for the removal of causes of ill-health; the law was indefinite, and the determination of nuisances prejudicial to public health depended upon the views of persons incompetent to form conclusions thereon, while decisions could be reversed by appeals to higher courts of judicature. The whole of our sanitary legislature has been a tissue of uncertainty and doubt. The new Metropolis Local Management

Act removes much of the complicated machinery that stood in the way of the application of remedy, and by the formation of a corps of scientific and trained health officers, has paved the way for the attainment of certainty, and has given confidence in the beneficial operation of our sanitary regulations.

An association comprising all officers of health, and others interested in the advancement of sanitary and medico-legal science, would doubtless prove a powerful means to this end. We have now all the elements for he formation of a British society of experts, analogous to the college proposed by M. Barse. Experience is yet wanting to most of the newly-appointed officers of health, but as this is accumulated, if it be enlarged and corrected by comparison and discussion, the result must be that greater precision will rapidly be attained, and the public proportionately inspired with confidence in the opinions of those to whom they have entrusted those hygiènic and medical affairs which alone can be safely confided to professional hands. It may be hoped that an association of this nature will ere long be in course of formation.*

Thus, besides the duties immediately of a sanitary nature, the medical officers of health will eventually be looked up to as the most trustworthy aids to the coroner in the prosecution of medico-legal inquiries. Such assistance is absolutely needed in most law courts, as well as the coroners' court. The irregularities and oversights now too frequently occurring before the coroner's tribunal would, under such circumstances, be much less likely to occur. The progress that would be made in the diffusion and improvement of medico-legal science, by the greater certainty and facility that would be afforded for the detection of crime, would have the effect of deterring from its perpetration. It may seem superfluous further to allude to the need actually existing for improvement of the coroner's court in England. But a still more lamentable want of a medical jurist is to be found in Scotland, as Mr. Craig shows in the pamphlet named at the head of this article. The coroner's court does not now, although it did anciently, exist in Scotland. The following is the practice of inquests in that portion of the United Kingdom, as stated by Mr. Craig; it is very different to the practice in England or Ireland, where direct application to the coroner may at once obtain an inquest, if there be ground of suspicion:

"1st. In all cases of sudden death, the district constable repairs to the place where it has occurred, collects information, and sends off a report immediately to the superintendent; and, in cases of rape, child murder, or concealment of pregnancy, the *constable* is to ascertain, with precision, all appearances exhibited, such as marks of feet, blood, &c. &c. If there be any circumstance calculated to raise ground of suspicion as to the death, such as external marks of violence, bruises, fractures, &c., the constable is to apply to the nearest medical man without delay, and, after an examination, is to obtain a certificate, and forward it immediately to the superintendent. In all cases of serious assault, and where death is likely to occur, the constable, without delay, procures the assistance of the nearest medical man, and sends off a report, as before described; and instructions are given as to what circumstances the medical man is to certify. Upon receiving such a report, it is laid by the superintendent before the procurator-fiscal of the county, who either acts upon his own responsibility, or occasionally

* The metropolitan medical officers of health have recently formed themselves into an association.

takes a fresh precognition, and prepares a case to submit to the crown-agent, to whom the police reports are also frequently sent, and whose instructions are thereafter acted upon."

The "Procurators-Fiscal" are legal officers appointed by the Government to each county, their duties being to inquire into alleged crimes, to receive the reports of the police, and to determine whether prosecution shall be undertaken. In the event of a trial, the medical attendant of the person to whom violence or accident has occurred is required to give evidence and assist the court by his opinions. Should the condition of the person so injured be supposed to be such as shall endanger life, the procurator-fiscal may require that the police medical officer shall visit and examine into the state of the health of the person, in order that he may report whether he is in a fit state to "emit a declaration," or make a statement of the circumstances attendant on the accident or violence, to the sheriff, in order that important evidence may not be lost by the death of the injured.

Some change is evidently demanded where the initiation of an inquiry involving questions of life or death is dependent upon the caprice or conceit of a parish constable. No stronger proof could be afforded of the importance of medical knowledge in the institution of inquiries touching the causes of death, than is afforded by its total absence in this instance beyond the Tweed. So protective to criminality is the existing order of things, that Mr. Craig, in his very striking pamphlet, informs us that it was a matter of discussion among the servants of a family whether it was better to have a bastard child in the town or in the country; they came to the conclusion that a child is more easily disposed of in towns. Surely it is high time that the practice of the English laws of coroner, registration, and medical officers of health, should be extended northward. Mr. Craig relates an instance also of the deaths of both child and mother after the obstetric administration of chloroform, and interment without inquiry. While we congratulate ourselves that such occurrences can scarcely take place in England, we regret not only the impunity it offers to crime or rashness in Scotland, but we also regret the confusion it necessarily introduces into the statistics of the results of any novel or hazardous line of practice.

The appointment of a Public Prosecutor has repeatedly been spoken of, and the proposition had so far assumed a definite form, that early in 1854, a bill was introduced into the House of Commons by Mr. Phillimore, for the express purpose of creating public officers under this name. The bill meanwhile was withdrawn, upon assurance given by the Attorney-General that he had been requested by the Government to prepare a measure having the same object. This bill, however, so far as we are aware, has not yet been brought under the notice of the legislature.* Its principal features were such as to promise much improvement upon the present mode of proceeding. The bill proposed to divide the country into districts, with a public prosecutor for each, whose functions would resemble those of the *Procureur Impérial* in France.

That the introduction of this functionary into our system of criminal

* While writing these observations, the Report of the Select Committee of the House of Commons, recommending the appointment of public prosecutors, is published. 'The Times,' May 29th.

jurisprudence would be in the highest degree advantageous, none but those who have a personal interest in existing arrangements can doubt. Aided by the counsel of officers of health, or by those eminent medical jurists which it is the honour of Great Britain to possess, the jurisdiction of civil and criminal courts would cease to furnish so many examples of prosecution carelessly conducted, evidence destroyed or overlooked, and guilt escaping.

From the preceding remarks, it will be seen that, in many of their most essential and most useful features, the new officers of health approximate to the German *physici*. The French system of the administration of hygiènic affairs, resembles the functions and powers of our English boards of vestrymen under the new Metropolitan Local Management Act. The extension of the principles of this legislation to other towns will complete the resemblance, and extend the operations of so beneficial a law.

As it is among the German *physici*, and among the medical members of the French Councils of Hygiène, that forensic medical science is sought and found, so it must eventually come to pass that the medical sanitary officers of England will constitute the body in which medico-legal science will be most assiduously and most successfully cultivated. The sanitary and the forensic duties of officers of health are closely associated—the qualifications which fit them for the performance of the one especially adapt them to the requirements of the other. As, by the new act, the British legislature is expressing a just appreciation of the scientific attainments of the medical profession, and regarding its members as the only trustworthy advisers in all questions affecting public health, it must of necessity follow that public opinion will concede the highest respect to the opinions given in courts of justice, by an experienced body of scientific *experts*, upon all medical questions involved in criminal or civil jurisprudence.

We regret very much that the late period at which we received Mr. Rumsey's work has prevented our incorporating with the preceding observations our abstract of its contents. We are at present limited to a brief analysis of the several essays. Doubtless, frequent future occasions will occur to induce us to bring this work under the notice of our readers.

Mr. Rumsey, in common with all who have bestowed any attention upon the subject, is struck with the entire absence of design exhibited in the sanitary legislation that has hitherto been effected in this country. So desultory and unconnected have been the labours of our countrymen in all that relates to State medicine—so systematic has been the study of "*hygiène publique*," or of medical polity, "*medicinal polizei*,"* among our continental neighbours, that British writers and sanitary officers must consult German or French authors if they would devise a comprehensive scheme of State medicine. · So true is this, that even Mr. Rumsey's essays throughout have the German type very distinctly stamped thereon, although he has not been entirely led by French or German systems. The arrangements of detail are necessarily influenced by the existing legislation of this kingdom, imperfect as it is.

In his Introductory Essay, Mr. Rumsey observes:

* Medical police has in Germany the more extended signification of medical *polity*, rather than of *police*, as generally received among us.

"Had it been my intention, in the following pages, to treat methodically of all the various matters pertaining to the care of the public health, for which either the enactment of special laws, or the delegation of discretionary power to constitutional authorities, has been found necessary in this and other countries, I should, both as regards matter and arrangement, have drawn more or less from some of the most approved treatises, chiefly German and French, which have been published within the last eighty years on the Continent; for, to say the truth, this subject has never been systematically written upon in England.

"Or, had I wished to describe in detail the most successful methods of effecting a few sanitary objects, special in kind and limited in application, I should have examined the measures now in progress in certain cities and towns, at home and abroad; I should have compared these, not merely with some of the public works and municipal regulations of ancient times, but also with those other more appropriate and practicable projects, suggested in many pamphlets and reports on sanitary affairs, which have deluged this country during the last twenty years." (pp. 3, 4.)

The author's design is to offer a scheme of *agenda* in State medicine, —suggestions as to what he deems advisable in the relations of the science of medicine to the State executive. These he comprises in three great divisions:

"I. In relation to subjects concerning which the State should direct INVESTIGATION.

"Whether these be—

A. Statistical.
B. Topographical.
C. Jurisprudential.

"II. In relation to PRACTICAL ARRANGEMENTS for the personal safety and health of the people, requiring for their enforcement either direct or legislative enactments, or local institutions and regulations.

"These may be subdivided into—

A. Preventive, and
B. Palliative, measures.

"III. In relation to the establishment by law of an ORGANIZED MACHINERY for carrying into effect the aforesaid inquiries, for deliberation and advice on special arrangements and emergencies, and for the administration of existing laws.

"And this would comprehend—

"*A.* The education of medical men, and the qualification of other technical, scientific, and administrative agents.

"*B.* The institution of official authorities—board and offices—for central and local superintendence and action." (p. 6.)

Under the suggestions regarding statistical records, Mr. Rumsey enumerates all the data that can be of any public importance, and some which we think Englishmen would be prone to count as private. The regulations which the author would advise for the prevention of disease are most judicious, but have many of them a somewhat too paternal character; for instance, Mr. Rumsey would prohibit the erection of buildings in districts where permanently unhealthy influences are confined to a limited area; and also the extension of towns in the direction of sources of malaria. Such stringent laws would directly raise questions of the rights of property, and would enlist a host of opponents to hygiènic legislation. We doubt also whether legislative interference would, in this country, ever be permitted to that extent that the author thinks desirable in the construction of houses, streets, &c. &c.

As, however, the first Essay proposes only the outlines of a sanitary code, we need not too closely criticise every detail. Mr. Rumsey has clearly drawn his outline from the model of Continental systems, where Governments act for, and supersede the intelligence and free action of, the people. It is not to be supposed, however, that because we find laws promulgated they are always executed. There are many Continental towns enjoying the advantages of very wise sanitary and hygiènic laws, but where the want, as Mr. Rumsey observes, "of English capital and energy have prevented the application of engineering skill to the execution of great works of purification."

We must not lose sight, however, of the fact that many of the author's suggestions are the subjects of existing Acts of Parliament. We may instance the laws regulating the employment of children in factories.

In the succeeding Essays, Mr. Rumsey unfolds his scheme, and fully explains its details.

The second of these is occupied with the consideration of Education in the Healing and Health Preserving Arts. On this, however, as opening the whole question of "medical reform," we shall make but few remarks, the matter being now before Parliament, and receiving very ample discussion in other quarters. We observe that the author admires the autocratic form of government as much in this as in other departments of State medicine. The objections to State interference in medical questions are among the strongest arguments against particular measures of medical reform. We do not here enter upon the discussion, but we think most of our readers will, with us, demur to any governmental restriction of the admissions to the profession. This is unhesitatingly advised by Mr. Rumsey, in days when even all such close corporate restrictions in matters of trade have been found to have been wisely repealed. The author has altogether overlooked the fact that many of the greatest promoters of medical science have been those who, though members of the profession, have not been dependent upon daily practice. These men, therefore, if the supply were strictly limited by what a Government might suppose to be the demand, would be entirely excluded, or compelled to abandon the pursuit of the science for the drudgeries of practice. No greater mistake could assuredly be made than the introduction of State control of this kind.

In the third Essay we have an inquiry into the methods and defects of Sanitary Inquiry in England, and the directions in which it needs extension under State authority. The author first gives an historical notice of the opinions of Hippocrates, and Bacon's scheme, or 'Historia Vitæ et Mortis,' remarkable for "their singularly comprehensive character," and "their obvious connexion with present subjects and methods of research."

In commenting upon the modern statistical modes of inquiry, the author gives an instance of the misapplication of figures in an inquiry which took place a few years ago respecting workhouse dietaries, in which an altogether fallacious conclusion was arrived at by the omission of important data. We surmise, however, that some fallacy must lurk in the calculations upon which the author bases the conclusion (although he admits that the proof is not complete), "that the working classes, in

places where there are no gin palaces, notwithstanding their miserable dwellings and exposure to casualties, are longer lived than the affluent and luxurious." (p. 94.)

In this essay, Mr. Rumsey dwells upon defective sewerage, drainage, offensiveness of factories, defective structure of dwellings, intramural interments, and other allied topics, which have been so repeatedly brought under public notice during late years. The want of system in the collection and preservation of the information thus accumulated is pointed out by the author.

A just tribute is paid to the office of Registration of Births and Deaths, which by its energy, activity, precision, and civility, presents a striking contrast to every other board having to deal with public health, and with official boards in general. That there are still defects in the registration of births and deaths is not the fault of the registrar's department.

The total want of the means of ascertaining the amount of sickness prevailing during any period, is obvious to all; the construction of the machinery by which it shall be registered remains, and will, we fear, remain for some time longer, a *desideratum*. Until this shall be attained, our knowledge of industrial pathology, and of endemic and epidemic causes of disease, must be obscure.

The consideration of these topics leads to the fourth Essay, which treats of The Medical Care of the Poor in England, with notices relating to Ireland. This branch of his subject the author divides into two parts; first, the history of the rise and progress of Poor-Law medical relief; secondly, its present condition and requirements. These topics are too large for us to attempt to follow the discussion at this moment. We must be content to refer our readers to the author's account of these public institutions, and to an exposition of the Irish dispensary system published in a past number of this Journal (April, 1854). We must reiterate the wish there expressed, that a similar machinery for the State supervision of gratuitous medical relief could in this country be made to supersede our incongruous medley of infirmaries, dispensaries, and medical charities of various kinds, dependent upon uncertain or fluctuating voluntary contributions.

The fifth Essay, On Local Sanitary Administration, includes necessarily the consideration of the design and functions of officers of health. In discussing these, the author takes the opportunity to give an historical sketch of analogous appointments among the Romans and their European successors. The office of German *physicus* comes also under the author's notice, of which he remarks :—

"A succinct and intelligible description of the modern appointments of *kreis-* and *stadt physicus*, and, indeed, of the whole medical polity in the different states of Germany, is still a desideratum in the literature of Hygiène." (p. 301.)

We trust that in a former part of this article we have given a succinct and intelligible account of the *kreis-* and *stadt-physicus*, but we take leave to doubt whether the English reader would be interested in the "whole medical polity in the different German states." German literature of hygiène possesses a most full and elaborate account of all these matters in the work of Rönne and Von Simon, quoted in our preceding remarks.

Mr. Rumsey dwells upon the attempts that have been made before the passing of the recent act, to obtain the appointment of officers of health. The imperfect operations of preceding acts are also pointed out. A high standard of qualifications for the duties of officers of health, is very properly insisted upon by Mr. Rumsey, and we fully concur with the author in his opinion that the functions of this officer are quite incompatible with the obligations and engagements of private practice—at all events, of the general practitioner.

The author deprecates the committing the local administration of sanitary measures to boards of guardians; and quotes from published reports instances in which these functionaries have thwarted the efforts of health officers, and obstructed measures for the prevention of the spread of epidemic disease. The metropolitan officers of health are no longer under the control of parochial guardians, but of vestrymen; the representatives of the French and German local boards. It remains to be seen whether these councils will entertain clearer and higher notions upon questions affecting the public health.

The last of Mr. Rumsey's series of Essays embraces several departments of health police, in their relations with local sanitary administration. These several departments are as follow:—

"Registration of births and deaths; medical evidence in forensic inquiries; adulteration of food, drinks, and medicines; public vaccination; local organization of a civil medico-sanitary service; circuit inspection."

With regard to registration, Mr. Rumsey enforces the advantages that would result to science by the union of the registrar's with the duties of the sanitary officer.

The glaring defects of our coroners' inquests afford the author abundant reasons for urging the need of reform in our system of medico-legal researches. Mr. Rumsey also touches upon the question of the supposed superior qualifications of a medical man over a lawyer for the performance of the coroner's duties. This question we think, however, has been answered. It has been brought to the test of experiment by the coroner for the county of Middlesex, who obtained the appointment mainly by urging his medical claims, and who has himself appointed as his deputy, a member of the legal profession—his own son.

We have in a preceding page of these observations commented upon the imperfections of medico-legal investigation in this kingdom; we have shown the positive need of reform, and have urged that the appointment of a public prosecutor and medico-forensic experts to aid his investigation, offer the greatest amount of protection against the continuance of the present state of impunity to crime, ignorance, or carelessness.

The third department of which Mr. Rumsey speaks in his Essay, is that of adulterations of food, &c. The experiments and conclusions of Dr. Hassall are mentioned. Rigid inspection is recommended. Mr. Rumsey's observations upon the erroneous legislation which has thrown the preparation of the commonest articles of food into the hands of manufacturers, deserve notice. By removing restrictions upon the importation and distillation of spirits, the State has directly given rise to "gin palaces," the fountains of debauchery, disease, and death; while, at

the same time, by taxing malt, the practice of private brewing has been discouraged, and a wholesome, necessary beverage is exchanged for "liquid fire," that has branded disgrace and intemperance upon the lower classes, desolated their homes, degraded their wives and daughters. We entertain a hope that the remedy for these existing miseries will be so strongly urged by the future medical officers of health and British experts, that wiser counsels will prevail, and our legislators be led to retrace their steps. One condition, however, we hold to be essential to the usefulness of all efforts to extinguish fraudulent substitutions and adulterations of food—that is, that no exaggeration be admitted into the statements of adulteration; that due care be taken to distinguish between adulteration and preparation, or simply manufacturing processes. Such misrepresentations have been—doubtless, unintentionally—made; but caution should exclude them.

We come to the fourth topic, which the author here introduces to the notice of his readers—viz., vaccination. This is treated first historically, or an account is given of the establishment of the National Vaccine Institution, the only State provision for vaccination before the passing of Lord Ellenborough's Act of 1840.

Mr. Rumsey concurs in the objection taken by the Committee of the Epidemiological Society to this department of sanitary police being committed to the authorities for the control and relief of pauperism. It is obviously a matter relating to preservation of life—not of alms-giving.

Mr. Rumsey repeats the criticisms and imperfections of Lord Lyttleton's Act, as pointed out by the Epidemiological Society. These defects will, we hope, be in some degree remedied by the Amendment Act now before the legislature.

Mr. Rumsey objects to the three months' clause of the Vaccination Act, as advised by the committee of the Epidemiological Society. We hold, on the contrary, that this is a wise provision. The operation is thereby performed before the constitutional disturbances so commonly attendant on dentition have taken place; its course is therefore less liable to interruption. If the operation be delayed to the age of one year, it cannot be disputed that a large proportion of infants would be left open to the attacks of a specially formidable infantile disease. On the other hand, no valid objection is urged against the early period. Prejudices should not be admitted as arguments, else where would be vaccination at any age? We anxiously await the appearance of the second division of the Report on Small-pox and Vaccination, by the Epidemiological Society, which promises therein more strictly pathological data than were called for in a legislative measure, to which statistics were deemed the more essential.

Mr. Rumsey objects to the principle of compulsion—so do others; but the majority of those members of the profession who took the trouble to answer the queries of the Epidemiological Society, approved the principle as offering the only chance of efficient legislation; we have little faith in the march of intellect, after so lamentable an exhibition of ignorance as that which took place not long ago in the British House of Commons in connexion with this question.

Mr. Rumsey proceeds to discuss the several features of the two Bills

now before Parliament. We trust that the important department of Vaccination will not be separated from other sanitary regulations.

The next section, on Health Officers, does not call for observations beyond what have already been made in this and a preceding article.

The last section recommends the very requisite inspection of all public medical institutions and sanitary offices, by superior inspectors. With regard to the importance of this measure we apprehend there can be no difference of opinion; the difficulty will be in the selection of officers, and the definition of their functions.

Mr. Rumsey's volume concludes with "Supplementary Notes" to the several subjects in the body of his work. One of these requires notice. The scheme of providing nurses for the sick poor, from among the inmates of workhouses. Mr. Rumsey thus criticises the proposition:—

> "It is, however, to be regretted, that its humane and learned originator and promoters were not more practically acquainted with the class out of whom they propose to make nurses, and that, before promulgating a specific plan, they had not more extensively consulted the chaplains and medical officers of workhouses. Even by this time they may have learnt, that the training, habits, notions, and associations of female paupers, as a class (for there are exceptions, of course), are such as to render them most unfit for an employment in which the strictest decency, cleanliness, and morality, with some delicacy of feeling, are essential to the welfare of the patient." (p. 411.)

Mr. Rumsey has overlooked the first principles and most essential feature of the proposed scheme, consisting in the *training* and *educating* of the female inmates of workhouses for the duties of nursing, under the direct supervision of medical and other officers.

From some experience among the poor, we cannot concur in Mr. Rumsey's objections to the *class* of paupers. We entertain much stronger objection to the formation of institutions of "Nursing Sisters" or organized "Sisterhoods," as fraught with dangers not always obvious to the casual observer.

We would add, in conclusion, that Mr. Rumsey has done good service to the present sanitary movement, by bringing together in an accessible form a connected account of the dissociated and heterogeneous mass of legislation, and private efforts, in the direction of Hygiène and State Medicine. His work may be fairly said to represent the first chaotic stage incidental to all great popular movements; at the same time that it marks our arrival at a period of order, and the accomplishment of some of the designs, hitherto but vaguely conceived.

Review VIII.

On Calculous Disease and its Consequences. Being the Croonian Lectures for the year 1856, delivered before the Royal College of Physicians, by G. Owen Rees, M.D., F.R.S., &c., &c., Fellow of the College, Assistant-Physician and Lecturer at Guy's Hospital, Examiner on Materia Medica in the University of London.—*London*, 1856. pp. 81.

The profession are already familiar with some of the tenets advocated by Dr. Owen Rees, in the Croonian Lectures delivered before the College of Physicians during the present year. He has recently published, in the Guy's Hospital Reports,* a paper, in which he contests the opinion of Dr. Prout regarding the existence of a phosphatic diathesis, the symptoms indicating which he explains by the precipitation of earthy phosphates from acid urine by the *alkaline* fluid poured out by the inflamed urinary mucous membranes. This view is still further developed in the present lectures. Their chief object is to prove that the excessive formation of uric acid is the efficient cause of the great majority of all calculous disorders. Dr. Rees commences with the consideration of oxaluria, and adverting to the facility with which urates and uric acid are converted into oxalates, goes through a series of Dr. Golding Bird's cases, in order to prove that the so-called oxalic acid diathesis is nothing more "than an accidental and unimportant modification of the uric." That a conversion of uric acid into oxalic acid may and does take place after the urine has been discharged, cannot be doubted. We are of opinion, from microscopical observation, that the converse may also occur, and that oxalic may be transmuted into uric acid. But may this change of uric into oxalic acid occur where no urates are seen in the urine? in short, may the conversion have been effected previous to its discharge from the body? Dr. Rees, in answer to this question, makes the following remarks:

"Let us first consider whether we do not occasionally observe severe symptoms in cases characterised by a deposit of urates, identical in kind as well as degree with those observed in oxaluria; and whether, again, we do not occasionally observe in oxaluria an almost entire absence of symptoms, or symptoms of trivial character and identical with those most frequently noticed, where the lateritious sediment prevails.

"On the first point I would observe that nearly all dyspeptics pass urates, and that the severest symptoms of hypochondriasis are to be met with in such cases, without the oxalates necessarily appearing in the urine. On the second point I can most confidently state, that I have had cases under my care in which the excretion of oxalate of lime has gone on even to the production of calculous disease, in which hypochondriasis and irritability have never been prominent symptoms. I have, in fact, entirely failed to detect the peculiar pathological conditions which have been said to connect themselves with the oxalic acid diathesis, and am every day more confirmed in my opinion that it must be regarded, as I have before suggested, as an accidental and unimportant modification of that most significant variation from health which consists in the excretion of uric acid or its compounds in abnormally increased proportion." (p. 8.)

Dr. Rees' experience with regard to the occurrence of oxaluria as a prominent symptom in gout, is confirmatory of the above view; and he suggests that the reason why the connexion between gout and oxaluria

* British and Foreign Medico-Chirurgical Review, April, 1856, p. 317.

has so long remained unknown, is because the oxalate of lime constantly escapes notice, whereas the uric acid deposits are obvious to the patient.

If we admit the formation of oxalates from urates, the uric acid diathesis may be regarded as the chief source of calculous disease, since phosphatic calculi are but rarely met with without a nucleus of a different character, which by its irritation gave rise to the deposit of phosphates (upon Dr. Rees's theory) from the alkaline secretions of the mucous membrane of the urinary passages. When pure phosphatic calculi are met with—

"They are only observed in cases where the mucous membrane of the bladder has become greatly diseased, and where of necessity we have the alkaline secretion poured out from it in quantity. This state of things, as is well known, often follows upon enlargement of the prostate with stricture, so that the bladder is not easily emptied; portions of urine always remaining in the bladder after micturition. These retained portions will have their earthy phosphates precipitated by contact with the alkaline secretion of the diseased mucous membrane. In this state of matters it is easy to imagine how a calculus may form." (p. 25.)

A case of eversion of the bladder is quoted in confirmation of the fact that the secretion of the vesical mucous membrane when inflamed has the power of rapidly rendering the urine alkaline. In that case the fundus of the bladder projected so as to expose the orifices of the ureters.

"The mucous membrane was red and inflamed from exposure, and alkaline fluid was constantly discharging from its surface. To what this alkaline flux amounted during the day it was of course impossible to ascertain; but it was more than sufficient to destroy the acidity of the urine, which was quite alkaline after flowing over the membrane."

Blue litmus paper applied to the mouth of a ureter was reddened, but even at the distance of a quarter of an inch below the opening, the urine had become sufficiently alkaline to restore the blue colour of the paper.

Dr. Rees applies his theory to the formation of calculi in the bladder and in the kidney; but however we may be inclined to adopt the views of which we have given the above summary, we think the author fails to establish his point with reference to the kidneys. After remarking that the alkaline secretions of the bladder are necessary and sufficient by their chemical action to cause the precipitation of calculous matter in the bladder, he observes that analogy justifies our assuming the same process to effect the formation of calculi in the kidney. For fear of being misunderstood, we quote the author's words:

"A moment's reflection will serve to show that we have the same conditions present in the kidney which we have noticed with regard to the bladder; that is to say, we have urine, and a mucous surface with which the urine comes in contact, and analogy points to its secretion possessing the same chemical qualities."

If the analogy held good, we ought to find the urine alkaline whenever the kidneys were subjected to irritating or inflammatory influences; but the reverse is the case, the secretion presenting a greater amount of acidity under those circumstances than in the healthy state. Dr. Rees dwells upon the desquamation of the renal epithelium which occurs in some forms of nephritic irritation, as favouring the production of calculus, by entangling the lithic acid, or lithates, which are the prominent constituents of renal calculi. But though willing to admit the influence of

the renal epithelium in entangling and causing a further enlargement of small portions of lithic acid after they are eliminated from the urine, such mechanical action can scarcely be compared to the chemical properties so well urged by the author as characterizing the secretions of the vesical mucous membrane.

Whether or not we adopt the author's theory with regard to the formation of renal calculi, his remarks on the symptoms and consequences of this disorder are of much interest. We advert specially to his observations on the subject of calculi becoming encysted in the kidneys. In certain cases marked by all the symptoms of renal calculi, severe pain in the loins, sides, and abdomen, nausea, vomiting, and hæmaturia, there is *sudden* relief, attributable to the passage of the calculus from the kidney to the bladder; in others, the relief from pain is *gradual*. Patients—

"Who suffer in this way are apt to have a return of symptoms after a day or two, and are restored to health only after a series of attacks. It is often difficult to persuade them that there can be anything in their kidney; and after a year or two of impunity, they will hint that a mistake must have been made in their case. They are not likely to be satisfied with less than the production of the stone, if they have once heard of it; and this may only see light on a post-mortem examination, and therefore too late to produce a modification in their opinion. When treating such cases, the practitioner had best explain at first the relief of symptoms which may probably take place by the calculus becoming encysted—a termination, I have reason to believe, not always sufficiently expected by the profession." (p. 40.)

The renal calculus which appears to give rise to the least amount of suffering, and is often characterized by hæmaturia only, is the oxalate of lime variety, which, from the constant draining of blood, with the absence of pain, is apt to induce a belief that the patient is suffering from malignant disease. Dr. Rees admits the difficulty, which must have occurred to many physicians, of arriving at a correct diagnosis in a case of this kind. The microscope may not show anything in the urine beyond the blood-corpuscles. In malignant disease of the kidney, it is by no means necessary that corpuscles characteristic of malignant growth should be visible. In cancerous growth affecting the bladder, we are more likely to meet with

"Cells characteristic of malignant growth. These, which are intimately intermixed with the blood-corpuscles, have an appearance unlike that of any ordinary matter deposited from the urine. These cells are of variable size, the smaller being about four times the diameter of a blood-corpuscle; the larger twice that size, and even of greater diameter. They are colourless, and more transparent than the white corpuscles of the blood, and contain within them nuclei of varying size. These nuclei differ in number in each cell. Sometimes one only is present, sometimes four or five. Though there would appear a general tendency on the part of these bodies to assume the circular form, they are for the most part of irregular outline. Sometimes a mass of them may be seen agglutinated together, and then they are more or less square, or they may approach to the hexagonal form. After many years' experience in the examination of urinary deposits, I can affirm that I have never seen corpuscles like these in the urine, except in cases of malignant disease." (p. 52.)

Dr. Rees lays much stress, and properly so, on the importance of a correct diagnosis of the cause of hæmaturia, more particularly with the view to preventing the undue introduction of instruments.

"It is not a very unlikely supposition that deaths are every year accelerated, and perhaps sometimes absolutely produced, by sounding in cases of malignant disease of the bladder, morbus Brightii, and renal calculus."

In speaking of cystitis connected with vesical calculus, as liable to induce the assumption of its presence, Dr. Rees expresses a doubt as to the existence of an idiopathic disease of that kind altogether. We are satisfied that at least catarrhal inflammation of the bladder ought not to be eliminated from our nosological classifications as a mere fiction; but then, this affection, like catarrhal inflammation of other mucous membranes, is not one likely to be met with in the post-mortem examination.

On the subject of hæmaturia as a symptom of disease in the urinary organs, the author offers several important practical suggestions. He doubts the occurrence of idiopathic hæmorrhage, and regards it solely as an indication of deeper-seated disease of the kidney or the accessory organs. The main questions that arise in the case of hæmaturia depending upon disease of the kidneys, affect the presence of calculus in the organs of Bright's disease, or of malignant growths in the kidney. The presence of albumen when the urine is not tinged by blood, is stated to be a sufficient indication of the presence of Bright's disease.

"If, however, the urine, on becoming of its natural colour after an attack of hæmaturia, does not prove to contain albumen, then we may feel nearly sure that the hæmorrhage proceeded either from a calculus in the kidney, or some malignant disease of the organ."

With regard to the diagnosis of Bright's disease, if we may still include all forms of chronic albuminuria under one term, we would observe, first, upon the comparative rarity of hæmorrhage as a symptom, and, secondly, upon the importance of certain well-known microscopic characters to which Dr. Rees does not even advert, and which consist in the presence of renal epithelium and the epithelial or fibrinous casts of the renal tubules. The diagnosis between hæmaturia dependent upon calculus or malignant disease of kidney, is summed up under the following heads:

"1. In malignant disease the blood is generally passed in larger quantity than in calculus of the kidney.

"2. There is more frequent tendency to nausea *on slight occasion* than in calculous disease.

"3. Microscopic examination of the urine will frequently show pus or mucus in excess, if there be calculus; whereas in malignant disease this sign does not so frequently exist.

"4. The appearance of those suffering from malignant disease of the kidney, is nearly always indicative of a state of anæmia more or less advanced.

"5. In calculus, hæmaturia generally follows upon some unwonted exertion.

"6. Careful examination of the abdomen will frequently lead to the detection of tumour, if there be malignant disease of the kidney." (p. 64.)

Our readers will infer from the account we have given of Dr. Rees' views relative to calculous disorders, that his treatment consists mainly in the exhibition of alkalies, with the intention of rendering the urine alkaline as secreted by the kidney. He especially recommends for this purpose the citrate of potash in the effervescing form. For the relief of the violent pains attending renal calculus, he approves of the administration of opiates; and in severe hæmaturia, he gives the acetate of lead as the drug possessing the greatest power in controlling and arresting the sanguineous flux.

Review IX.

1. *Lectures on the Diseases of Women.* By Charles West, M.D., Fellow of the Royal College of Physicians, &c. &c. Part I. *Diseases of the Uterus.—London*, 1856. 8vo, pp. 413.
2. *A Review of the Present State of Uterine Pathology.* By James Henry Bennet, M.D.—*London*, 1856. 8vo, pp. 99.

Those who have benefited by the study of Dr. West's excellent work 'On Diseases of Children,' will doubtless welcome another work by the same author. Nor will they be disappointed; for although the same amount of originality could not reasonably be expected, the present volume possesses very solid merits of its own. It is remarkably well written, its style is easy and flowing, and the form of lectures divests it of formality, and permits a familiarity of observation which facilitates the communication of information. Moreover, Dr. West has brought to bear upon the diseases of the uterus all that increase of pathological knowledge which minute and careful investigation, aided by chemistry and the microscope, has so strikingly developed within the last few years. On the other hand, the volume has hardly the comprehension and completeness of a systematic treatise, and although the practical part sufficiently reflects the author's experience, it might have given more fully the recorded experience of others.

The present volume is only a portion of the entire work: it embraces diseases of the uterus, including disorders of menstruation, hypertrophy and inflammation of the uterus, displacements, polypus, fibrous growths, and cancerous diseases. Fully appreciating the great value of the views put forth, we shall endeavour, as far as our space permits, to give the reader a short sketch of Dr. West's opinions upon most of these subjects.

The first two lectures are introductory, and occupied with the general consideration of female diseases, their symptoms, &c., illustrating, amongst other things, the mistake of regarding them merely as local affections on the one hand, or, on the other, of attributing them always to a constitutional origin; whereas they are sometimes one and sometimes the other, and not unfrequently a compound of both. The author next dwells upon the three modes in which diseases of the uterus show themselves—viz., in disordered function, alteration of sensibility, and change of texture; and he lays down at some length, and very judiciously, the appropriate modes of investigation. Manual examination, external and internal, with ocular inspection, are the only means of ascertaining organic changes, and we are glad to find Dr. West dwelling strongly upon the value of the former, which seems to us to have been rather under-estimated since the employment of the speculum has been so general. In Dr. West's favourable opinion of the uterine sound we also concur, provided that it be employed in suitable cases; and also that—

"In the majority of cases, its introduction causes some pain, though this is generally by no means severe, and is almost always of very short duration; and in no instance which has come under my observation have dangerous consequences

resulted from its use, though awkwardness and foolhardiness have, I know, done mischief with this as with almost every other instrument that has ever been invented." (West, p. 17.)

Dr. West's estimate of the practical value of the speculum is not so favourable as that of many recent writers, and although we agree with him in thinking that some of the topical applications for which it has been employed have been overrated, yet we must admit that there are others of great importance in the application of which the speculum is most useful. As a help in diagnosis, he

"Thinks that the advances in knowledge of uterine disease, of which it was the indirect occasion by the impulse which it gave to their study, are sometimes confounded with those positive additions to our information which we owe exclusively to the use of that instrument. The former have been very great indeed; and I think candour compels us to acknowledge, that they have been due almost exclusively to persons who, not content with our previous means of investigating uterine disease, have laboured to increase them by the employment of instruments. The latter have certainly been less considerable, but, nevertheless, the speculum enables us, in many instances, to decide at once and with certainty upon the nature of a case which otherwise we should only have understood after long and careful watching; to discern some minute polypus, which the finger alone would not have detected; to determine the source of a profuse leucorrhœal discharge, and to decide whether it is furnished by the cavity of the womb or the walls of the vagina; or from the redness, congestion, or abrasion of the os uteri, to infer the state of the womb generally; and thus to conduct our treatment upon the sure ground of positive observation, not upon bare presumptions. At the same time, however, that I hold the speculum to be, in many cases, of most essential service, I think that the endeavour of all of us should be, to ascertain the minimum of frequency with which its employment is necessary. This is to be done, not by decrying the instrument, still less by attributing dishonest motives to those who use it, but by soberly and honestly trying to test the information which we derive from it, and learning to discriminate between those appearances which the speculum discloses that are of moment, and such as are of no importance." (West, pp. 23, 24.)

Lectures III. and IV. are occupied with the disorders of menstruation. The description of the different varieties is very good, but, as we might expect, there is nothing new advanced. When speaking of the local causes of amenorrhœa—such as absence of the ovaries and uterus, &c.—the author relates two cases in which he had reason to suspect that the former organs were wanting; but he observes that, during life, we can only *infer* this to be the case by comparing the history and symptoms with those of other cases in which post-mortem inspection has demonstrated their absence. The remarks upon the first occurrence of menstruation, its delay, or the substitution of a white discharge, are practical and judicious. In treating cases of amenorrhœa, allowance must always be made for natural variations and disturbing causes, in which little or no interference is necessary; but after eliminating these, there still remains a considerable number who may be benefited by our aid, and these Dr. West divides into two classes—the plethoric and anæmic, and the treatment must vary accordingly. When there is no local effort, our object is to strengthen the system by tonics, mild pure air, &c., and to guard as far as possible against cold, remembering how liable this class of patients is to phthisical attacks. If there be any uterine effort, we must try and aid it by hot hip-baths, stimulant diuretics, iron, &c. Electricity has occasionally succeeded,

but its effects are uncertain. Ergot has failed, as might have been anticipated; and as to more heroic means, we quite agree with Dr. West, when he says that

"All violent measures, such as the administration of cantharides or of the oil of savin, in large doses; or very powerful local stimulants, such as vaginal injections of liquor ammoniæ, mixed with milk; or the introduction of nitrate of silver into the uterine cavity by means of Lallemand's *porte caustique*, appear to me to deserve reprobation, as both uncertain and unsafe." (West, p. 43.)

Menorrhagia is classified, according to our author, according as it depends upon—1. Some general constitutional cause; or 2. Some affection of the sexual system. Among the former are enumerated, the effects of certain climates; an altered state of the blood; irregularities of circulation, especially in the decline of life; certain diseases of the liver and other abdominal organs, &c. In such cases, however, when persistent, a secondary change takes place in the uterus. Among the causes which act primarily upon the uterus, are all those which produce excitement and congestion of that organ or the ovaries—such, for example, as excessive or incomplete sexual intercourse. Dr. West, we think, is the first to notice the ill effects of the latter, and our own experience amply bears out his statement. The indications of treatment in menorrhagia are, of course, to arrest the hæmorrhage, and to remove its cause. Tonics, regulated diet, rest in the horizontal position, astringents when the hæmorrhage is passive, and bleeding or leeches if there be general febrile disturbance, are the remedies upon which Dr. West relies. He enumerates four special astringents—viz., gallic acid, matico, alum, and lead: in the two former he has the most confidence. We are rather surprised at the very qualified approbation of ergot of rye expressed by Dr. West, as we have generally found it very effectual. Indian hemp, which we can also strongly recommend, does not seem to have been tried by him. Digitalis has been less effective in his hands than in those of Mr. Dickinson and Dr. Lee. External irritation by a blister, which is sometimes so beneficial, is not mentioned; but he relates a case in which, as a *dernier ressort*, he injected gallic acid into the uterine cavity, without causing pain or subsequent inflammation, and with the effect of arresting the discharge.

Dysmenorrhœa is divided into neuralgic, congestive, and mechanical; and the description of each variety is very accurate. Dr. West adopts Dr. Simpson's view of the nature of the dysmenorrhœal membrane, that it is analogous to the decidua, both being the mucous membrane of the uterus, modified and exfoliated. It may possibly be so, but the evidence for either is hardly conclusive as yet.

We are inclined to agree with the author, that the frequency of mechanical dysmenorrhœa has been overrated; but when it does exist, he prefers dilatation by metallic bougies, or a small sponge tent, to a division of the stricture by the *bistouri caché*; of which he remarks:

"I am perfectly at a loss as to the principle on which these instruments are recommended. If the cervix (os) uteri be wide enough to admit them, I do not see how its narrowness can be a mechanical impediment to the escape of the menses. I can, however, readily understand that the uterus may suffer severely from the violence offered to it; and, indeed, have known pelvic abscesses succeed to some of these manipulations." (West, p. 86.)

The next subjects to which our attention is directed are, hypertrophy and acute inflammation of the uterus; and to Dr. West's observations upon the former of these affections, we would particularly direct the reader. That form of hypertrophy dependent upon deficient involution of the uterus, and which we believe was first pointed out by Dr. Simpson, is here shown to have a very important practical bearing: in fact, that many cases which have been regarded as dependent upon recent changes, and for which it is difficult to find a cause, may be traced back to a distant confinement for their solution. We feel no doubt that this will, in future, enter more largely into our calculations as to the origin of uterine disease. Dr. West regards pregnancy and delivery, prematurely or at the full time, as among the most common exciting causes of diseases of the sexual organs; and if to these we add menorrhagia, we cannot hesitate to agree with him. Hypertrophy may undoubtedly result from inflammation after delivery; and—

"It must, however, be at once apparent that, after inflammation has passed away, its effects may remain in the larger size and altered structure of the womb; and that the very nature of these changes will be such as to render the repair of the damaged organ both unlikely to occur and slow to be accomplished, and must leave it in a condition liable to be aggravated during the fluctuations of circulation and alternations of activity and repose to which the female sexual system is liable. It must also be obvious that, for these results to be produced, it is by no means necessary that the inflammation be very severe in character, but that a degree of inflammatory action, far short of what is requisite to endanger life or to occasion much suffering, may yet interpose a great obstacle to the complete involution of the womb." (West, p. 92.)

There are few, probably, who have not met with cases of this kind; and when of long standing it is often very difficult to trace them to their origin, or to decide whether the hypertrophy is an active increase, or an arrested decrease of the organ. The symptoms are chiefly mechanical, and due to the size and weight; the patient complains of fulness and weight in the pelvis, and a degree of bearing down; but there may be also excessive menstruation and a good deal of pain. The condition of the organ will be detected by an internal examination.

But besides this species of hypertrophy, we undoubtedly meet with cases in which it is due to a growth in size, although it may be difficult to explain the exact nature of the morbid process. Dr. West remarks of such cases, that he has met with them "exclusively in women who have lived for a longer or shorter time in childless marriage;" and that they

"Present themselves in most instances without any definite clue to their history; sense of weight in the pelvis; pain, usually of a burning character; and hæmorrhages having gradually come on, and forced themselves by their slowly increasing severity (sometimes not till after the lapse of years) on the patient's notice. Excessive or intemperate sexual intercourse does not produce it, though that leads to its own train of evils; but there has, in many instances, seemed good reason for associating the condition with the imperfect performance of that function, and sometimes the evidence of that being the case has been conclusive." (West, p. 94.)

Rest, attention to the bowels, local leeching at intervals, with the internal administration of iron, with iodide of potassium, seem to be the remedies which Dr. West has found most useful. We may add,

from our own experience, the application of strong tincture of iodine over the cervix, and the use of the medicated pessaries of iodine or hydriodate of potash.

After describing acute inflammation of the unimpregnated uterus, Dr. West proceeds to treat of the *quæstio vexata*, chronic inflammation and so-called ulceration of the cervix uteri. On the publication of his Croonian Lectures, we laid his views pretty fully before our readers, so that there is no occasion to repeat them now. We shall merely state that the chapter On Chronic Inflammation of the Cavity and Substance of the Uterus, has given a degree of completeness to Dr. West's views, as well as afforded an opportunity of explaining some of the symptoms common to ulceration and other affections, which was felt as a *desideratum* in his former work. Taken altogether, although there remains a considerable difference between the views of Drs. West and Bennet, we think there has been really an approximation.

Dr. Bennet has also, it appears to us, not modified perhaps, but guarded his opinions more carefully in the little work which we now introduce to the reader's notice. The same pathological views are maintained, but the limits and qualifications and checks are more prominent. The work is altogether of a controversial character, in answer to various attacks which have been made upon his larger work, but controversies conducted in such a spirit are of advantage both to the writer and the profession at large. Dr. Bennet's personal opponents are Dr. Lee, Dr. West, and Dr. Tyler Smith. By the aid of Dr. West's Reports he disposes of Dr. Lee very satisfactorily, and then takes up the cudgels against Dr. West's assertion, that although ulceration or erosion is more frequent than some have supposed, yet that it is of little pathological significance, neither giving rise to the symptoms attributed to it, nor requiring peculiar local treatment. In answer to which, Dr. Bennet repeats his former opinion, but lays great stress upon the ulceration being caused by, and coincident with, chronic inflammation, to which he attributes the local and general symptoms. He considers this inflammation to commence in, and to be ordinarily confined to, the cervix-canal or substance; whereas Dr. West conceives the lining membrane of the cavity to be the starting point.

In opposition to Dr. Tyler Smith's opinion, that the primary cause of the erosion and consequent symptoms is a morbid secretion from the cervical canal, he maintains that the local characteristics are those of inflammation, and that the increased or depraved secretion is a secondary consequence; and, in many cases at least, we are inclined to decide in his favour.

After these personal encounters, we have a detailed examination of the "leucorrhœal theory, the syphilis theory, the ovarian theory, and the displacement theory," which have each been set up in opposition to Dr. Bennet's views, and as an explanation of the class of cases to which he has especially directed attention. We strongly recommend this part of the work, as possessing great interest and treated with great ability. His remarks upon displacements we think particularly good. Our space forbids us to do more than extract the conclusions at which he arrives:

"Thus cases may and do occur in which inflammatory lesions of the neck of the uterus, including ulceration, exist without presenting any pathological im-

portance. In some women, the organic sensibility of the womb, and its sympathetic connexion with the rest of the economy, are so slight, that severe uterine disease, inflammatory or other, may exist for months or years—as in other organs—without producing much local discomfort, or much general disturbance; but these are exceptional cases. To conclude from them that inflammatory lesions in this region are, as a general rule, of no pathological importance, is to state what is, on the one hand, contrary to experience, and on the other, contrary to the laws of general pathology to which I have so often and so confidently appealed in the course of this review.

"Thus leucorrhœa often exists as a mucous membrane and follicular hypersecretion, the result of physiological or pathological congestion, and may in some rare cases exercise a morbid reaction on health, and require treatment. But to consider this hypersecretion as the essential disease that generally produces the symptoms of uterine ailment, local and constitutional, and to look upon the recognised inflammatory lesions and reactions of uterine mucous membrane as mere symptoms of this essential disease, is to ignore utterly the laws of general pathology. It is indeed to mingle together, in inextricable confusion, the cause, nature, symptoms, and sequelæ of uterine disease.

"Thus ovaritis exists both in the acute, subacute, and chronic forms, and when it is present reacts of course on the uterine functions, giving rise to a regular sequence of symptoms; but to attribute to subacute ovaritis the cases in which tenderness, pain, and fulness of the ovarian region are found, and to look upon the co-existing uterine lesions and symptoms as merely sympathetic conditions, is simply a pathological error, the result of pathological prepossessions. It is giving to the ovaries, pathologically, the same pre-eminence in the female genital system that they really do exercise physiologically—a pre-eminence to which they have no real claim.

"Thus primary and secondary syphilis are both observed in the neck of the womb; but their presence is, in reality, so rare, that even in the wards of a syphilitic hospital they are seldom observed, and they have very little to do with the uterine disease observed in town practice.

"Thus displacements of the uterus are constantly met with; but, except in extreme cases, they are in reality of secondary importance. They often exist in the healthy without being recognised or complained of; and they often remain after the removal of disease without distress or inconvenience being experienced. Whilst in those who suffer from uterine ailment they generally coexist with decidedly inflammatory lesions, their presence may be generally explained by these lesions; and they generally disappear by degrees, as the inflammatory lesions are cured and removed." (Bennet, p. 81.)

In a controversial point of view, Dr. Bennet has attained a considerable measure of success. If his theory has its weak points, its too sweeping and exclusive generalization, so that we cannot always quite agree with him, he has certainly shown that this equally applies to his opponents, and that certain of them have much slighter foundation for their conclusions. But the lesson we should all learn from these controversies, is the danger and evil of one-sided opinions; of adopting a theory, and trying to explain all cases on that hypothesis, instead of admitting the manifold character of disease, and endeavouring to add to our knowledge by more extended observations.

Let us now return to Dr. West's volume. The description of the way in which prolapse and procidentia of the uterus is produced is one of the best in the English language. In it Dr. West follows the late Professor Kiwisch, whose eminence is unquestioned here and on the Continent. He agrees with the highest modern authorities in attributing much more importance to the vaginal supports of the uterus, in preventing its descent,

than to the so-called ligaments. But when all the supports have been relaxed by abortion or childbearing, or even by long-continued leucorrhœa, if at the same time the uterus be larger or heavier than natural, from disease or deficient involution, then a gradual depression takes place, which in time may result in prolapse, or even in procidentia. But this is not all, for the same mechanism involves, according to Dr. West, a certain amount of retroversion in all cases of prolapse. And, moreover—

"This misplacement of the womb does not happen, or at least occurs comparatively seldom unaccompanied by other alterations both in the organ itself and in the surrounding parts. The womb, subject to constant and unusual irritation, obeys the law which we observe to be exemplified in almost all the affections to which it is liable, and increases in size by a process of simple hypertrophy, which differs from the enlargement of pregnancy only in the somewhat greater density of the tissue. The neck of the womb is the part in which this alteration chiefly takes place; for it is the neck which is exposed to the most and the most constant irritation. This enlargement, too, occurs both in length as well as in thickness; so that the neck of the womb may not only be found nearly of the thickness of the wrist, but also greatly elongated, and the os uteri be thus approximated to the pelvic outlet, not simply by the general descent of the womb, but also in great measure by positive growth of its neck. The lips of the uterus become enlarged, together with the rest of the womb, and the small transverse aperture which in women who have borne children should represent the orifice of the womb, becomes converted into a wide opening, situated deep in between the projecting lips, whose surface, irritated and excoriated, presents, in parts at least, a vivid red, finely-granular surface, covered by a copious albuminous secretion." (West, p. 156.)

Thus increased in volume and weight, with relaxed and yielding supports, the progress of the case is pretty certain, though slow, if it be not quickened by some sudden exertion or force acting down upon the pelvis, which is most probable; and this downward course is traced with great accuracy by Dr. West, who also describes with great care the secondary displacements of the vagina and bladder. He seems doubtful as to the first step in vaginal cystocele—that is, whether the protruding vagina draws down the bladder, or the distended bladder pushes before it the vagina. Judging from the cases which have come before us, we think that the latter is the ordinary course, as also that the first step in vaginal cystocele is over-distension of the rectum; although, perhaps, in both we may presume a degree of undue relaxation in the supports.

The symptoms to which these displacements (by the way, we are inclined to quarrel with Dr. West's substitution of the word misplacements for the older and better one) give rise are very well described, as well as the information to be acquired by sight and touch. In the treatment recommended, there is an important practical distinction, to which we would draw attention:—

"Sometimes the prolapsus is the result of causes which add to the weight of the uterus, and thus render its ordinary supports unequal to maintain it in its proper position, while in other instances a weakening of the supports themselves by accident or disease is the first step towards producing the misplacement; and according as the one or the other of these conditions predominates, will the use of mechanical means be expedient or undesirable. Thus, for instance, time and care, and judicious management, generally suffice to remove that form of descent of the womb which succeeds to miscarriage or labour, whereas the as yet imperfect

involution of the organ, and its consequent increase of weight, are the main causes of its misplacement, while mechanical contrivances are always needed when the support which the vagina should afford has been destroyed by extensive laceration of the perineum or greatly enfeebled by the atrophy of old age." (West, p. 174.)

We quite agree with this passage, but we would also apply the same rule to those cases in which the depression is the result of removable disease, unconnected with delivery.

Of the various contrivances to afford mechanical support, Dr. West seems to prefer the globular or disc pessary of boxwood, and he speaks rather favourably of the india-rubber bags, which are introduced empty and inflated afterwards by a tube attached to them. He speaks very satisfactorily of Dr. Ashburner's bandage, and indeed of Hall's utero-abdominal supporter, and especially as to the relief afforded from the sympathetic pains. He does not give much encouragement to operative proceeding, having known but little benefit either from closure of the outlet, diminution of the calibre of the vagina, or the entire removal of the uterus.

The mechanism of retroversion of the womb has been partially alluded to, and the reader will find it very satisfactorily described by our author. We should have liked to see some statement of his opinion, however, as to its normal mobility. We believe this to be considerable, and we suspect that some cases which have been pronounced and treated as such were within these limits. We agree in this with Dr. Bennet—

"That even the unimpregnated uterus, in health, is by no means destined to remain constantly in the same anatomical position, to preserve constantly the same axis. It is also equally evident that the healthy uterus bears changes of position and considerable pressure from surrounding organs, &c., without either pain, discomfort, or inconvenience."

Abstracting these cases, however, modern researches have shown that these versions or flexions of the unimpregnated womb occur as a morbid state much more frequently than was suspected. The passive conditions, as we may call them, are a relaxed condition of the supports of the uterus, and a certain amount of depression; if, then, in addition, the fundus of the uterus acquire additional weight posteriorly, we have all the necessary conditions of retroversion; or if the weight be anteriorly, of anteversion. The actual completion of either displacement may occur gradually, without consciousness or distress, or in consequence of sudden force the process may be momentary.

We concur with Kiwisch and West, that it is difficult to understand how such a displacement as anteversion can occur in an otherwise natural condition of the womb, nor have we had reason to believe it frequent. It may, however, be produced by disease of the anterior wall; but these cases are rare, and Dr. West thinks that "the probabilities are that in most instances when the uterus has been supposed to be anteverted it was in reality anteflexed, or its fundus bent forward on its cervix, or else that the indurated and enlarged uterus was tied down in its position by old adhesions." Indeed, if we consider the connexions of the uterus anteriorly, it is not quite easy to understand the occurrence of anteflexion. The point of flexion, whether anterior or posterior, appears to be the junction of the cervix and body, and Virchow's explanation is the one

preferred by Dr. West. Our suspicions may be aroused by a digital examination, but it will be difficult indeed to decide between retroflexion and a tumour of the posterior wall, unless we are able to employ the uterine sound. The treatment will vary according as we decide with Schweighauser, Schmidt, and Oldham, to attempt the removal of the cause in the first instance, or content ourselves with remedying the displacement mechanically. The former is at least the most philosophical proceeding. After enumerating some objections to the stem pessary, Dr. West thus states his own plan:

"On these accounts, though I have tried the uterine supporter in a few cases, I have now for some time quite given up its employment, and content myself with a mode of treatment which, though it seems to promise less, yet almost always affords great relief, which in a large number of cases quite removes the patient's sufferings, and is not unfrequently followed by the complete rectification of the position of the womb. The principle, indeed, upon which I act in the management of these cases amounts pretty much to this: that to the best of my power I take care of the general symptoms, and leave the misplacement to take care of itself. (West, p. 225.)

We must slightly qualify our agreement in Dr. West's plan of treatment: we have certainly found, as he has, that when the enlarged condition which causes the displacement is removed, the uterus will, in most cases, resume its proper position; but we have also found that in a few cases this re-position did not take place, and that certain inconveniences were the consequence, which were capable of being removed by mechanical support. This we have afforded, not by Dr. Simpson's stem pessary, but by one made of gutta percha, which acts on the principle of distending the vagina upward behind the uterine neck.

In addition to the local remedies mentioned by Dr. West for the removal of that condition which causes the displacement, we may mention that we have found great benefit from the local application of iodine.

Lecture XIII. is devoted to the consideration of inversion of the uterus, either as occurring after labour, or as a chronic complaint, in which its mode of production, causes, symptoms, and treatment are fully discussed; but as Dr. West's views do not materially differ from the standard authorities, we need not detain the reader, but shall pass on to the subject of polypus uteri. Under this term Dr. West includes mucous, cellular, glandular, cystic, and fibrous polypi, and he has given a remarkably clear description of each. All give rise to nearly the same symptoms, the most prominent being hæmorrhage, which may be as profuse with the smaller polypi as with the larger, depending probably upon their nearness to the cervical canal; at least, we find it much more profuse when the outgrowth is enclosed in that canal than when it projects far into the vagina. As to the source of the hæmorrhage, Dr. West remarks that—

"The growths themselves are well supplied with vessels; if wounded, they bleed; if excised, the hæmorrhage which takes place from their pedicle is sometimes considerable, has even been known to prove dangerous; but yet all evidence goes to prove that it is rather from the womb itself than from the outgrowth that the principal bleeding flows, and that the hæmorrhage is proportionate, less to the size of the outgrowth than to the intimacy of the relation between it and the womb." (West, p. 258.)

The diagnosis may be difficult or impossible if we trust alone to the

finger, but even with the speculum we may not discover the polypus if it be within the cervical canal or the uterus.

"The only rule that can be given for practical guidance, however, is this: that in no case of long-continued menorrhagia should we be content with mere digital examination, but should invariably employ the speculum; and further, if no satisfactory conclusion be thereby arrived at, we should dilate the os uteri with sponge tents, in order that the cervical canal may be brought within reach both of examination with the finger and with the speculum." (West, p. 260.)

The removal of these outgrowths is the only remedy; the smaller ones may be twisted off, the larger twisted and excised; the cysts punctured or scarified, and the acid nitrate of mercury applied. Dr. West objects to the forcible avulsion of polypi, and sees no advantage in their strangulation by forceps constructed for that purpose.

At the end of this chapter there is a short but interesting account of those semi-organized clots sometimes found in or expelled from the uterus, which have been termed fibrinous polypi, and were first described by Professor Kiwisch.

"In certain conditions, independent, as he believes, of impregnation; consequent, as others think, upon previous abortion, the walls of the uterus may be so soft and yielding as to allow of the gradual accumulation of effused blood in the cavity of the organ. In the course of time the clot may not only pass through those changes by which the colouring matter is removed from its exterior, which assumes a dirty white or greyish aspect, while portions of a dark red hue are still to be found within, but may also be the seat of the same kind of imperfect organization as has been observed in the case of hæmorrhages into the arachnoid, or of blood effused in other situations. Like cardiac polypi, so these become firmly adherent to the walls of the cavity within which they form; and the late Franz Kilian, of Mayence, found one, whose constituent fibrin was in various stages of fibrillization, while its surface had received a partial investment of tesselated epithelium, which he believed to be due to the advanced organization of the outer layer of fibrin." (p. 263.)

The uterus, irritated by the presence of the clot, ultimately contracts and expels it. In a case, we believe to be of this nature, which was under our care, the increased bulk caused retroversion, which was spontaneously rectified after the expulsion of the clot.

The lecture on Fibrous Tumours of the Uterus is a very able one, bringing to bear upon the subject all the light which modern investigation has thrown upon its nature and history. We know now that the structure of these tumours is nearly identical with that of the uterus itself. However they may differ in appearance, when divided they consist of fibres of dense cellular tissue, or of tendinous substance, or of elastic tissue, intermixed with cytoblasts and granular matter, with the broad unstriped muscular fibres of the uterine tissue. Their situation varies; they may occupy the outer or inner portion of the uterine parietes, and in process of growth may project beneath the peritoneum, or into the uterine cavity, forming polypi; or they may remain in the centre of the uterine substance. The presence of these tumours gives rise to increase in the size of the womb, but greater in proportion with the smaller tumours and with those that project into the cavity, and that not merely by distension but by growth of the tissue and unfolding of its muscularity, such as takes place in pregnancy. These morbid growths vary much in number

and size, and in their course there are attempts made at cure, sometimes successful, at others unsuccessful.

"In the case of fibrous tumours, there are five different modes in which this attempt is made. Either the pedicle undergoes a process of gradual attenuation, and then gives way, the tumour thus becoming detached from the uterus; or more rarely, a portion of its investment becomes ulcerated or dies, and the growth gradually shells out from the sheath of the cellular membrane which contained it; or a change takes place in its substance, the exact nature of which is not quite understood, it becomes disintegrated, dies, and is got rid of piecemeal; or a different change occurs, similar to what we see in other morbid products,—the tumour undergoes the cretaceous transformation, and though not eliminated from the womb, it ceases to stand in any vital relation to it, and the symptoms which it once produced, diminish or altogether disappear." (West, p. 272.)

In the majority of cases the diagnosis is not very difficult.

"The sound may show the cavity of the uterus to be elongated; and I believe that an enlarged and heavy and somewhat hard uterus, coupled with the causeless occurrence and frequent return of uterine hæmorrhage, while the os and cervix uteri are healthy, are almost always pathognomonic of fibrous deposit in the uterine substance."

Still, cases occur every now and then which require the utmost care and skill to avoid mistakes, and these are principally where the question is between uterine and ovarian enlargements, sometimes between fibrous tumours and pregnancy, and sometimes of fibrous tumours complicated with pregnancy. The argument *par voie d'exclusion*, as the French term it, is of great value here; and, in addition, we may derive more or less positive evidence from the previous history of the case, from the equal or unequal density of the tumour, and from the use of the uterine sound. Dr. West's remarks upon these exceptional cases are very judicious, and the rules he lays down as precise as is possible under the circumstances. There is one sign, however, which he does not notice, and which we have found very satisfactory in many cases. If the point of the finger be placed on the cervix uteri, and slight but sudden pressure or percussion be made upon the abdominal tumour, the shock communicated to the finger is distinct and clear when the tumour is uterine, or when the communication is continuous and unbroken; but it is obscure and faint in the case of ovarian tumour.

As to the treatment, we agree with Dr. West, that if by precautionary measures we can prevent mischief, and by the use of iodine, &c. we can retard their growth, it is quite as much as we can expect. He has not succeeded in arresting or diminishing them, and he is not favourable to the surgical operations which have been attempted, unless the tumour have passed into the vagina.

The last subject treated of in the volume is Malignant or Cancerous Disease, under which title is included the varieties of ordinary cancer, epithelial cancer, corroding ulcer, &c. The author gives a very good description of the pathology of cancer, according to the most recent researches, and its effects upon the neck of the womb, its most common seat; or when it attacks the body of the organ; as also the condition and changes in the neighbouring tissues.

Among the predisposing causes of cancer, we find age very prominent, its frequency increasing with every ten years beyond twenty. The condi-

tion of the menstrual function seems to exert no influence, and, contrary to the common belief, women who have not had children are not more liable to cancer. Hereditary taint prevails to a certain extent.

The most common symptoms, as every one knows, are pain, hæmorrhage, and leucorrhœa; and Dr. West has given us the following statement as to the occurrence of the first symptoms in 116 cases. In 23 it began by pain; in 50, by hæmorrhage; in 12, by pain and leucorrhœa; and in 18, by leucorrhœa without pain. He has entered minutely into the characters, course, and variations of each of these symptoms, and has given a graphic description of the cancerous cachexia. Of the results of cancer of the neck complicating labour, we find that in 74 cases, 41 women died in or very soon after labour, and 33 recovered from the effects of labour; while of 72 children, 47 were born dead, and 25 alive. In 17 cases, Dr. West was able to ascertain the exact duration: it was under four months in 1 case; under five in 2 cases; under nine in 1 case; under twelve in 3 cases; exactly a year in 2 cases; between one and two years in 4 cases; between two and two and a half years in 2 cases; between two and a half and three years in 1 case; and exactly three and a quarter years in 1 case: giving an average duration of fifteen months; rather less than the average in Lebert's cases.

The treatment of cancer consists in maintaining, as far as possible, the general health, and in the relief of local symptoms. Dr. West does not believe in the possibility of arresting the first stage of the disease. For the purpose of arresting the hæmorrhage, all exciting causes must be removed, the bowels kept free by mild saline aperients, and a mild unstimulating diet employed. We agree with Dr. West, that local depletion is a very questionable measure at best, and decidedly bad if the blood be drawn from the uterus itself. Internally, gallic acid seems to have succeeded best as an astringent, and infusion of matico as a local application. We strongly recommend Dr. West to try the tincture of Indian hemp, from which we have derived much benefit.

In soft medullary cancer, or epithelial cancer, Dr. West recommends Kiwisch's plan of breaking down the tissue with the finger, and injecting into the midst of it the tincture of the sesquichloride of iron.

Sooner or later, anodynes become necessary, and we are wisely recommended to commence with the mildest. Some kind of astringent injection will be advisable to control the discharge, and Dr. West prefers matico, tannin, or oak bark to zinc or alum, and, we think, with good reason. He speaks highly of a very weak acid lotion, as not only diminishing the discharge, but improving the surface of the ulcer. We can corroborate Dr. West's statement, that a solution of the nitrate of silver will not only improve the ulcer and remove the fœtid odour, but frequently relieve the pain.

After detailing the treatment required by the cancerous cachexia, and the secondary affections, we come to the feasibility of any operative proceedings. The propriety of removing the entire uterus is disposed of by the fact, that of 25 cases in which it was tried, 22 died in consequence of the operation, without any adequate prolongation of life.

Excision of the cervix is less objectionable, of course, but we rarely see the disease in the most favourable stage for the operation; and we are

very much inclined to agree with Dr. West, that the operation should be almost limited to epithelial cancer of the cervix.

We have now laid before our readers a sketch of the contents of this valuable work, briefly, it is true, but sufficiently full to justify the high opinion we expressed at the commencement of this notice.

We trust to welcome contributions from the pen of Dr. West for many years to come; for when a man of such a scientific mind and careful observation and untiring diligence applies himself to any given department, the result must be a great and valuable augmentation of our information.

Review X.

On the Constitutional and Local Effects of Disease of the Supra-Renal Capsules. By Thomas Addison, M.D., Senior Physician to Guy's Hospital.—*London*, 1856. pp. 39. Plates.

Dr. Addison, the distinguished Senior Physician of Guy's Hospital, rather more than a year ago, laid before the medical profession a monograph, in which he endeavoured to prove that a peculiar bronzed condition of the skin, accompanied by a remarkable and fatal form of cachexia, is characteristic of disease of the supra-renal capsules. He states that he stumbled upon the curious facts which form the groundwork of his interesting and suggestive essay, while seeking to throw some additional light on a form of anæmia occurring without any discoverable cause whatever, in cases "in which there had been no previous loss of blood, no exhausting diarrhœa, no chlorosis, no purpura, no renal, splenic, miasmatic, glandular, strumous, or malignant disease." That he should have *stumbled* upon them is not remarkable, inasmuch as, in the present state of our knowledge, no *à priori* reasoning could have suggested any, even the remotest, connexion between disease of the supra-renal glands and disease of the tegumentary system. Yet to have recognised the importance of facts that have come, as it were, accidentally before us, and then, by a careful investigation of them, to have acquired an insight into the laws which regulate their connexion with one another, are deserving of high praise. And to such praise Dr. Addison is fairly entitled, if the observations he has published are the result of as much well-directed labour and thought as his character as an observer would warrant us in believing them to be.

Dr. Addison, after characterizing his work as "a first and feeble attempt towards an inquiry into the functions and influence of the supra-renal capsules, as suggested by pathology," expresses the hope that—

> "However unimportant or unsatisfactory his facts may at first sight appear, they may, by attracting the attention and enlisting the co-operation of the profession at large, lead to the subject being properly examined and sifted, and the inquiry so extended as to suggest at least some interesting physiological speculations, if not still more important practical indications." (p. 4.)

This hope has been to some extent realized; and chiefly through the energy of Mr. Jonathan Hutchinson,* a large amount of evidence has been collected from all sources, strongly confirmatory of the views

* Medical Times and Gazette, Nos. 297 and 299, 1856.

originally promulgated by Dr. Addison. We feel—though truly the whole subject is still involved in deep obscurity—that the time has already arrived when the facts that have accumulated may be weighed, and their value approximately estimated.

We purpose in the present article first to describe the symptoms and pathology of the disease which forms the subject of Dr. Addison's book, and then to discuss the evidence by which *it is supposed* that the dependence of the general symptoms on disease of the supra-renal bodies is established. It is only fair, however, to premise, that from a comparison of almost all the cases that have been recorded subsequently to the appearance of Dr. Addison's work, and of those which are published by Dr. Addison himself, Mr. Hutchinson has so carefully described the disease, and so fairly examined the evidence in relation to it, that there is little else left for us to do, in the almost necessary absence of anything like a good practical acquaintance with the subject, than to follow in his footsteps and to make free use of his valuable papers.

Dr. Addison's description of the disease is short, and we are therefore tempted to quote it entire. He says:—

"The leading and characteristic features of the morbid state to which I would direct attention, are, anæmia, general languor and debility, remarkable feebleness of the heart's action, irritability of the stomach, and a peculiar change of colour in the skin, occurring in connexion with a diseased condition of the supra-renal capsules.

"As has been observed in other forms of anæmic disease, this singular disorder usually commences in such a manner, that the individual has considerable difficulty in assigning the number of weeks or even months that have elapsed since he first experienced indications of failing health and strength; the rapidity, however, with which the morbid change takes place, varies in different instances. In some cases that rapidity is very great, a few weeks proving sufficient to break up the powers of the constitution, or even to destroy life; the result, I believe, being determined by the extent, and by the more or less speedy development, of the organic lesion. The patient, in most of the cases I have seen, has been observed gradually to fall off in general health; he becomes languid and weak, indisposed to either bodily or mental exertion; the appetite is impaired or entirely lost; the whites of the eyes become pearly; the pulse small and feeble, or perhaps somewhat large, but excessively soft and compressible; the body wastes, without, however, presenting the dry and shrivelled skin, and extreme emaciation, usually attendant on protracted malignant disease; slight pain or uneasiness is from time to time referred to the region of the stomach, and there is occasionally actual vomiting, which in one instance was both urgent and distressing; and it is by no means uncommon for the patient to manifest indications of disturbed cerebral circulation. Notwithstanding these unequivocal signs of feeble circulation, anæmia, and general prostration, neither the most diligent inquiry, nor the most careful physical examination, tends to throw the slightest gleam of light upon the precise nature of the patient's malady; nor do we succeed in fixing upon any special lesion as the cause of this gradual and extraordinary constitutional change. We may indeed suspect some malignant or strumous disease; we may be led to inquire into the condition of the so-called blood-making organs, but we discover no proof of organic change anywhere—no enlargement of the spleen, thyroid, thymus, or lymphatic glands—no evidence of renal disease, of purpura, of previous exhausting diarrhœa, or ague, or any long-continued exposure to miasmatic influences; but with a more or less manifestation of the symptoms already enumerated, we discover a most remarkable, and, so far as I know, characteristic discoloration taking place in the skin—sufficiently marked indeed as generally to have attracted the attention of the

patient himself, or of the patient's friends. This discoloration pervades the whole surface of the body, but is commonly most strongly manifested on the face, neck, superior extremities, penis and scrotum, and in the flexures of the axillæ and around the navel. It may be said to present a dingy or smoky appearance, or various tints or shades of deep amber or chesnut-brown; and in one instance the skin was so universally and so deeply darkened, that, but for the features, the patient might have been mistaken for a mulatto.

"In some cases this discoloration occurs in patches, or perhaps rather certain parts are so much darker than others, as to impart to the surface a mottled or somewhat checkered appearance; and in one instance there were, in the midst of this dark mottling, certain insular portions of the integument presenting a blanched or morbidly-white appearance, either in consequence of these portions having remained altogether unaffected by the disease, and thereby contrasting strongly with the surrounding skin, or, as I believe, from an actual defect of colouring matter in these parts. Indeed, as will appear in the subsequent cases, this irregular distribution of pigment-cells is by no means limited to the integument, but is occasionally also made manifest on some of the internal structures. We have seen it in the form of small black spots, beneath the peritoneum of the mesentery and omentum—a form which in one instance presented itself on the skin of the abdomen.

"This singular discoloration usually increases with the advance of the disease; the anæmia, languor, failure of appetite, and feebleness of the heart, become aggravated; a darkish streak usually appears upon the commissure of the lips; the body wastes, but without the extreme emaciation and dry harsh condition of the surface so commonly observed in ordinary malignant diseases; the pulse becomes smaller and weaker, and, without any special complaint of pain or uneasiness, the patient at length gradually sinks and expires. In one case, which may be said to have been acute in its development as well as rapid in its course, and in which both capsules were found universally diseased after death, the mottled or checkered discoloration was very manifest, the anæmic condition strongly marked, and the sickness and vomiting urgent; but the pulse, instead of being small and feeble as usual, was large, soft, extremely compressible, and jerking on the slightest exertion or emotion, and the patient speedily died.

"My experience, though necessarily limited, leads to a belief that the disease is by no means of very rare occurrence, and that were we better acquainted with its symptoms and progress, we should probably succeed in detecting many cases, which, in the present state of our knowledge, may be entirely overlooked or misunderstood; and, I think, I may with some confidence affirm, that although partial disease of the capsules may give rise to symptoms, and to a condition of the general system, extremely equivocal and inconclusive, yet that a more extensive lesion will be found to produce a state, which may not only create a suspicion, but be pronounced with some confidence to arise from the lesion in question. When the lesion is acute and rapid, I believe the anæmia, prostration, and peculiar condition of the skin will present a corresponding character, and that whether acute or chronic, provided the lesion involve the entire structure of both organs, death will inevitably be the consequence." (pp. 4—7.)

The description, derived from an analysis of about twenty-seven cases which Mr. Hutchinson gives of the disease, is strongly confirmatory of the account which we have just quoted; and in the commentary we are about to subjoin, we shall avail ourselves largely of his labours.

The change of colour of the skin would appear to be the most marked and constant symptom. If the accounts that are published are trustworthy, it would seem that this change is one of the earliest symptoms of the disease, that it frequently precedes all others, and that it becomes more and more intense up to the final issue. The tint assumed evidently

varies in different cases, for sometimes the skin is described as having a light yellowish-brown hue, while at other times it is stated to resemble that of a mulatto. In all cases, however, it would seem that the term "bronzing" conveys a good idea of the character acquired by the skin. "It strongly resembles," says Mr. Hutchinson, "the colour of a bronzed statue from which the gloss has been rubbed off." Pressure has no effect in causing its diminution. It seems, as a rule, to commence in patches with ill-defined borders on those parts exposed to the air and to friction, and on those parts where pigment naturally abounds, and to spread thence over the general surface of the body. Those parts, however, which originally present little or no pigment, as the palms of the hands, soles of the feet, ungual matrices, &c., appear to remain unaffected to the last. The discoloration is said occasionally to invade the lips, and occasionally to extend even to other parts of the mucous membrane of the mouth. But it has not been satisfactorily observed in any other part of the body. For although Dr. Addison states that in one case there was black pigmentary deposit in the peritoneum, it must be borne in mind that there was tubercular peritonitis as well, and in such cases the appearance of black spots and patches in that situation is exceedingly common. Hence it would appear that the discoloration is, so far as is at present known, strictly limited to the skin and those portions of the mucous membrane which adjoin integument. And it is important to bear in mind that the conjunctivæ are stated in almost all cases to have remained clear and pearly. We may add that, in three instances, a peculiarly disagreeable sickening odour was exhaled from the patient's body, a circumstance which does not appear to have attracted Dr. Addison's attention, and which possibly may not have existed in any of his cases. Mr. Hutchinson observes that:

"Next to the bronzing of the integument, the extreme and peculiar feebleness manifested appears to be the most striking of the symptoms. Without any evidence of thoracic disease the patient becomes liable to faintings, loses energy, is unable to exert either body or mind, and, in short, appears to be on the point of death from sheer weakness.

"That there has generally been observed a want of correspondence between the extreme debility and the degree of emaciation coincident with it, seems evident. Several of the patients are described as having remained muscular and fat up to the very last. In almost all, however, there had been some loss of flesh, and in many it had even been considerable. Dr. Addison's observation, that flabbiness of the solids rather than actual wasting is characteristic of the condition, seems true of the majority of cases.

"In almost all cases there would seem to have been present great depravation of the coloured constituents of the blood, as manifested by the pallor of those parts not involved in the bronzing, the general flabbiness of the muscles, the pearly state of the conjunctiva, &c. In two only was the blood examined with the microscope, and in both those it was found to be loaded with white corpuscles.

"In almost all cases prior to death, and in many for protracted periods, great irritability of the stomach was present. In most there was loss of appetite, more or less persistent nausea, and occasional vomiting, with pain and sense of sinking at the epigastrium. In the majority it would seem that the bowels have been costive rather than otherwise, while in a few, attacks of diarrhœa had occurred." (Med. Times and Gaz.)

Symptoms referrible to disorder of the cerebro-spinal system occurred

in several of the cases. In a few instances death was preceded by a low form of muttering delirium. In one case failure of memory was noted; in another numbness of the fingers, legs, and tip of the tongue. In two or three cases neuralgia was present. Two patients had epileptic attacks, but in one of the two this complication was clearly due to disease of the medulla oblongata.

In the generality of cases the pulse was peculiar only in its extreme softness and compressibility.

Pain in the loins was frequently complained of, but is probably to be looked upon as only a part of that general debility from which the patients were suffering.

Neither the tongue nor the urine appears to have exhibited any important deviation from the healthy condition.

Judging from the few cases in which the mode of death has been described, Mr. Hutchinson remarks, "the phenomena attending death are those of utter prostration of the vital powers, not unfrequently complicated by disturbance of the nervous functions."

From all that is above stated, we think it may be assumed that the distinctive features of the disease under consideration are the peculiar discoloration of the skin, and the general anæmic condition; for neither the dyspeptic nor the nervous symptoms are sufficiently constant or uniform in character to render it probable that they are anything more than the natural sequelæ of the progressive and extreme debility. Now, the anæmic condition does not appear to us to differ, except in the fatality that attends it, from that which accompanies many other forms of disease; and hence it is clear that it is upon the peculiar changes taking place in the skin that we must mainly rely in forming our diagnosis. It is therefore highly important that we should be able to distinguish the bronzed condition of the integument, supposed to be indicative of renal-capsular disease, from other affections attended by discoloration, to which the skin is liable. Mr. Hutchinson has, we believe, correctly pointed out the distinctive marks by which a differential diagnosis between true bronzing of the skin and other cutaneous affections may be established. *Jaundice* may be discriminated, not only by the general symptoms which accompany it, but by its peculiar tint, by its uniform diffusion, and by its presence in the matrices of the nails and in the conjunctivæ. *Browning from exposure to the sun* may be recognised by its occurrence in those situations only which are habitually exposed.

"*Patches of Pityriasis versicolor* sometimes remarkably resemble those of bronzed skin. Their limitation to the abdomen and chest, their defined outline, their furfuraceous surface, the slight itching which attends them, their contagious character, and, above all, the microscopic examination of the cuticle, furnish, however, abundant means by which to distinguish between the two." (Med. Times and Gaz.)

Mr. Hutchinson remarks, lastly, that it is important not to confound the diffused brown muddiness of some other cachexiæ with the bronzing of supra-renal disease. With regard to the means of distinguishing by means of the microscope between pityriasis versicolor and true bronzed skin, we may remark, that although we believe Mr. Hutchinson's opinion will prove correct, we suspect that he has asserted more than our present knowledge justifies him in asserting. It is somewhat strange, yet we

believe it to be a fact, that up to the present time it is a mere assumption that the bronzed condition of the skin depends on pigmentary deposit—at least, so far as we know, no account of its microscopical examination has been published.

The morbid anatomy of the supra-renal capsules need not detain us long. The diseases which have been found in them, in connexion with bronzed skin, are various:—1. Acute and recent inflammation, ending in abscess. 2. Atrophy, with fibro-calcareous concretions. 3. The conversion of the viscus into a sort of fibroid structure, with great enlargement and induration. 4. The deposit of tubercle, or of a fibroid material resembling tubercle. 5. The growth of cancer. Occasionally the affection of the glands, especially when of a cancerous nature, appears to be secondary to disease in other parts; but it is a very interesting fact, that in many cases these bodies were the only organs in which disease was detected. It is scarcely necessary to say that the supra-renal glands are sometimes partially diseased, and that sometimes one or both are wholly destroyed; but it is very important to bear in mind, that, to judge from the cases that have been published, the degree of bronzing, and the severity of the general symptoms, appear to have been proportionate to the amount of disease in these bodies, and to have had no relation whatever to the nature of that disease.

It will, we conceive, be readily conceded, that the facts above given are very remarkable, and if the suggested connexion between them turn out to be real, exceedingly valuable and important. We will proceed, therefore, now to examine the evidence by which the dependence of the fatal cachexia, and of the concurrent change in the colour of the skin, upon disease of the supra-renal capsule, is thought to be proved. Before we enter on this subject, however, it is very essential that it should be clearly understood, that the proof of the coincidence of these phenomena by no means establishes that the one is the cause of the other; indeed, such a proof would still leave it an open question,—whether the disease in one of the organs is the cause of the morbid changes in the other, and of the general symptoms—or, whether the supra-renal disease, and the affection of the skin, like the intestinal ulceration and cutaneous rash of typhoid fever, are concurrently the efflorescence, if we may so express it, of some more deeply-seated systemic mischief.

Up to the present time, about 33 cases of bronzed skin, including the 12 originally furnished by Dr. Addison, have been published. Of this number all but one died. In 6 cases no autopsy was made, and in 2 the supra-renal glands were overlooked at the time of the post-mortem examination. But in every other case—that is, in every one of the remaining 23—these bodies were found in a more or less diseased condition. And hence it follows, that notwithstanding the close attention that has for several years past been paid to the subject in Guy's Hospital, and the general interest that has been manifested in it throughout the profession since the publication of Dr. Addison's work, upwards of a year ago, not a single case has been published, in which a bronzed condition of the skin has been proved to have existed without manifest disease of the supra-renal capsules having been present at the same time. Now we do not mean to affirm that all the 23 cases, in which the association of these

phenomena was proved by post-mortem examination, are altogether trustworthy; the details of several are very meagre and imperfect; and with regard to one or two, perhaps, a suspicion might arise that the authors had unconsciously modified or moulded the facts, to adapt them to Dr. Addison's views. Still, allowing for all these possible sources of error, we feel convinced that a critical examination of the above cases, so far from weakening, will tend materially to strengthen the evidence in favour of the close connexion between the two pathological phenomena of which we are speaking. In 7 of the cases, either one organ only, or both partially, were diseased. In 16, the healthy structure of both organs was wholly destroyed; and in 9 or 10 of these, the supra-renal bodies were the only organs in which any trace of disease was recognised. In nearly every one of the 16 cases in which both organs were found affected, the change in the colour of the skin was so marked, as to have attracted the attention of the patient and his friends; and in several of the cases that are related by Dr. Addison, and in several of those that have been subsequently described, disease of the supra-renal glands was diagnosed during life. One of the latter cases is so remarkable and conclusive, that we are tempted to give a short abstract of it. It was under the care of Drs. Ranking and Vincent, and a complete account of it was published in the 'Medical Times and Gazette' for May 24th of the current year. The patient was a lady, fifty-nine years of age. In May, 1855, she first observed that the skin of her face and hands was discoloured, and she was often annoyed when making calls, by friends offering her water to wash her hands. Her appetite failed, the stomach rejected almost everything, and emaciation became very evident. The symptoms gradually increased, and in August, and again in October, she consulted Dr. Ranking, who states, that on these occasions she complained mainly of great and increasing loss of strength, with sinking at the pit of the stomach, nausea, and complete loss of appetite. The face was dark brown, as dark, in fact, as that of a Japanese. The hands also were discoloured, especially at the knuckles. Her heart's action was feeble. The secretions of the liver, intestines, and kidneys were healthy. The case continued a complete mystery to all who were concerned in it up to December, when Dr. Vincent, happening to read a review of Dr. Addison's work, was struck by the remarkably close analogy between the case that so sorely puzzled him and those which are described by Dr. Addison. He mentioned the suspicions that naturally arose in his mind to Dr. Ranking, who, when put in possession of the circumstances on which they were founded, fully acknowledged their justice. The consequence was, that the case was published in the 'Medical Times and Gazette' for December 22nd, 1855, as one in which supra-renal disease probably existed. The symptoms continued with but little alteration up to the 25th of April, 1856, on which day the patient died, extremely emaciated, after several hours' muttering delirium. A tolerably careful post-mortem examination was made. All the abdominal and thoracic viscera were found healthy, with the single exception of the supra-renal glands. These bodies were enlarged, they were infiltrated with a putty-like deposit, and their normal structure was wholly destroyed. That they were completely disorganized is confirmed by the testimony of Drs. Addison and Wilks, to whom they were referred for examination.

There is one branch of the evidence which seems strangely to have been overlooked by Dr. Addison: it is that which is furnished by an examination of the supra-renal capsules in those cases in which no bronzing of the skin has existed. This deficiency has been supplied by Dr. Wilks, who states (Dec. 29, 1855) that, in 500 post-mortem examinations conducted in Guy's Hospital during the previous two years, in one instance only was disease in the capsules found unassociated with discoloration of the skin, and in that case only a few malignant tubercles grew from the surface of one of the organs. It is quite certain, however, that, during the last year, several examples (to say the least) have been met with, in which one or both glands have been partially diseased, and in which no discoloration of the skin has occurred. We have ourselves, within the last four months, examined four cases in which these bodies were partially diseased—three times with cancer and once with fibro-calcareous deposit—and in which we do not feel justified in admitting that there was any unusual discoloration whatever of the skin. Nevertheless, no case has yet been published in which, when both glands were wholly diseased, bronzing of the skin did not co-exist. We feel justified, therefore, in saying that the following facts are demonstrated by the evidence adduced:

Sixteen cases have been recorded in which a bronzed condition of the skin was associated with total destruction of the supra-renal capsules;

No case of bronzing of the skin has been published in which the capsules were found healthy;

In those cases in which partial disease of the capsules was detected, bronzing of the skin has been sometimes present, sometimes absent;

But, not a single case is on record in which total destruction of the capsules has existed, without manifest discoloration of the skin having existed also.

We think, with these facts before us—facts the truth of which we have no reason whatever to question—that we cannot do otherwise than admit that there is really some very close connexion between bronzing of the skin and disease of the supra-renal capsules: nay, further, that there is very strong reason for believing that bronzing of the skin may be looked upon as diagnostic of disease of the supra-renal capsules.

Assuming, then, the intimate connexion between these two pathological phenomena to be established, it remains for us to inquire what the nature of that connexion is. Is the capsular disease dependent on the skin affection?—are these two morbid conditions the coincident effects of some other cause?—or is the affection of the skin produced by disease of the supra-renal capsules?

Now, by reference to the statements which were made in the last paragraph, it will be seen that, though a bronzed condition of the skin seems always to have been attended by disease of the capsules, disease of the capsules has not always been accompanied by bronzing of the skin; and hence it is clear that the cutaneous discoloration may be dependent on the capsular affection, but that the converse of that proposition cannot be for a moment maintained. That the two morbid conditions are the coincident effects of some other cause, is a view the probability of which has most likely suggested itself, at one time or other, to the minds of all who have bestowed any thought on the subject; but we believe a little con-

sideration will prove it to be altogether untenable: for had the morbid condition of the supra-renal glands been produced by some agency which was at the same time working its ill effects on the integument, we should have expected to find the same unity of type in the disease of the glands as appears to have been found in that of the skin. The diseases in the glands, however, which have been found associated with bronzed skin, present the most varied characters; and indeed, all the evidence goes strongly to show that bronzing of the skin, and the cachectic symptoms which accompany it, are found to be associated with every form of disease that has yet been recognised in these bodies, provided that disease be sufficiently extensive to have effected their entire destruction. The third alternative is thus irresistibly forced upon our attention; and that it furnishes a correct explanation, so far as it goes, of the disease in question, is rendered nearly certain by the fact, that those considerations which militate so powerfully against the first two alternatives, are strong positive arguments in favour of this. We believe, therefore, that the evidence before the profession with regard to supra-renal capsular disease, proves not only that such a condition of the skin as has been described at a previous page is diagnostic of disease of the supra-renal capsules, but that disease of the supra-renal capsules is the cause of that discoloration of the skin, and we may add, of the symptoms that co-exist therewith.

There are yet two or three points which deserve to be alluded to before we bring the present article to a conclusion:—1. Dr. Addison speaks in very gloomy terms of the prognosis of the disease. It is possible that his fears may be well founded. Indeed, it is certain that the published cases confirm them. We must recollect, however, that most of these cases, and certainly the more conclusive ones, were cases in which the glands were structurally and irremediably disorganized; and since we have no reason to suppose that the supra-renal bodies do not resemble all other organs in being subject to transient and remediable forms of disease as well as to progressive and incurable ones, we cannot help suspecting that, as our knowledge of the disease in question becomes more extensive, we shall find that bronzing of the skin, with the attendant symptoms, is not so invariably fatal as it is at present believed to be. 2. With regard to treatment, little can be said. The symptoms usually manifested appear to indicate the desirability of the exhibition of tonics: that is the treatment that seems usually to have been resorted to, and probably with advantage; but with what ultimate benefit, may be in some measure estimated by the invariably fatal result which has attended the cases. 3. Dr. Addison has—wisely, as we think—refrained from speculating upon the functions which the renal-capsules subserve, although he has certainly shown that their importance is much greater than has generally been suspected. We are not more disposed to speculate on the subject than Dr. Addison, and shall therefore refrain from discussing the possibility of their function being a nervous one, which is a view that Mr. Hutchinson, partly on anatomical grounds, seems inclined to adopt.

Finally, we beg to reiterate our belief, that the connexion between disease of the supra-renal capsules and a bronzed condition of the skin, which Dr. Addison sought to establish, has been proved to exist; and we have to thank him, therefore, for a most valuable and interesting contri-

bution to pathology—for a work which, we believe, contains important truths, and the germs of truths probably still more important. It would be ungracious if we refused to acknowledge, also, the services which in this inquiry have been rendered by Mr. Hutchinson, and those other gentlemen who have made Mr. Hutchinson the medium by which valuable cases have been laid before the profession. We venture to hope that their good example will be followed by numerous pathologists, and that ere long the supra-renal capsules may be entitled to hold as definite position on the map of pathology as is at present occupied by other organs which have been successfully investigated by the light of modern science and by the industry of modern observers.

Review XI.

1. *Ueber das Absorptionsvermögen des Bluts für Sauerstoff.* Von G. Magnus. ('Annalen der Physik und Chemie,' Band lxvi. 1846.)

On the Capacity of the Blood for the Absorption of Oxygen. By G. Magnus.

2. *The Effects of Respiration on the Inspired Air: Gases Absorbed and Given Out by the Blood.* (Chap. xxv. in 'Letters on Chemistry in its Relation to Physiology, Dietetics,' &c. By Justus von Liebig. 1851.)

3. *Respiration.* ('Lehrbuch der Physiologischen Chemie.' Von Prof. Dr. C. G. Lehmann. Band iii. p. 284. Zweite Auflage.—*Leipzig,* 1853.)

On Respiration. ('Handbook of Physiological Chemistry.' By Professor Lehmann. Second Edition.)

Physiology may now be said to have reached that point where an accurate knowledge of the changes produced by the respired air upon the animal economy becomes indispensably necessary not only to the just appreciation of the normal functions, but also to the comprehension of the assimilation of food and the action of medicines upon the body, not a single function of which is independent of respiration. Nervous action, muscular contraction, secretion and excretion, are alike under its sway. No change can take place in either the living or dead animal organism without an interchange of gases. Shut out the atmospheric supply, and development ceases; prevent the action of air, and decay is instantly arrested. No new cell can be formed, no old one destroyed, without the influence of this all-important agent. From the moment when the animal or vegetable germ springs into existence, throughout its development into tissues, during its whole life as an organized being, to the time of its death, and even throughout its decay, until the last cell has been resolved into its primary elements, oxygen has been uninterruptedly employed; and according to the supply of this indispensable agent have these changes been accelerated or retarded. Is it, then, surprising that a complete knowledge of the chemistry of respiration should be considered one of the principal indicators towards the understanding and treatment of disease, and that it should rank as a most important pillar in the structure of rational medicine?

The knowledge of the changes produced by the action of atmospheric air attracts us, therefore, alike by the interest and the importance connected with its acquirement.

The function of respiration, as we have said, knows no intermission so long as animal life continues; by day and by night, sleeping and waking, the lungs are ever performing their allotted labour, inhaling the fresh, and expelling the effete, air. In a single hour, taking the average number of respirations at sixteen in a minute, the gases essential to the continuance of organic life have been renewed 960 times; in the course of every twenty-four hours, no fewer than 23,040 times; and before we had passed a single short year of independent existence, our lungs had been inflated and exhausted, inhaling and exhaling oxygen and carbonic acid with each effort, until the respirations had reached the enormous number of 8,409,600. These figures, taken alone, are sufficient to point out the indissoluble bond existing between the respiratory functions and the continuance of "vital action" in the animal economy; and render us anxious to discover not only the various causes inducing the absorption of oxygen, but likewise the intricate transformations produced by its presence in the blood.

Many points connected with the changes brought about in the animal economy through the agency of the respired gases, are still involved in obscurity. We know that gases enter the blood; we know that gases of a different character, possessing other properties, emerge thence; but the form in which they exist in the liquid, and the changes they induce in its constituents, remain, in spite of all that has already been achieved, subjects to be elucidated by future investigation. If we were content to form our opinions from the beautiful experiments of Magnus, some of which we are now about to recapitulate, we might suppose that the gases interchanged into the lungs, enter into *no* chemical combination whatever with the constituents of the blood, either in their course to or from the tissues and organs, but form merely a physical mixture with the circulating liquid; a view which our philosophical author has tried hard to promulgate. To this opinion, however, we are by no means inclined to bow, inasmuch as numerous experiments, instituted by ourselves with a view of ascertaining this point, have forced upon us conclusions of a diametrically opposite character. Instead of finding, as we had been led by the experiments of Magnus to anticipate, that the gases remain in the blood unchanged, retained there merely by the laws of mechanical absorption, our experiments demonstrated to us that the gases *cannot* come into contact with blood, and remain in a free state, but must of necessity undergo certain changes, in consequence of their entering into chemical combination with one or more of the constituents of that liquid. Although recent writers have generally adopted this view, we were, we believe, the first to prove its correctness by direct experiment.

Before relating these experiments, we shall pass in review some of those made by Magnus, and quote the conclusions at which he arrived. In so doing we shall have occasion to notice several errors which he was enabled to correct in the doctrines which had, before his time, been accepted without contradiction.

Magnus observed that a certain quantity of arterial blood, shaken

together with carbonic acid gas during several minutes, absorbed a considerable quantity of this gas, while, at the same time it gave off all the oxygen it had previously contained. This circumstance led him to conclude that the oxygen could not have been chemically combined with the blood, but had merely been present in the liquid, in a state of mechanical absorption. He rested this view on the two following grounds: Firstly, the ready displacement of the oxygen by another gas; secondly, the absence of deoxidizing power in carbonic acid gas, the replacing agent. Continuing the investigation, he found that the same portion of blood, re-saturated with oxygen, again yielded up this gas on being shaken together with a fresh portion of carbonic acid, incontestably proving, as he imagined, that these two gases replace each other thus easily in consequence of neither having any affinity for the liquid with which it was mixed beyond that necessarily dependent on the principle of absorption.

Although the fact that oxygen and carbonic acid gases possess the property of replacing each other is undeniable, we cannot help thinking that the conclusions deduced from it by Magnus are of much too general a character; and we are of opinion that a further consideration might probably lead to a considerable modification of these views. The mere fact of a gas being very easily separable from a liquid, does not of itself furnish sufficient evidence that it did not exist in the liquid in a state of chemical combination; nor is the circumstance of the replacement of one gas by another, and *vice versâ*, of any value in proving that such replacement has occurred from the absence of any greater affinity than that of mere mechanical absorption.

As we shall hereafter have occasion to prove the correctness of this statement, we may, to avoid repetition, before entering more fully into the *minutiæ* of the question, at once proceed to mention another of the series of experiments instituted by Magnus, which has a direct bearing upon the preceding one. He found by experiment, that animals, placed under circumstances which prevented their respiring oxygen, and compelled them to breathe nitrogen instead, exhaled from their lungs carbonic acid gas, just as if they had been breathing an atmosphere of common air. (This experiment had frequently before been performed by other inquirers with an exactly similar result.) From this he concluded that the exhalation of carbonic acid gas from the lungs is not necessarily dependent upon the absorption of oxygen; he asserts, moreover, from the same premises, that the carbonic acid exhaled by the lungs must have originated in the tissues, entered the blood traversing the capillaries, and been carried to the pulmonary organs, without entering, during its transit, into any chemical combination with the circulating liquid.

This is one of the facts that have worked together for the total abolition of the theory of respiration as first promulgated by the great French chemist, Lavoisier, who supposed that the respired oxygen entered into immediate combination with the free carbon imagined to exist in the lungs, for the formation of the carbonic acid gas, to be immediately expelled by the succeeding expiration. Had this mode of reasoning been correct, the temperature of the lungs must necessarily have been found higher than that of any other part of the body; for the combination of oxygen with carbon, which forms carbonic acid gas, is the chief source of

animal heat.* The lungs are, on the contrary, found almost the least warm of any of the organs of the body; and this, together with other important facts, has brought Lavoisier's theory into entire disrepute.

Before it was known that gases could be extracted from the blood, oxygen was naturally supposed to have entered into chemical combination with one or other of the constituents of that liquid; since the discovery made by Magnus, that gases not only exist in the blood, but can partly be extracted from it without much difficulty, physiologists, rushing at once to the opposite extreme, asserted, with Magnus, that oxygen enters into no chemical combination whatever, so long as it remains in the circulating fluid, and that such a combination can occur only in the tissues and organs of the body.

Magnus has not only discovered that oxygen exists in venous as well as in arterial blood, but also that carbonic acid can be extracted from arterial as well as from venous blood. The relative proportion of these gases in the two kinds of blood is, however, different—the arterial blood contains more oxygen gas than the venous; the venous, on the other hand, more carbonic acid gas than the arterial. We shall give the author's statement of the fact in his own words:

> "By means of the air-pump, a certain quantity of air was extracted from the arterial as well as the venous blood of various animals, and was subjected to analysis; and numerous experiments proved that the quantity of oxygen contained in the gas obtained from venous blood amounted at most to one-fourth, and frequently did not exceed one-fifth of the carbonic acid therein contained. In arterial blood, on the other hand, the oxygen amounted to at least one-third, and nearly to one-half of the carbonic acid; therefore, although the entire quantity of these gases in the blood cannot be ascertained, I consider absorption to be so essential an element in the function of respiration, that the latter may be considered to depend, if not entirely, at least partly, upon it." (p. 186.)

The fact that a smaller quantity of carbonic acid is found in arterial than is present in venous blood, proves that carbonic acid is either derived from the organs and tissues, and absorbed by the blood in the capillaries, or generated in the blood itself. The former view is adopted by Magnus, to the entire exclusion of the other; and the consequence has been, that although he has many followers, there exists a small class not entirely prepared unconditionally to adopt his views; and among this minority we fear we must be ranked, for we cannot divest ourselves of the belief that *both* causes must be at work to call forth the results attributed by Magnus to one of them alone.

In the preceding quotation from Magnus, it is observed that not only does carbonic acid exist in arterial as well as in venous blood, but oxygen has been discovered in the one as well as in the other. To this discovery he attaches immense importance, looking upon it as the most conclusive of all proofs that the oxygen imbibed during respiration, is not combined with, but only absorbed by, the blood. It may be, perhaps, that we are unable justly to appreciate the value of this statement, or rather of the fact, but it certainly appears to our mind to prove nothing beyond the mere circumstance that the *whole* amount of oxygen respired has not been chemically combined with the constituents either of the blood or of the

* Liebig's Letters on Chemistry, p. 317.

tissues and organs. Moreover, it is possible that the oxygen found in the venous blood forms no portion of that which had immediately before entered the lungs, but it may consist of a part of the gases set free in the retrograde transformation of some of the organic compounds in the tissues or the blood itself. If those who differ from us on this point were even to grant that oxygen enters into chemical combination with the blood, it does not necessarily follow that this liquid is capable of combining with oxygen in an indefinite amount, or that this combination must be instantaneous. We believe, on the contrary, that the constituents of the blood are capable of combining only with a limited quantity of oxygen, in order to form new organic compounds better adapted for transformation into tissues and organs, in the act of assimilation; and that as soon as these compounds are removed from the blood, a constant series of others is ready to replace them, by undergoing the same changes to which their predecessors have been subjected; and this transformation and replacement continue so long as the animal body exists in a state of healthy activity of function. The process of oxidation does not appear to be instantaneous, but, on the contrary, seems to be a slow one, dependent on circumstances to which we shall hereafter have occasion to revert.

The view taken by Magnus appears at first sight to receive additional support from an experiment he made. He took some calves'-blood, well mixed with air, placed it in a vessel with carbonic acid, shook them together during a considerable time, and on analysing the gas, found that the blood had yielded up by this process 11·6 per cent. of oxygen, and absorbed no less than 154·0 per cent. of carbonic acid gas. The same portion of blood, which had become very dark in colour, was again agitated with atmospheric air, by which process it absorbed 15·8 per cent. of oxygen, giving off only 138·4 per cent. of carbonic acid, a volume 15·6 per cent. less than it had taken up, and exactly equal to that of the oxygen which had been absorbed. The same blood was again agitated with carbonic acid gas, and this time it yielded only 9·9 per cent. of oxygen, while it absorbed 92·1 per cent. of carbonic acid. Magnus concluded from these results, that,

"The fact, that almost the whole quantity of oxygen taken up by the blood can be extracted from it again, is a striking proof that oxygen is not chemically combined with, but only mechanically absorbed by, the blood."

To the manipulation of the above experiments we have nothing to object, but our deductions from them are very different from the conclusions drawn by Magnus. According to the figures given above, it appears that blood absorbed 15·8 per cent. of oxygen, and when treated with carbonic acid, yielded up only 9·9 per cent., in other words, 62 per cent. of the whole amount of oxygen absorbed. We are somewhat at a loss to imagine how our author can call this "nearly the whole;" to us it appears to be little more than the half; and we should be inclined to opine that the remaining 37·4 per cent. of oxygen which could not again be extracted from the blood, had most probably entered in part into chemical combination with one or more of the organic substances therein contained. Our only surprise is, that so much oxygen was retained by the blood; and we can only attribute this fact to the supposition that the oxygen and blood were placed under very favourable

circumstances for the production of chemical combination; these circumstances being, agitation, length of time, a certain amount of heat, &c.

The absorption by the blood, at each renewed treatment with air, of a certain quantity of oxygen which could not be extracted from the liquid by means of replacement with carbonic acid gas, forces us to believe that some of it at least had been chemically combined with the blood; for if the oxidation and deoxidation were several times repeated, and a certain quantity of oxygen retained in the blood after each experiment, a point would at length be arrived at where the quantity of oxygen in that liquid must of necessity be far greater than we can suppose it possible for blood to contain by the laws of mere mechanical absorption. Although the blood possesses a greater absorptive power than water for oxygen, this power amounts, after all, to but one-tenth of its volume, and this, according to the ratio of Magnus indicated above, is a much smaller quantity than would be retained in the blood after repeated shakings. The fact that a greater quantity of carbonic acid is necessary in order to displace oxygen, than we need have of oxygen to displace carbonic acid, Magnus attributes to two causes—firstly, that artificially-made venous blood contains a greater amount of carbonic acid than is contained in normal venous blood; and, secondly, to the absence of any membrane having the power of absorption, as found in the lungs. These explanations do not appear to us altogether satisfactory; for we should think that the absorption of oxygen by artificial venous blood would be regulated by the same law which governs its absorption in real venous blood. We are not aware of any chemical difference in these two bloods, beyond the circumstance that the amount of carbonic acid existing in the one, may in some degree exceed that present in the other. And even this difference can only exist under peculiar circumstances; for if the two bloods be left to themselves, exposed to an atmosphere of common air, they would speedily become identical. If we were to hazard an opinion on the subject, we should rest our attempted explanation on very different grounds.

The law of absorption being the same for all gaseous bodies, the amount of a given gas absorbed always stands in direct proportion to the attraction possessed by the liquid for the individual gas at the particular temperature. When blood thoroughly saturated with oxygen is introduced into a tightly-closed bottle, containing a certain amount of carbonic acid, and well agitated, the oxygen gas will escape from the blood, and diffuse itself among the carbonic acid gas; and, on the other hand, the carbonic acid will be absorbed by the blood, until an equilibrium has been established between the portions of the gases contained in the blood itself, and in the free space above it. The interchange of the gases will cease as soon as the proportion of each one in the blood is exactly similar to the amount which the same liquid has the power of absorbing under a pressure equal to the one existing in the closed vessel. As the absorptive power of the blood with regard to oxygen gas is very small when compared with that of the same liquid for carbonic acid gas, the proportion of oxygen escaping from the blood, and diffusing itself throughout the space occupied by carbonic acid, will be very much smaller than the quantity of carbonic acid which the blood will absorb before the equilibrium is established.

It has just been remarked, that the absorptive power of blood for oxygen, though absolutely considerable, is relatively small when compared to the affinity of the same liquid for carbonic acid. Magnus has clearly demonstrated this by an interesting experiment, the result of which was, that one volume of blood can absorb 1½ time its volume of carbonic acid (Davy and others have obtained nearly the same proportions), and only about one-tenth of its volume of oxygen gas. Thus we see that blood has the power of absorbing, on an average, nearly thirteen times as much oxygen as water does; a fact almost sufficient in itself to prove the fallacy of the doctrine, that oxygen does not enter into chemical combination with the constituents of the blood, and one beautifully turned to account by Liebig in his attempted disproof of Magnus's doctrine. Liebig, reasoning from analogy, has shown that the oxygen *must* exist in the blood in some other form than that of mere mechanical absorption; for while, as he says, 1000 volumes of water, when agitated with air until thoroughly saturated, absorb only 9½ volumes of oxygen (Guy Lussac), 1000 volumes of blood, treated in precisely the same manner (Magnus), absorb no less than 100 to 130 volumes of oxygen gas. Now, since the liquid part of the blood is nothing more or less than water, and as blood absorbs from eleven to fourteen times more oxygen than the same quantity of pure water would do under similar circumstances, it is obvious that the excess of oxygen taken up by the blood cannot depend upon the absorptive power of its liquid (which is pure water), but upon the presence of certain constituents having a much more powerful attraction for oxygen than water possesses. Although the degree of attraction by which the oxygen is retained in the blood is comparatively slight, this is by no means a proof that the gas is not in a state of chemical combination; it being well known that very numerous chemical combinations are as readily, or even much more easily, destroyed than that of the oxygen in the blood; and nevertheless they are true chemical compounds, in every sense of the word.

The absorptive power of water for particular gases can be very much augmented by the addition of certain substances possessing a chemical attraction for the gas. Liebig has shown that if one per cent. of phosphate of soda be added to water, the absorptive power of that liquid for carbonic acid gas is immediately doubled; notwithstanding this, the gas can again be separated from the solution by simple agitation with air, just as is the case with venous blood. Still, no one would think of regarding this as an instance of mechanical absorption; for in every case in which any particular gas is retained in a liquid by mere mechanical absorption, the quantity of gas so detained depends entirely upon the tension of the particular gas at the surface of the liquid, and increases or diminishes in the ratio of the tension; whereas, when a gas is chemically combined in a liquid, the absorptive power of the solution does not increase in proportion to the pressure or tension, but stands in direct relation to the amount of the dissolved substance, whose particles have a chemical attraction for the gas. This, as Liebig has remarked, is beautifully exemplified in the case in point. No sooner has the solution of phosphate of soda become saturated with carbonic acid (having taken up twice as much of the gas as water alone would have done under the same pressure), than the absorptive power of the solution ceases to increase in the same ratio when

the pressure is doubled, but augments in a much smaller one. This is in consequence of the cessation, upon the combination being completed, of the chemical attraction which at first increased its absorptive power.

So far as is yet known, the blood behaves towards gases in exactly the same way as a solution of phosphate of soda towards carbonic acid. The attraction possessed by blood for oxygen gas does not follow the law of absorption laid down by Henry and Dalton, and so beautifully explained by Bunsen, but appears to resemble much more closely effects looked upon as those of chemical affinity. The remarkable fact observed by Reisit and Regnault, that animals breathing in an atmosphere containing two or three times more oxygen than goes to the formation of ordinary air, exhibited no visible symptoms of uneasiness, and that the product of their respiration did not differ either in quantity or relative proportion from the result of their breathing in common air, goes far to establish the theory that oxygen is not mechanically absorbed, as Magnus believes, but enters into chemical combination with one or more of the constituents of the blood. The last-mentioned fact, taken *per se*, we believe indeed to have had the opportunity of proving beyond a doubt, by numerous experiments instituted on this subject in Professor Bunsen's laboratory at Heidelberg. The method we employed in analysing the gases obtained by treating the blood in various ways, was the one described by Professor Bunsen, which has become so justly celebrated on account of its great exactitude. Our experiments were made principally with the view of ascertaining—firstly, whether blood has the property of chemically combining with oxygen; secondly, which of its constituents enter into combination with oxygen; thirdly, whether these constituents, by combining with oxygen, simply become oxidised, or whether they at the same time give off carbonic acid gas; lastly, what are the agents which control these changes?

In order to ascertain these points, a certain quantity of blood was agitated with renewed portions of air, until it became thoroughly saturated with oxygen, and had yielded up all its carbonic acid, or at least as much as it could possibly give off. The blood so treated was then introduced into a graduated glass retort, of the shape represented in the accompanying figure (Fig. A). The neck (*a*) was drawn out to a fine capillary tube, upon the end of which was placed a piece of caoutchouc tube. After a certain quantity of blood (*c*) had been introduced at the mouth (*b*), the latter was firmly closed with a tightly-fitting cork, and the remaining opening at (*f*) secured by a ligature drawn tightly on the caoutchouc tube, so that all communication between the external atmosphere and the air in contact with the blood contained in the retort was interrupted. Any change, therefore, that might occur in the imprisoned air, would depend upon the action of chemical combination as no law either of absorption or of displacement could have any effect in bringing about the change; firstly, because the blood was in contact with a gas with which it had been previously saturated; and secondly, because the tension of the air contained in the retort was identical with that of the external atmosphere, under the pressure of which the saturation had been accomplished.

It is therefore clear, as we have stated, that any change occurring in

the air in the retort must be dependent upon chemical action between it and the blood; if no such chemical action took place, the air in the retort would remain entirely unaltered.

To return to our experiment. The retort, after having been repeatedly agitated, that the blood and air might be well mixed, was further laid on its side, in order to bring as great a surface of blood as possible into contact with the air. The temperature of the room where the experiments were made was always carefully noted, as it was found that temperature had a great effect in hastening or retarding the chemical action. After a certain number of hours (usually twenty-four, but sometimes no more than two, four, six, or perhaps eight hours, according as it seemed desirable to vary the experiment), the cork at *b* was carefully removed, under mercury, so that no atmospheric air could obtain admittance into the retort, and none of the gases operated upon could effect their escape. A tube (B), partly filled with mercury, was carefully adjusted to the retort by a well-fitting cork, *d;* the retort and its attached tube were now removed from the mercury trough, and into the free end of the caoutchouc tube a long, fine capillary tube of glass (C) was inserted. The end (*e*) was now dipped under the surface of the mercury in the trough, and the ligature on the caoutchouc tube at *f* removed; the mercury in the tube B immediately descended, forcing the atmospheric air out of the

tube C, the point of which (*e*), on the tube becoming filled with the gas which had been operated upon, was brought under an inverted eudiometer filled with mercury, and more of the same liquid poured into the tube B, until a sufficient quantity of gas had been obtained in the eudiometer. The retort and its appendages were then removed, and the collected gas subjected to analysis according to Bunsen's method.

From among our experiments,* we shall now select a few, which we consider to prove, in the most striking manner, that the oxygen of the atmospheric air cannot be brought into contact with blood without entering into chemical combination with one or more of its constituents.

A certain quantity of fresh ox blood was well agitated with renewed portions of air; and when thoroughly saturated with the gas, it was introduced into the retort, together with 100 per cent. of common air; it was then carefully corked up, and kept, during twenty-four hours, in a room of moderate temperature. That a fresh portion of blood might as frequently as possible come into contact with a portion of oxygen which had not yet been affected by its contact, the retort was, in the course of this period, frequently agitated, and kept on its side during the intervals. At the expiration of the twenty-four hours, the gas was carefully collected in a eudiometer, in the manner described above, and subjected to analysis. The gas was found to have the following composition in 100 parts:

Oxygen . . .	10·42	total of oxygen . . 15·47
Carbonic acid . .	5·05	
Nitrogen . . .	84·53	
	100·00	

Looking at the gas taken from the retort after twenty-fours' contact with the blood, we find its composition no longer identical with that of the common air which was introduced into the retort. Its constituents are materially altered; the proportion of some being considerably increased, while that of others has diminished in a manner no less marked. Ordinary air is said to have the following composition:

Oxygen	20·960
Carbonic acid	0·002
Nitrogen	79·038
	100·000

On comparing this analysis with the former one, of the gas acted upon by the blood, we find that 10·54 per cent. of oxygen has disappeared, while 5·05 per cent. of carbonic acid now exists where only a trace of its presence could before be detected. It thus appears that the blood has acted upon the oxygen of the atmospheric air—first, by combining a certain quantity of it with carbon, from one or more of its constituents, to form carbonic acid; and, secondly, a certain quantity of oxygen has perhaps been exhausted in the oxidation of some of its organic contents; for if we add the oxygen, combined with the carbon, in the form of carbonic acid, to the oxygen remaining free and unchanged in the retort, we find the sum only to amount to 15·47 per cent., instead of being 20·96 per

* While working in the laboratory of Professor Bunsen, at Heidelberg, we made upwards of one hundred analyses in the above-mentioned manner.

cent., as it ought to have been, if none of the oxygen was retained in the blood. Here it is observed, that 5·49 per cent. of oxygen is still to be accounted for. There can be no doubt but that it is contained in the blood; and the great question, therefore, now is, In what form does it there exist? Is it simply absorbed, or is it chemically combined? That the whole is not simply absorbed in the state of uncombined oxygen, can be distinctly averred; for as the blood, before having been brought into contact with the air in the retort, had already absorbed as much oxygen as it was possible for it to do, and since the saturation with that gas was accomplished under exactly the same pressure as it now experienced, it could not by any possibility absorb more of the same gas. The oxygen in the blood must therefore exist in another form. It may, by entering into chemical combination with some of the organic compounds of the blood, have directly oxidised them; or it may have combined with a certain amount of disengaged hydrogen to form water. But as we had no means of ascertaining this, in consequence of water being already present in indefinite quantity, we prefer, in our calculations, to leave this point for the present entirely out of the question. Again, it may, like the other portion of oxygen, have entered into combination with another quantity of the carbon in the blood, and have been retained in the liquid in the form of carbonic acid, by the pressure of the portion of the corresponding gas which had escaped in among the air in the retort, there exerting sufficient pressure to retain the remainder of the carbonic acid in the blood, in a state of mechanical absorption. This view is, however, untenable, as the volume of carbonic acid free in the retort scarcely equals the volume of the same gas which is supposed to be absorbed by the blood. What, then, can have become of the oxygen? The only other way in which we can attempt to explain the phenomenon, is by supposing that a part (not the whole) had entered into combination with some carbon from the organic substances in the blood, and was there kept in a state of mechanical absorption by the pressure of the carbonic acid diffused throughout the air in the retort, while another portion had combined with the blood to oxidise it. If the combination of oxygen with hydrogen to form water is left entirely out of the question, this certainly appears to us the most probable view; but in our own mind, we still incline to the opinion, that some of this oxygen, for the disappearance of which no direct explanation is furnished by the result, has combined with a certain amount of hydrogen to form water. Although we have as yet had no opportunity of proving this by direct experiment, still, reasoning from the circumstance that by each expiration we exhale a quantity of aqueous vapour which is supposed to have been produced in the blood, chiefly by a combination of the inspired oxygen with the hydrogen from the organic substances, we may assume the possibility of a similar phenomenon having occurred, unnoticed in the course of these observations.

Valentin and Lehmann assert that the quantity of azotised food exerts a powerful influence on the amount of urine passed, the hydrogen of the ingesta having combined with the inspired oxygen to produce water; and Böcker has shown the amount of urine passed to be uniformly in excess of the quantity of fluid taken. For example, he found that if 1260 grammes of liquid be taken, 2621 grammes of urine will be passed; if

3360 grammes be taken, 4994 grammes will be passed in twenty-four hours.

If the foregoing be the true explanation of the facts, it is seen that the constituents of the blood have become oxidised in two ways: firstly, by direct combination with oxygen; secondly, by the loss of carbon. Mulder's theory of the oxidation of protein substances may not, therefore, be so far from the truth as some authors have supposed; future investigations may yet impart to it fresh importance.

We shall here cite another of our experiments in corroboration of the above.

A quantity of defibrinated, fresh arterial blood from a calf was agitated with air during half-an-hour, until there could not remain the slightest doubt as to its thorough saturation with oxygen. As in the preceding cases, it was then put into the retort with 100 per cent. of atmospheric air. After standing for twenty-four hours, during which time it was repeatedly shaken, the gas was subjected to analysis, as in the previous experiment, and found to consist in 100 parts of the following proportions:—

Oxygen . . .	11·33	Total oxygen, 17·29.
Carbonic acid .	5·96	
Nitrogen . . .	82·71	
	100·00	

Here again, as in the example already given, a certain quantity of free oxygen has disappeared (= 9·63), and a great increase in the amount of carbonic acid has taken place (= 5·96). Although the proportion of carbonic acid is in this case greater than in the preceding one, the quantity of oxygen which has disappeared is still somewhat less; and if we add the free oxygen to the oxygen combined with the carbon in the form of carbonic acid, we shall find that the total of oxygen amounts to 17·29 instead of 20·96, as in air; so that 3·7 per cent. of oxygen has totally disappeared, and must, as we have before mentioned, have entered into chemical combination with the organic substances in the blood, and also with the carbon, to form a limited amount of carbonic acid, which, by the law of absorption, is retained in the blood. These two experiments, it will be observed, point to the same conclusions; and a number of others, where the mode of procedure was precisely analogous, and which were attended with similar results, have convinced us of the fallacy of the doctrine of Magnus, that the oxygen received into the blood during respiration is only mechanically absorbed into, and not at all chemically combined with that liquid.

It appears to us, from these experiments, that a portion of oxygen combining with some of the constituents of the blood to prepare them for assimilation, probably also enters into combination with some other of the effete products, in order to render them more fit for excretion. That this process of chemical combination is a slow one, and that it occurs gradually, is proved by the different amounts of air transformed. When, for example, the air was simply passed through a tube with a number of bulbs containing blood, which had been treated in the manner before mentioned, the quantity of air changed was very small. The analysis was as follows:—

Oxygen	20·520
Carbonic acid	0·921
Nitrogen	78·559
	100·000

A certain time is thus shown to be required for the development of the chemical changes.

An additional proof that chemical action occurs in the blood, is furnished by the fact that during digestion more nutritive material is absorbed into the circulation than is requisite for the support and development of the body, and that this excess is afterwards excreted in a different form from that in which it entered the system, without having ever become assimilated with the tissues. The transformation of these substances must therefore have occurred in the blood, and it is natural to suppose that the oxygen in that liquid would not remain entirely inactive during these chemical changes;—indeed, it is utterly impossible to account for the enormous quantity of carbonic acid which is daily expelled from the lungs, by any other assumption than that of the respired oxygen having combined with a certain amount of the carbon of these substances. Upon this supposition, Liebig founds his theory of the generation of animal heat.

It is well known that certain substances are formed in the circulation. Urea, for example, although secreted by the kidneys, is not formed by them. The researches of Prévost and Dumas have shown that, in the blood of animals whose kidneys had been extracted, a great quantity of urea existed, while only a trace of that substance could, before the extraction of the kidneys, be detected. In disease of the kidneys, the same thing has often been observed. We may therefore conclude that urea is not found in the kidneys, but only excreted by them.

To return to our experiment. It has been seen that the air in the retort underwent certain changes during its contact with the blood; the next point was to determine the number and identity of the substances by which these changes were brought about. With this object, the organic constituents of blood were successively subjected to the action of air, and treated by the process adopted in the case of the blood itself; it will presently be shown with what success. First, we repeated Scherer's* experiment on fibrin, taking care, however, not to let that substance remain in contact with the air above twenty-four hours. This precaution was adopted in order to obviate the objection raised by Magnus against Scherer's experiment—namely, that the fibrin had been allowed to remain in contact with the air until putrefaction had set in, and that consequently the changes which the air confined along with the fibrin was found to have undergone, were attributable to decomposition or putrefaction, rather than to any chemical action taking place between fresh fibrin and oxygen in the atmospheric air.

In our experiment, the fibrin, after having been in contact with air during twenty-four hours, appeared as fresh as at the moment of its first introduction into the retort; not the smallest symptom of putrefaction could be detected. The result of this experiment may therefore be con-

* Annalen der Pharm., Band xl. § 1.

sidered conclusive. The result corroborated the opinion expressed by Scherer, that fibrin has the power of absorbing oxygen and giving out carbonic acid,* as the following experiment shows.

One volume of fibrin (fourteen grains), slightly moistened with water, was placed in the retort along with eight volumes of air, kept at a temperature of from 20° to 25° per cent., and occasionally shaken during a period of twenty-four hours. At the expiration of that time, the gas in the retort was analysed, and found to have the following composition in 100 parts:—

Oxygen . . .	6·81	} Total oxygen, 17·98.
Carbonic acid .	11·17	
Nitrogen . . .	82·02	
	100·00	

Thus showing that the fibrin itself, exactly like the blood, takes up a certain quantity of oxygen, and gives off a stated amount of carbon in combination with oxygen, in the form of carbonic acid. The separation of the carbon may have occurred in two ways—either as carbonic acid, or (and this latter we consider the most probable supposition) as free carbon, which in its nascent state combined with some of oxygen present in the air to form carbonic acid.

Our next experiments were made on a substance which plays a still more important part than fibrin, at least, if we are to judge from its universal distribution throughout the animal economy. We allude, of course, to albumen. As this substance cannot be extracted from the blood in a pure and uncoagulated state, we were forced to avail ourselves of a substitute easily obtained in a nearly pure and liquid condition, and supposed to bear the greatest resemblance to the albumen of the blood—the white of the hen's egg. When a certain amount of albumen of fresh eggs was well agitated with renewed portions of air, and kept in contact during fourteen hours, at a temperature of about 26° per cent., with 100 per cent. of ordinary atmospheric air, 100 parts of the gas yielded on analysis—

Oxygen . . .	17·05	} Total oxygen, 19·14
Carbonic acid .	2·09	
Nitrogen . . .	80·86	
	100·00	

Proving, in common with the experiments on the blood and on fibrin, that albumen also possesses the property of chemically combining with oxygen, and giving off carbonic acid. The proportion of oxygen which has disappeared, and that of carbonic acid which has been formed, are somewhat less than in the case of the fibrin; but this difference would have been much less marked if the albumen had been allowed to stand for twenty-four instead of only fourteen hours in contact with the air.

* It was long ago pointed out by Spallanzani, as well as by Aldini, that muscular substance and several other of the animal tissues possess the property of absorbing oxygen and giving out carbonic acid. Recently these experiments have been verified by Mr. George Liebig, at least in as far as regards muscle; and Valentin has demonstrated how the lower extremities of a frog, when freed from the skin, absorb oxygen and exhale carbonic acid in definite proportion, as long as muscular irritability continues; and that when muscular irritability ceases, an important alteration takes place in the amount of the gases interchanged. (Archiv der Phys. Heilkunde, Band xiv. p. 2, 1855; Arch. Gén. de Médecine, Paris, Mai, 1856.)

As the serum of blood contains a large quantity of albumen, while, on the other hand, all the fibrin and blood-corpuscles occur in the coagulum, we instituted some comparative experiments between these two, and cite the following as the average of the results. Equal portions of coagulum and of serum, after repeated agitation with atmospheric air, were introduced into retorts, each with 100 per cent. of ordinary air, and during six hours kept at a temperature of 36° per cent. When that time had elapsed, the gas in each retort was subjected to analysis, and the results were found to be—

Coagulum—

Oxygen . . .	8·57	Total oxygen, 15·86
Carbonic acid .	7·29	
Nitrogen . . .	84·14	
	100·00	

Serum—

Oxygen . . .	16·74	Total oxygen, 19·04
Carbonic acid .	2·30	
Nitrogen . . .	80·96	
	100·00	

The difference in the results of these two experiments is very striking. The coagulum, which contained the fibrin and blood-corpuscles, wherein is a quantity of albumen, appears to have exerted a more powerful chemical action upon the oxygen of the atmospheric air than the colourless aqueous serum, which contains only albumen. This difference is perhaps more marked in consequence of the greater facility of saturating the serum with oxygen than in the case of the coagulum, which, even when pounded as fine as possible, is not in such minute division as the albumen dissolved in the serum. A greater difficulty is consequently experienced in saturating every part of it with oxygen, during its agitation with atmospheric air, before being introduced into the retort, than is found to occur with the albuminous serum. This is, however, in some degree, compensated by the circumstance that it is more difficult to mix the coagulum than the serum with the enclosed air in the retort. The effect of imperfect saturation may be looked upon as in a great measure counterbalanced by this last-mentioned fact. The difference in the amounts of air transformed in these two experiments was, however, so very great, that we felt exceedingly anxious to discover the source whence it proceeded. Under the impression that the presence of the hæmatin might have contributed to produce this result, some experiments were made on the latter substance, in the hope of obtaining a solution of the difficulty. It is now many years since a very distinguished French chemist, Monsieur Chevreuil, drew attention to the fact that sundry colouring matters used in the process of dyeing possessed the property of absorbing oxygen and giving out carbonic acid. Knowing that urohæmatin likewise possessed this property, as Scherer first observed, and also that Lehmann had obtained very similar results with his blood-crystals, we resolved on trying the effects of the pure colouring matter of the blood upon air. It was necessary, however, in the experiments on this substance, somewhat to modify the method of procedure adopted in those previously detailed,

as we possessed but a small quantity of pure hæmatin.* We cite the following experiment on this substance, inasmuch as it furnished the most striking results.

A portion of hæmatin, slightly moistened with water, was introduced into a glass apparatus furnished with a larger and a smaller bulb. The proportion of hæmatin to that of air was one volume of the former to about 1000 volumes of the latter; but as the measurement was made simply by the eye, this statement can only be considered as approximative. After the introduction of the hæmatin, the glass vessel was hermetically sealed, and hung up in the window exposed to the light during a period of nearly four months. The gas in the apparatus was then analysed, and contained in 100 parts—

Oxygen . . .	16·01	Total oxygen, 19·81
Carbonic acid .	3·80	
Nitrogen . . .	80·19	
	100·00	

Corroborating the different results obtained by previous observers on various colouring matters.

The pure colouring principle of the blood, therefore, by exposure to air, gives off carbonic acid gas, and becomes oxidised in two ways—firstly, by a loss of carbon; secondly, by direct combination with oxygen. These changes, moreover, take place to an enormous extent; for had equal volumes of blood-hæmatin and air, instead of only 1 volume of the former to 1000 of the latter, been employed, we may presume that a corresponding increase would have taken place in the amount of oxygen combined, and of carbonic acid disengaged; nor would it have been necessary to wait so long for a perceptible change in the composition of the atmospheric air confined in the retort. We were much gratified with the result of this experiment, as it furnishes additional evidence of the correctness of the theory we hazarded two years ago,† by which a more important office in the function of respiration than they before had been considered to possess, was assigned to the colouring matters of the animal and vegetable economy.

It must be obvious to the most cursory observer, that a close connexion exists between the distribution of the colouring matters in the animal and vegetable kingdoms, and the organs of respiration. In the animal body, for example, where the circulating liquid is the medium for the absorption of oxygen and the exhalation of carbonic acid, this liquid is rich in colouring principle. In plants, on the other hand, which are destitute of any proper circulatory apparatus, the colouring matter is found to be confined exclusively to the respiratory organs themselves (leaves, &c.). So indisputable, indeed, is the intimate connexion between the colouring matters and the respiratory process, that those parts of the plant in which no colouring matter is deposited, or which, on exposure to air, do not become coloured, entirely lack the property of absorbing

* This hæmatin we had prepared from ox-blood, according to the method of Verdeil, while working in his laboratory, three years before.

† Ueber Urohæmatin und seine Verbindungen mit Animalischem Harz. Verhand. der physik. Medicin. Gesell. zu Würzburg. 1854.

oxygen and disengaging carbonic acid. We do not mean to assert that the colouring matters are the only agents by which the function of respiration is accomplished in the animal and vegetable organisms; for our own experiments have demonstrated that other substances are not destitute of the property of absorbing oxygen and exhaling carbonic acid. But in our own mind, we cannot help associating the principal office of the colouring matters with the function of respiration; for, in our opinion, the blood-corpuscles, the recognised transporters of the respired gases, owe this property principally to the presence of the colouring matters, and not, as Liebig asserts, to the iron they contain.

It has been shown that time has a most important influence on the amount of oxygen absorbed, and the quantity of carbonic acid exhaled, by the blood; we shall now point out how temperature, as well as the presence of foreign substances—especially those having poisonous properties, exerts an equal power over the chemical changes induced by the presence of oxygen in the circulating liquid.

When two portions of the blood which had been thoroughly saturated with oxygen were confined with 100 per cent. of ordinary air, during twenty-four hours, one in a room of moderate temperature, the other in an ice cellar, the gas from the former yielded in 100 parts:

Oxygen	10·42
Carbonic acid	5·05
Nitrogen	84·53
	100·00

The latter, which had been kept surrounded by ice:

Oxygen	17·43
Carbonic acid	00·59
Nitrogen	81·98
	100·00

We have in these numbers a proof that a certain amount of heat is necessary for the development of the chemical changes.

To show the influence of foreign substances, we may cite the case of chloroform. When two equal portions of the same ox-blood, treated in a precisely similar way, were kept during twenty-four hours in contact with 100 per cent. of atmospheric air, the one in its normal state, the other mixed with three drops of chloroform, the analysis of the gas confined with the pure blood yielded, as already cited,

Oxygen	10·42
Carbonic acid	5·05
Nitrogen	84·53
	100·00

while that to which the chloroform had been added, yielded,

Oxygen	18·38
Carbonic acid	1·88
Nitrogen	79·74
	100·00

This proves that chloroform possesses the property of diminishing the power of the organic constituents of the blood to unite with oxygen, and give off carbonic acid.* Alcohol, as will be seen from the following example, has a similar power, although in a less degree. A certain amount of cow's-blood was confined with the same quantity of air, and treated in exactly the same manner as in the foregoing cases. After the expiration of twenty-four hours, the analysis of the gas confined with the pure blood yielded,

Oxygen	10·23
Carbonic acid	3·31
Nitrogen	85·46
	100·00

While that confined with blood, to which had been added 5 per cent. of alcohol, gave,

Oxygen	16·19
Carbonic acid	2·36
Nitrogen	81·45
	100·00

Many other poisonous substances possess the same power. The action of strychnine and brucine in this respect we have already pointed out,† and on an early occasion we intend offering the results of our analysis, proving that an exactly similar power is exerted by hydrocyanic acid, nicotin, ether, quinine, morphine, &c.

To conclude.—The foregoing experiments prove—

Firstly, That blood has the property of chemically combining with the respired oxygen.

Secondly, That the coagulum, the serum, fibrin, albumen, and hæmatin are among the substances possessing this property.

Thirdly, That these substances not only become oxidised, but also yield carbonic acid gas.

Fourthly, That time, temperature, and the presence of foreign matters, are among the agents which modify these changes.

While, however, we consider that our investigations have supplied what was wanting in direct evidence of the untenableness of Magnus's theory, we are not unconscious of the fact that the labours of some future inquirer may in due course overturn ours. So rapid is the advance of physiological science, that the theory regarded as true to-day, may be recognised as false to-morrow. The facts, however, on which the theory is based, if rightly observed, remain unaltered and unalterable.

G. Harley.

* Dr. C. F. Jackson on analysing the blood in the case of a woman poisoned by chloroform, found that the chloroform had become changed into formic acid, which he separated by distillation. The blood, by combining with the chlorine, had lost the power of coagulating and becoming red on exposure to the oxygen of the air. From the latter observation it appears that chloroform exerts the same power over the blood in as it does out of the body. See Comptes Rendus, Feb. 25th, 1856.

† Lancet, June 7th and 14th, 1856.

Review XII.

The Trial of William Palmer, at the Central Criminal Court, Old Bailey, London, May 14, *and following days*, 1856.

It was in the latter part of last November, that public attention was first drawn to the case which furnishes the subject of the present article, by an announcement in a local paper, that a young man named William Cook had died at Rugeley, under circumstances so suspicious as to call for an inquest; and that, as the result of that inquest, a verdict of Wilful Murder had been returned against William Palmer, a surgeon of that place, who was accordingly committed for trial. Little did any one then suspect, that the drama thus opened would speedily attract the attention, not only of all England, but we may almost say of the whole civilized world. Yet it was not long ere the single crime was amplified into a catalogue so monstrous, as to cause William Palmer to be ranked among the most terrible poisoners of any age or country; and the particular case which had given occasion to the investigation, thus acquired an importance in social estimation, which seemed almost to rival that of the great public events which were contemporaneously enacting.

How far this catalogue is correct, whether it included less or more than the truth, is known to no mortal being. For ourselves, it will be enough to state that the evidence brought out on the inquest as to Palmer's complicity in his wife's death seems to us morally conclusive; and that though we are not so sure that he gave his brother the *coup de grâce* by any special dose of poison, yet it is quite clear to our minds that he was doing his best to compass his death by the slower process of habitual intoxication, and that he would have finished him more speedily had he found occasion to do so. On these points there has been a public investigation and a public verdict, and we are therefore justified in expressing a decided opinion. In regard to the cases which have not been so gone into, we have nothing but hearsay evidence for our basis; and all that we can say of this is, that taking it in its connexion with the proved cases, it affords a strong presumption of his guilt in several more.

A juridical investigation of so remarkable a character ought not, as it seems to us, to pass without some special notice on our part; and there are two subjects on which we think that some comment is specially called for, quite independently of the important questions of chemical toxicology with which we do not at present propose to grapple.

The first and principal topic to which we are desirous of directing the attention of our readers in connexion with this remarkable trial, is the *probative value of circumstantial evidence.* This, we are prepared to affirm, *may* rise to the full force of direct proof; and when the uncertainty attending on all *testimony* is taken into the account, we hold that even a stronger conviction may be afforded by circumstantial than by direct evidence. We doubt if the annals of criminal jurisprudence afford any more remarkable example of the overwhelming force which arises from the concurrence of a number of independent probabilities, of which every one, taken by itself, might bear some other explanation. To com-

plete the proof that strychnine administered by Palmer to Cook was the cause of the death of the latter, to the satisfaction of every unprejudiced mind capable of estimating the circumstances, but one point was wanting,—the discovery of the poison in the body of the deceased. Had this been detected, the only ground of cavil would have been removed; and not even those most interested in procuring the prisoner's acquittal, could have had the hardihood to protest his innocence. As it was, even in spite of this unfortunate deficiency, and of the attempts which were perseveringly made to convert this absence of an important probative fact into a positive *dis*proof, which we shall presently show that (under the circumstances) it had no right to be designated, we doubt if the slightest doubt of Palmer's guilt existed in the mind of any bystander who steadily followed the course of the proceedings, and carefully noted the important points of the case, save in the case of such as (from whatever motive) did not *wish* to believe him guilty, and allowed their feelings to warp their judgment. And notwithstanding this serious drawback, we hold that the evidence against him was really much stronger than if, without any adequate confirmatory testimony from independent sources, a single witness had deposed to having actually seen Palmer mix strychnia with the pills which he administered to his unfortunate victim, and had thus furnished what would be commonly regarded as the only *direct* proof of his guilt.

Before proceeding to justify our assertion by a reference to the principal features of this particular case, we shall beg the attention of our readers to a few general considerations as to the essential nature of *proof*, and the probative force of different kinds of evidence. In these we shall present, under a somewhat different form and with different illustrations, a principle on which we enlarged on a previous occasion (vol. vi. p. 1); and on which we deem it the more important to insist, since it is that on which the Physician and the Chemist are constantly acting without any definite appreciation of its value, and also because, although it is not recognised (that we are aware of) in any of our formal treatises on logic, we are able to appeal for confirmation of its correctness to some of the most eminent logicians of the day, who have given their full sanction to the views we formerly advanced.

It may seem no better than a truism to say, that the probative value of all evidence depends upon the strength of the conviction which it is capable of producing in the human mind, as ordinarily constituted; yet upon this truism the whole science of evidence rests. We can no more define what constitutes an intellectual assent to a certain proposition, than we can define what constitutes the sense of beauty that is awakened by a certain external object, or explain why certain objects produce either, whilst others do not. There is a state of mind which varies in intensity from *absolute certainty* down to a mere *preponderating belief*. In the first case the mind cannot entertain, as even possibly true, any other hypothesis than that to which it gives its assent; whilst in the second, it merely gives a preference to one hypothesis as more likely to be true than another, or perhaps than several others, each of which may possibly be true, while some of them may appear not altogether improbable. Conversely, there are certain ideas, or combinations of ideas, which, when present to the mind, excite in it the state in question, either as to themselves, or as to

some other idea, of which they are said to furnish the evidence. The degree of intensity in which that state shall be excited, in any particular case, doubtless depends, in some degree, upon the antecedent training of the individual. What is all but demonstrative evidence to one, may be entirely destitute of probative force to another. The Chemist can express a positive assurance of the presence of a substance whose characteristic reactions he has witnessed, when an unscientific bystander can only bear testimony to having seen a succession of phenomena which are to him altogether meaningless. Nay more, one chemist (as we shall presently show) will feel himself justified in expressing a positive assurance, whilst another with equal, or even greater chemical knowledge, will only draw a probable inference from the very same data, because his previous logical training has not been such as to lead him to recognise the probative value of those data. So, again, the Toxicologist sees the unmistakeable proof of the action of a corrosive poison, in morbid appearances which, to the mind of an ordinary observer, would possess no such significance. There are, however, certain kinds of evidence, which make their appeal, not to the individual consciousness of the expert, but to the common sense of mankind; and it is only of that assurance which is felt by every ordinarily-constituted mind, as to the ideas which these evidentiary facts call-forth in it, that *absolute certainty* can be rightly predicated. And it is further to be noticed, that this absolute certainty can only be felt regarding certain abstract ideas which are of the mind's own coinage; since, as soon as we begin to rely upon the "evidence of our senses," with regard to matters external to it, elements of uncertainty begin to creep in, which weaken the probative value of even the best-observed facts; and when, in addition, we have to rely for our knowledge of the facts, not upon the evidence of *our own* senses, but upon the testimony of another as to the impression made upon *his*, that element of uncertainty is still further augmented. Thus no one who duly weighs the fallacies of sense to which every human being is liable, as shown in the diversities in the account given of the very same occurrence by a number of truth-telling witnesses, would venture to say that he has the same positive certainty respecting any event that has fallen-out under his own eyes, which he entertains in regard to the axiom that "if equals be added to equals, the sums will be equal." He would feel that he *may be* mistaken (however improbable he may consider it that he should be) respecting the one; but that he *cannot* be mistaken respecting the other. He can conceive something else to have really occurred, than what he believes himself to have seen or felt; but he cannot conceive that if equals be added to equals, their sums should be anything else than equal. Again, whatever confidence we may entertain in the veracity of another, even to the extent of staking upon it everything we ourselves hold dearest, we cannot predicate of any fellow-mortal the absolute impossibility that his testimony should be intentionally deceptive; and further, we are of course more ready to believe that *he* may be *self*-deceived, than to admit that *we* can be. Hence, even with regard to the data on which every fabric of proof has to be built-up, there is a fundamental distinction to be drawn, between those that are necessarily or absolutely true, and those that are only contingently true; and it will be found that under the former category can only be ranged

our conviction of certain abstract truths, which (like the axioms of Geometry) we believe, because we cannot help doing so; whilst the latter includes every event of our outward life, even such as we are accustomed to speak-of with the most positive assurance.

The same distinction applies to every inference based upon these fundamental data. There are certain inferences which approve themselves to every ordinarily-constructed mind as necessarily true, because it cannot conceive either the contrary, or anything else, to be in the remotest degree possible. Hence every one of these inferences becomes a secure basis for the next; and thus, however prolonged our chain of reasoning may be, if every link possess this absolute tenacity, the conclusion is so firmly held-on to the premises, that we can no more admit any possible doubt of the former, than we can of the latter. Of this kind of reasoning, mathematical demonstration is the typical example; affording, in fact, almost the only case, in which—the premises and the inferences being alike *necessarily* true to our minds—the conclusion becomes forced upon us as a certainty which cannot be evaded. To any kind of reasoning, indeed, in which the requirements of logic are absolutely satisfied, the term *demonstrative* is commonly applied. But it very commonly happens, that whilst the deductions are soundly made, the data from which they are inferred contain some elements of uncertainty; or, to state the matter in the ordinary language of the science, the syllogism may be perfectly good, but the conclusion is vitiated by some want of soundness in one or both of the premises; hence to speak of such a proposition as demonstrated, is a misuse of terms, since this designation should be reserved for cases in which both the data and the inferences deduced from them are alike free from the possibility of fallacy.

Now, to show how little the most approved verities of science can be considered as rising to this degree of certainty, let us refer to Chemistry for illustrations.

If we develope a blue colour in some vegetable structure by the contact of iodine, we feel as positive an assurance as that science can afford, that *starch* is present. Or, supposing that to a suspected fluid we add a little hydrochloric acid and gold-leaf, and that afterwards, on dropping-in some protochloride of tin, we get a purple precipitate, we feel an equally strong assurance that nitric acid was present. But in the first case, our conviction is based on the assumption that iodine produces a blue colour with no other substance than starch; and however justifiable such an assumption may be, as accordant with all we at present know, it is in the nature of things unquestionable that some other substance *may* be discovered which shall give the very same reaction. Such, indeed, happened in the case of meconic acid; the deep red which that substance gives with the perchloride of iron having been considered an infallible test of its presence, until it was discovered that sulphocyanogen gives a colour with the same reagent, only to be distinguished from that given by meconic acid by the circumstance that the one is removable by a solution of bichloride of mercury, whilst the other is not. In the second case our conviction is based on two such assumptions, each of which contains the same element of uncertainty; for, although we know nothing save gold in solution, which will give a purple precipitate with protochloride of tin,

still we cannot but admit the possibility that there *may be* something else; and although we know nothing save nitric acid, which, when added to hydrochloric acid, shall dissolve gold, we must again allow that there *may be.*

We are far from wishing to encourage a spirit of undue scepticism. All we are desirous of showing is, that even what we are accustomed to consider to be the most positive and direct evidence of objective realities, is subject to possibilities of fallacy, which reduce them from the category of *necessary* to that of *contingent* truths. They are still *realities* to us; and the common sense of mankind feels itself fully justified in acting on them, until their remotely-possible fallacy shall have been shown to be a real one. What we have now to urge, is that *the same degree* of conviction may be rightly attained from a concurrence of independent probabilities, each by itself (it may be) but a slight one, as from the evidence to which we are accustomed to attach the highest probative value. The test of the value of any assumption really lies (as we have seen) in the answer to the question, Can the contrary, or anything else, be conceived to be possible or probable? And we shall show that, when tried by this test, the conclusion to which circumstantial evidence points, may present as high a value as that afforded by the evidence we should consider the most "direct."—We shall again have recourse to Chemistry for an illustration. We have a suspected fluid, in which, on adding a few drops of ammoniated nitrate of silver, a yellow precipitate is thrown down. Again, on causing the flame of hydrogen gas disengaged in the same fluid to impinge on a piece of porcelain, a metallic crust is formed. We may hence infer the presence of arsenic with a high probability, though not with the degree of certainty that the case admits of. The inference from the first fact is by itself of little probative value; since there are various substances beside arsenic, which will give a yellow precipitate with nitrate of silver. And as antimony, like arsenic, will combine with hydrogen disengaged in its solution, and will form a metallic crust on the porcelain against which its flame is projected, there is an even chance that antimony may be the substance. Still if, from the same solution, we get *both* these results, we feel a strong probability that they are due to arsenic; since there is no other substance known to us, which shall give both the yellow precipitate with nitrate of silver, and the metallic crust when reduced from hydrogen. Of the various substances which give the former reaction, there is not one save arsenic that will give the latter; and antimony which alone, besides arsenic, will give the latter, does not give the former. Hence if we can be assured that it is *one and the same substance* that produces both reactions, we may feel the same strength of conviction of that substance being arsenic, and nothing else than arsenic, which we have of starch being present when a blue is produced by iodine. And we extend this principle to the liquid tests, which, if looked-at separately, are rightly considered as so fallacious that no reliance can be placed upon them. For the green precipitate given by arsenic with ammoniated sulphate of copper, and the orange-yellow with sulphuretted hydrogen, are, like the yellow with nitrate of silver, so far imitable by the reactions of other substances, that neither, taken by itself, can be regarded as affording more than a faint probability of the presence of arsenic. But as no substance known to us, save arsenic, which gives any one of these precipitates, will give either of

the two others, the only hypothesis we can entertain as possible, save that of the presence of arsenic in the liquid which exhibits these reactions, is that of a mixture of the substances which shall give each separately. This of course involves the highest scientific improbability, though not an impossibility; but even this chance of error may be excluded, by taking experimental means to show that it is *one and the same substance* which produces all the reactions.—Thus, to go back to the first case, if the metallic crust be volatilized in contact with air, it will rise in a white vapour which will condense again in octohedral crystals; and if these be dissolved, the solution will give the characteristic reactions with the liquid tests. Thus the hypothesis of antimony as the source of the metallic crust is excluded, first by the volatility of that crust, and second, by the reactions of the product of its volatilization; and the hypothesis of phosphoric acid, or any other substance than arsenic, as the source of the yellow precipitate with nitrate of silver, is excluded by the previous reduction of the precipitant to the metallic form from a state of gaseous combination. So, again, from the use of the liquid tests without any reduction of the metal, we hold that an equally high degree of certainty may be obtained, by taking care to exclude the hypothesis of mixture; and this can be readily effected by causing *the same substance* to give each reaction, as may easily be done by separating and redissolving the precipitate given by one of these tests, and then treating the solution with the other two. To any one who is capable of appreciating the probative force of evidence, such a use of the liquid tests ought to be quite as satisfactory as the reduction of the metal; since the production of the three, or even of any two, of their characteristic reactions, cannot be accounted-for (according to the present state of our knowledge of chemistry) on any other hypothesis than the presence of arsenic.

Thus, then, whilst the Chemist can recognise one substance by a single test, which he considers as affording direct evidence of its presence, *because he knows of no other substance which will give the same reaction*, and therefore cannot attribute that reaction to any other cause of which he is cognizant, he may recognize another with equal certainty by the use of two or more tests, each of which may be utterly inefficient *per se*, but of which the *aggregate* affords evidence no less direct than that in which he justly confides in the preceding case, *because he knows of no other substance which will give the same combination or succession of reactions.* Practically, our principle is admitted and acted-on by every Chemist; but that its true range is but little understood, is evident from the very slight estimation in which the liquid tests for arsenic are held when taken alone. Doubtless in a criminal procedure it is quite right to require every proof that science can afford; and the reduction-test can now be so easily applied, that none but the most ignorant and bungling operator could fail in its use. But cases have happened, and may happen again, in which the liquid tests having been used with success, but the arsenic not having been reduced, the presence of arsenic has been considered as *not proven*, on the ground that as each of the liquid tests is *per se* fallacious, even the combined result of all has no claim to afford such an assurance as is needed for conviction. Such assurance will be felt, however, in this and similar cases, by any competent inquirer who shall put

to himself this question,—"Is there any other substance than the one supposed, which fulfils *all the conditions* of the case?" If there be not, the fact is as clear as if the presence of that substance had been demonstrated (to use the current phrase) by the most conclusive single test.

Precisely the same method of reasoning is continually had recourse to in Medicine, especially in diagnosis; and its validity is practically recognised, though without, as we believe, a clear comprehension of its *rationale.* A certain peculiar group of symptoms is presented by a patient, from which the Physician endeavours to determine the malady under which he labours. Every one of these symptoms, taken by itself, may be conformable to half a dozen different diseases, and may afford no special probability in regard to either one of them. But of these diseases, a large proportion may be at once excluded by their incompatibility with other symptoms; and there may be only two or three, which, on a general view of the case, appear to give any rational account of its phenomena. And among these two or three, a choice may very possibly be made at last, by attention to minute details which did not enter into the previous survey, these details being found inconsistent with all the hypotheses but one. And although, as in Chemistry, we are bounded by the imperfection of *all* our knowledge of the external world, yet it may be safely said that a diagnosis based on the concurrence of a number of separate probabilities, *the aggregate of which is compatible with only one conceivable hypothesis*, is really just as secure, and may be considered as just as much demonstrated, as a diagnosis that rests on the most satisfactory pathognomonic sign that nosology can present. As in the second case no other disease than one can be conceived to exist, when its single pathognomonic sign is unmistakeably present, so in the first no other disease than one can be conceived to exist, when a combination of symptoms is present which is consistent with *its* existence, whilst some symptom or other is incompatible with the idea of *any other* malady with which we are acquainted.

Hence in considering the probative value of a mass of circumstantial evidence, such as was adduced in the trial of Palmer, we are not to be decided by the separate weight of every isolated fact, but by that of the aggregate; and not merely by the sum of the individual probabilities, but by the consideration of the force which they acquire from their mutual connexion. Thus every fact adduced against Palmer, might possibly have been explained-away on the hypothesis of his innocence; the aggregate of these facts, taken separately, might not constitute any overwhelming proof of his guilt, unless the several explanations given of them were inconsistent with one another; but it is when they are considered in their mutual relations, that they acquire a probative force, which, in our apprehension, nothing can withstand. Without going into such detail as our readers' knowledge of the circumstances would render superfluous, we shall briefly indicate those points which seem to us to possess the greatest cogency.

Let us first take the fundamental question, whether Cook died from the effects of strychnine administered to him. The medical evidence of poisoning is usually referable to three heads; that of the symptoms, that of the post-mortem appearances, and that of the presence of the poison

in the body. In the case of poisoning by strychnine, however, no reliable evidence is furnished by the post-mortem appearances; and in this individual case, no affirmative evidence was supplied by chemical analysis. But the evidence of symptoms is peculiarly strong. We know of no cases, in which, from the history of the symptoms alone, a more certain inference can be drawn as to their origin, than it can be in such as exhibit the phenomena of strychnine-poisoning in its most characteristic form. Putting aside all quibbles as to the minor variations which a comparison of the history of almost any two individual cases would present, the fact stands-out strongly, that death from strychnia is in its essential nature death from tetanus. Now tetanus is a disease so peculiar, as to be altogether unmistakeable for anything else; and moreover it is one whose different forms are marked-out with singular clearness by the course they take. Thus idiopathic tetanus, a disease so rare that many surgeons in extensive practice have never seen a case of it, is distinguished by the gradual nature of its access and by its protracted duration; traumatic tetanus is recognised by the previous existence of some wound or sore, whose situation or condition renders it liable (as experience shows) to give rise to this disease, and by the more rapid course of the malady, which sometimes terminates fatally in this country within twelve or fifteen hours, and *is said* to end occasionally in hot climates within an hour; while hysteric tetanus, to which in the suddenness and violence of its access the tetanus of strychnia bears the closest resemblance, is diagnosed by the previous hysterical diathesis of the subject, who is almost invariably a female. The tetanus of which Cook died—for to call it anything else would be a piece of disingenuous casuistry—was certainly not idiopathic, for the attack did not last above twenty minutes. It was certainly not traumatic, for neither did any wound or sore exist that could afford the requisite source of irritation, nor could any parallel be adduced from reliable experience to show that, even had such a possibility existed, the fatal event could have supervened within so short a time from natural causes. It was suggested on the defence, that the non-fatal attack on Monday evening and the fatal attack on Tuesday evening were parts of the same malady, the duration of which was thus about twenty-four hours; but this hypothesis was quite disposed-of by its entire inconsistency with the uniform history of traumatic tetanus, the idea of any such remission, after the paroxysms had once become violent, being utterly opposed to experience. We should therefore be obliged to treat the first and the second attacks as two separate and independent maladies, unless they could be shown to be possibly attributable to an *hysterical* condition of the system (in which, within our own experience, tetanic attacks have thus suddenly come-on at long intervals, with complete remission between); but of this there was not the faintest shadow of a probability.

But although the inference that the tetanus of which Cook died was attributable to administration of strychnia, mainly rests upon the incompatibility of the symptoms with any form of natural disease known to medical experience, this is by no means the only evidentiary fact bearing on the question; and it is a pure sophism to urge, as Palmer's defenders have done, that the charge against him, resting on the evidence of symptoms alone, is sufficiently disproved by the non-detection of the

poison in the body. We are not going to enter into any discussion of this part of the case, nor to inquire whether Drs. Taylor and Rees ought to have detected strychnia, if it existed, and were bunglers for not having done so. Without setting ourselves up as the apologists for those gentlemen, however, we think it right to point out the essential difference between the position of one analytical chemist, who simply receives a very limited amount of material, in which he is expected to search for every conceivable poison without the slightest clue from symptoms or post-mortem appearances as to the direction most likely to be fruitful, and that of another who applies all his skill and all the resources of his art to the detection of some one particular substance for which he is eagerly looking. It would have been no reflection on Drs. Taylor and Rees that they did not find strychnine in the stomach, even had they received the entire contents of the viscus, instead of a small fraction of them; for it may very probably have not been present there. And although it certainly ought to have been looked-for in the blood, after the evidence of symptoms became known; yet it was no fault of Drs. Taylor and Rees that it was not, since they never received any blood for analysis. The absence of any chemical evidence as to the presence of strychnine, is simply a deficiency of proof of the affirmative proposition, and affords no presumption whatever to the contrary. For not only was it stated by Drs. Taylor and Rees, that by the use of the very same methods they had failed to detect strychnia in the bodies of some of the animals which they had poisoned by that substance, but the like admission had to be made by some of the witnesses for the defence. To urge, therefore, that Cook did not die of strychnine, because none was found in his body, is tantamount to urging the monstrous proposition, that of the rabbits to which strychnine had been administered, and which died with all the symptoms of strychnine-poisoning, those were not killed by it, in whose bodies it could not be found after death.

But the want of full proof on this point, is most singularly supplied by the evidence as to the administration of the strychnine. The sufferer had two distinct attacks of tetanus, of a kind not capable of being accounted-for in any other way than on the toxic hypothesis. Each of these comes-on within an hour after Cook had taken pills, with which it was distinctly proved that Palmer had, on the first occasion, the opportunity of tampering, and which on the second occasion were brought by himself. Scarcely an hour before the first occasion, Palmer had purchased strychnine, if the witness Newton is to be believed; in the course of the next day, between the first and second occasions, he made another purchase of strychnine at a different shop. Now, whatever doubt may be considered to rest on the testimony of Newton, taken by itself, from the circumstance that this part of it was kept-back by him in the first instance, we hold that the *mutual bearing of these four asserted facts*—the first purchase of strychnine, immediately followed by the first tetanic attack, and the second purchase of strychnine, followed by another tetanic attack at the earliest opportunity at which the dose could be administered—is such as not only to give them a probative force, taken in combination with each other, vastly greater than the sum of their independent or separate values, but goes far to remove any doubts that might be

entertained on the score of possible errors of testimony. For it is obvious that the second purchase of strychnine confirms the first; and that the relation between the fatal attack of tetanus and the second purchase becomes an almost positive certainty, when taken in connexion with the like occurrences of the preceding night. Each circumstance so confirms and is confirmed by the rest, that the whole may be likened to a framework of which the several parts are so braced together as to afford a degree of strength equal to that of a solid mass, its weakest parts being so secured by their connexion with the strongest, as not to be in any more danger of giving way than they are.

In many cases of poisoning, the question whether the death of the deceased was attributable to poison, and the question who administered the poison, are entirely distinct. Here they are not so. The evidence that Cook died from strychnia is partly furnished by the fact that Palmer had purchased strychnia immediately before; and not on one occasion only, but on two. The evidence that the poison was administered by Palmer rather than by any one else, is partly based on the fact of his possession of it without being able to give any probable account of his object in purchasing it, partly on various little circumstances connecting him with the act—such as his going home for more than half an hour on the Tuesday evening, after having received the pills from Mr. Bamford—partly on the uncontradicted proof of the previous administration of antimony, and partly on the collateral circumstances which showed a motive for the act, which, to a man of Palmer's nature, was of overwhelming force. If we could stop to analyse the first of these sets of evidentiary facts, we could here again show how forcible may be the proof derived from a number of minute and insignificant circumstances, when they are considered in their bearing upon the one simple hypothesis in question, and how numerous and complicated must be the inventions which would be required to explain them, if this hypothesis be not admitted. But as this could only be done by such a minute detail as would be foreign to our present purpose, we shall pass on to the second portion of the case—namely, the previous administration of antimony to the deceased. This is an almost unprecedented feature in the history of juridical inquiry; and presents a most remarkable significance, when taken in connexion on the one hand with the subsequent administration of strychnia, and on the other with the very peculiar nature of the collateral transactions. No one, we should suppose, who considers in their connexion all the facts of this preliminary part of the case—the repeated and urgent vomiting of Cook, unconnected with any other symptoms of disease that could reasonably account for it, the repeated recurrence of this vomiting shortly after broth and drinks had been taken, the occurrence of the same disorder in one of the attendants who partook of one of these articles, and the proved presence of antimony in the deceased's body, can entertain any doubt that Cook's vomiting was due to antimony then administered. And when not only the ordinary opportunities which Palmer had of introducing any such substance into Cook's food or drink, but the peculiar anxiety which he showed that these articles should pass through his hands, are taken in connexion with the observation by Mrs. Brooks, who caught him almost in the fact of

mixing something with the brandy and water which shortly afterwards brought on vomiting; when, also, we see the very extraordinary advantage that was taken by Palmer of his victim's illness, to get into his own possession the large sum of money which Cook was to receive; no reasonable doubt can be entertained, that Palmer's was the hand by which the antimony was administered. His defenders, indeed, seemed to be quite aware that it would be a waste of energy to attempt to impugn this part of the case; they scarcely urge anything else than a denial that it has any bearing on the subsequent occurrences.

This bearing, as we have just said, is twofold. Supposing that Palmer had made up his mind to poison Cook with strychnia, as a substance of potent and certain operation, capable of being employed with but little chance of detection, the first question would be, how he should administer it. The intense bitterness of strychnia would effectually reveal its presence in any article of food or drink; the only mode of getting Cook to take it, therefore, would be to make it up into pills. But how could Cook be brought to take these pills, unless he were first made ill in some other mode? The administration of antimony furnished a ready means of bringing this about; and from the time the poor victim was once prostrated by sickness, he was within the clutches of his destroyer. But further, it would not suit Palmer's purpose to kill Cook outright. If he had poisoned him when he first made him ill, he would probably have never had it in his power to get in the debts due to Cook, and to apply these to his own purposes. The Monday on the evening of which the first attack of tetanus came on, was settling-day at Tattersall's. Cook would naturally have gone to town to receive the large amounts due to him; but he is laid-up by illness, and his friend Palmer goes in his stead, represents himself as Cook's agent, collects the bets due to him, and then, instead of bringing home the money to his client, applies it to stopping the mouths of his own most urgent creditors. Having succeeded in this, the sooner Cook was put out of the way, the better would it be for himself. Accordingly he loses no time; for immediately on his return from London, he obtains strychnia, and administers it that very night. This attempt not being successful, another purchase was made, and a second dose administered the next day; and then Palmer thought that he had succeeded. And so in truth he did, in his immediate object; but Nemesis soon appeared upon the scene; and the villain who had hunted-down his too-confiding friend, became in his turn the object of such an unwearied and determined pursuit, as tracked him through all his subtle evasions, unmasked his most secret doings, and presented to the world such an aggregate of infamies having their climax in this one act, as—to the credit of human nature be it said—can scarcely be matched in the annals of jurisprudence.

In speaking of the previous administration of antimony, we have been obliged to advert—so mutually intertwined are all the main threads of this inquiry—to the purpose which may fairly be imputed to the prisoner, and the motives which present themselves for his crime. The history of his previous transactions makes it evident, that he was fully conscious that not only ruin but punishment was immediately impending over him, and was only to be averted, even for the moment, by the possession of the

means of staying the proceedings of his most urgent creditors. For not only had he involved himself in debt far beyond any power of self-extrication, but he had committed repeated forgeries, the exposure of which was inevitable, unless the bills to which they were attached could be taken-up before being presented for payment. Had he so failed in carrying-out his scheme, as never to have touched a farthing of Cook's money, still the presumption would have been strong that he intended so to act, and the murder would have been sufficiently accounted-for. But the case does not stop here: for as he effectually did that, the intention to do which would have constituted a sufficient explanation of his motives, he converted a presumption into moral certainty; and by appropriating Cook's money at the particular juncture in question, he afforded just the same kind of presumption of his murderous design, as would be fairly drawn in the case of a highwayman, who might be taken in the act of rifling of his property a man who had just fallen from the wound of a bullet fitting a pistol carried by the robber.

These are, in our apprehension, the most prominent among the antecedent facts of the case; but they are by no means the only ones. In regard to the subsequent facts, it will be sufficient to refer to Palmer's obvious desire to manage the funeral himself, to the resentment he showed at the interference of Mr. Stevens, to the manner in which he conducted himself at the post-mortem examination, so as to prevent the full contents of the stomach from being transmitted, to the attempt he made to bribe the post-boy to upset the vehicle in which Mr. Stevens was carrying the jar to London, and to his tampering with the post-master and coroner, as all having a marked significance when considered in connexion with each other and with the hypothesis of his guilt, whilst, if not absolutely irreconcilable with the notion of his innocence, they require such explanations as have assuredly not been given.

Thus, then, what we have already designated as a *framework* of evidence, is strengthened by such a variety of additional braces and supports, that it may be pronounced to be absolutely immovable. Some of these may be weak, but the others are strong. Some may even be rotten, but the rest remain as they were. Take away all the slighter parts, the strength of the whole is but little impaired. Remove even one of the principal beams (such, for example, as the first purchase of strychnia), the loss is scarcely felt, so completely is its function performed by the accessory parts. Look at the case in every light, and there is not a single difficulty or inconsistency on the hypothesis of Palmer's guilt; every fact is at once explained; every one of his proceedings has its appropriate meaning. But on the hypothesis of his innocence, such an aggregate of makeshifts and inconsistencies, of gratuitous suppositions and improbable assumptions, become necessary, as no one would for a moment contemplate in any other case. It is only where a man's life is at stake, that we even think it necessary to examine into their value; in any ordinary investigation, we should scout such an alternative as altogether inadmissible.

Our first position, then, is, that the real inquiry in any case of "circumstantial evidence" is not into the separate probative value of individual facts, but into the probative force of the collective aggregate. The probability deducible from each of them taken by itself, may be so slight as

scarcely to deserve to be accounted one. And yet a small group of such probabilities may suffice to afford a firm conviction. For, to revert to the case of the liquid tests for arsenic, even if it were a hundred to one that the precipitate by each were something else than arsenic, yet if nothing but arsenic could give all three precipitates, or even any two of them, their concurrence would afford as high a certainty as any chemical proof could furnish. We have simply to ask ourselves the question, "What other hypothesis is accordant with *all* the facts of the case?" and when no other can be conceived which is self-consistent and complete, we are as much bound to accept the charge as proved, as if a reliable witness had actually seen the crime committed. The proved fallacies of circumstantial evidence are often urged, especially by those who find themselves incriminated by it; but in all the instances referred-to, there was some other hypothesis open, and the question lay between the more and the less probable. In this trial, we affirm that the question lay between the morally-proved and the morally-impossible; and so both judge and jury seem to have felt.

But we go further, and say that we consider the proof, in such a case as this, to be actually more complete than if it rested on the most direct testimony, this testimony being unsupported by collateral evidence. For, making use of the test already prescribed, we find that, in such a case, the answer to the question, "What other hypothesis can be entertained?" might be manifold. The witnesses may either be mistaken as to the facts, or they may be wilfully mis-stating them, either from spite against the prisoner, or to shift the responsibility from the real culprits, who may either be themselves, or some parties for whom they are interested. Numerous cases of this kind are on record; and the hypothesis is one which should always be canvassed, when the case mainly hangs upon the evidence of one or two witnesses. Generally speaking, however, there is some collateral evidence which supports the proof which the testimony of the principal witness, if reliable, has afforded. This was pre-eminently the case, our readers may recollect, in the trial of Dr. Webster, one of the principal witnesses against whom was Littlefield, the porter of the Medical School; and although it was urged by Dr. Webster that Littlefield was the real culprit, and had contrived some of the most inculpating pieces of evidence (such as the finding the parts of Dr. Parkman's body in the privy-vault of Dr. Webster's laboratory), yet there were so many facts which this hypothesis did not in the least account-for, and so many more to which it was entirely opposed, that no one unconnected with the defence seems to have entertained it for a moment; and Dr. Webster, before his execution, asked Littlefield's forgiveness for having made this charge against him. Now, it is one of the most singular features in Palmer's case, that the evidence is drawn from such a variety of sources, as altogether to forbid the idea of any considerable error, either from accident or design; every part of the case mutually supporting, and being supported by, every other part. We must suppose so large a proportion of the witnesses to have forsworn themselves, to make the case other than it presents itself, that, considering the mutual independence and previous characters of these witnesses, such a supposition is altogether inadmissible. Suppose that Newton did forswear himself in

testifying to the purchase of the strychnia by Palmer, on the Monday, this does not weaken the proof that he purchased strychnia on the Tuesday, or that on both evenings Cook suffered from a form of tetanus only attributable to strychnine. And even supposing the purchase of the strychnia on the Tuesday to have been also mis-stated, the proof that Cook died of strychnine-poisoning still rests securely on the evidence afforded by the symptoms, and by Palmer's conduct before and after the occurrence, including his previous administration of antimony and his appropriation of Cook's property. The history of the symptoms on both occasions is vouched-for by such a mass of witnesses, as to leave no possible doubt as to the main facts; and the minor discrepancies are such as all human testimony is liable to. So, again, the history of Palmer's pecuniary transactions is so complete and consistent, that no considerable flaw can be suspected to exist in it. We must take the whole as it stands, or reject the whole as the most extraordinary piece of imposture that ever was devised.

We have left ourselves but little space to dwell upon the other part of the subject which we proposed to ourselves to discuss—the peculiar moral phenomena which Palmer's history presents. This would seem at first unmistakeably to indicate a state in which all natural feelings had become callous, and the light of conscience entirely extinguished. It seems difficult to account for the hardihood with which (if there be any truth in the antecedents currently laid to his charge) he coolly sacrificed his wife, his wife's mother, his brother, several illegitimate children, at least one victim of his seduction, besides two or three other individuals,—without exhibiting in his face and manner such evidence of a brutal nature, as to cause all around him to shrink from him with abhorrence and disgust. On the contrary, all accounts attribute to him not only an entire absence of anything like ferocity, but an attractive *bonhomie* arising from a constitutional kindliness of disposition,—in fact, that sort of nature which impressed most persons who came into contact with him, with the notion of his being a pleasant sort of a fellow, who would rather do a kindness than an injury to any one. In this respect, Palmer's character seems to have been a striking contrast to that of Dove, the Leeds poisoner, whose nature seems to have been essentially brutal, leading him to delight in the infliction of suffering on others, and making him an object of repulsion to those about him. But to those who look beyond the surface, the apparent inconsistency will disappear. In the one case, as in the other, the unbridled indulgence of selfish propensities was the dominant feature in the character; and though the mode of indulgence was different, the result was the same. The "turf" seems very early to have had a peculiar fascination for Palmer, and to have exercised that baneful influence over him, which *any* fascination—whether for women or wine, gambling or horse-racing—will exert on those who allow their better nature to be overpowered and their will to be led captive by it. No tyranny is more complete, than the tyranny of one absorbing passion. However virtuous and amiable a man may be in every relation of life, yet if he once give himself over to any such influence, he gradually becomes so completely enthralled by it as to feel powerless for self-extrication; and thus he may be driven,

irresistibly at last, to the commission of any crime, however monstrous, without having forfeited by any overt act the general estimation in which he is held. Such a state of subjection to a dominant impulse is really, when complete, to be accounted monomaniacal; and we believe it to be, as we urged on a former occasion,* the state in which many great crimes are committed. But the criminal is justly punished, not so much for the act itself, which he scarcely had within his control, as for the antecedent course in which he had the power of checking himself. The case seems to us like that of a man in a boat that is being drawn towards a waterfall by a current, out of which a moderate exertion will enable him to project himself; not having made that exertion in time, he is carried on faster towards destruction, but still may be saved by a vigorous effort; the time for this goes-by, and he is hurried along by the irresistible force of the torrent, until precipitated to his destruction in the depths beneath.

We will not inquire too narrowly into the nature of the early influences under which Palmer was brought-up; but enough has publicly transpired to make it obvious, that whilst they were of a kind to foster both self-indulgence and sensuality, they were but little favourable to the development of the moral sense. And if we are rightly informed, there were circumstances in his student-career, which showed that no firm barrier of principle had even then to be broken-down, when his absorbing passion required the means of its gratification. Had the black catalogue of his imputed crimes been then exhibited to him, or had it been predicted that he would commit that single one for which he has suffered, he would doubtless have repudiated the idea with abhorrence. But such a warning would probably have had little permanent effect upon him. No habitually-recurring temptations are capable of being resisted, save by a man of most determined will; they must be fled-from; and Palmer was not a man to do either the one or the other. The man who began with fraud proceeded to forgery; from forgery the descent was rapid to poisoning, for the sake of preventing its exposure; and when once familiarity with the idea had been established in his mind, he seems not to have been restrained by any lingering feeling of humanity, but to have given himself over to the pleasure of successful villany, not unmingled, perhaps, with some professional interest in the course of the fatal events which he had devised.

Such is one of the dreadful results of that habitual yielding to the indulgence of selfish propensities, which allows them to take full possession of the soul; and from such tyranny, yet more than from its consequences, should every one of us both pray and strive for deliverance.

It is not uninstructive to contrast with the character of Palmer, that of the wretched Dove, whose crime, identical in itself, but far different in its mental source, may be considered in some degree as a consequence of his own. It seems scarcely possible to conceive two men more unlike than these, save as to the predominance of evil in both. Palmer was a remarkably complete villain. His vigour and address in carrying-out his plans, were in perfect harmony with his sagacity in devising, and his determination in resolving upon them; whilst the coolness and suavity which he seems to have maintained even under the pressure of circum-

* British and Foreign Medico-Chirurgical Review, vol. xxiv. p. 240.

stances which would have rendered most men restless and irritable, served as an admirable veil to the fearful schemes which he was working-out beneath. Dove's order of villany, on the other hand, was a very low one,—that of the sneaking, irresolute coward, who has neither sense to form deep-laid plots, nor daring to execute them, but who does just as much mischief as he thinks he can without disagreeable consequences to himself. That his intellectual powers were of a very inferior order, cannot for a moment be doubted; that his tastes were naturally low and selfish, and that he was singularly deficient in consideration for the feelings of others, appears equally clear. Still there is no evidence of any deterioration of his reason, or of any perversion of his moral nature. What he was when he poisoned his wife, he seems to have always been. None of the witnesses who were brought to prove his insanity, could point to any recent change in his conduct or demeanour; on the other hand, their testimony, going back to his childhood and youth, showed him to have been from the first one of those ill-conditioned persons, who unite a semi-brutal *morale* to a weak intellect and feeble will, only wanting more hardihood of character to become absolutely ferocious. His passions do not appear to have been strong enough to master what reason he had; nor does it seem that habitual indulgence in any one special vice had given to it a tyrannous power. His case, on the contrary, seems to have been one of a kind not at all uncommon, in which the habit of yielding to any impulses and suggestions that fall-in with the selfish propensities of the individual, prevents the will from ever acquiring its rightful domination. The remarkable confession which he left behind him, seems to us (as regards its main features at least) to bear as strong an impress of truthfulness as any similar document we have ever perused; it shows how the suggestive influence of the wicked counsels of the "wise man" of Leeds, taking root and flourishing like rank weeds in a soil too poor to furnish a wholesome crop, concurred with the notion as to the non-detectibility of strychnia, which Dove derived from the early proceedings against Palmer, to give his thoughts the direction towards their final issue; but a more striking picture of irresolution was perhaps never drawn, than that which this poor wretch has left, of the succession of nerveless attempts whereby he familiarized himself with the use of the deadly weapon, which he used at last with fatal effect. Here, as in the case of Palmer, we have to look upon the crime itself, not as an isolated act, but as the almost natural result of a habit long previously formed; but while the habit consisted, in the one case, in the fostering care with which a master-passion was cherished, until it tyrannized over a will whose strength was shown (like Samson's) even in its captivity, in the other it was the early indulgence of every selfish and malevolent impulse, which prevented the will from ever attaining its rightful sovereignty. How solemn is the lesson afforded by each of these terrible cases, especially to all concerned in the training of the young, we trust we need not point out. Both speak, though in different ways, as to the essential importance of the culture and discipline of the Will, and of the early and firm implantation of those principles of Right by which alone it can be safely directed.

William B. Carpenter.

PART SECOND.

Bibliographical Record.

ART. I.—1. *Practical Remarks on some Points in the Physical Diagnosis of Phthisis Pulmonalis.* By JOHN HUGHES BENNETT, M.D., F.R.S.E., Professor of the Institutes of Medicine and of Clinical Medicine in the University of Edinburgh.—*Edinburgh,* 1856. pp. 37.

2. *Lectures on Clinical Medicine.* By JOHN HUGHES BENNETT, M.D., F.R.S.E., &c., Nos. 9 and 10.—*Edinburgh,* 1856. pp. 389 to 504.

BOTH these works contain matter of much interest. In the first, Professor Bennett directs his chief attention to the consideration of the value of the cracked-pot sound, which, like other physical signs, is losing its pathognomonic character, and demands a careful balancing of the concomitant conditions to assist us in determining its exact signification in individual instances. No one who has employed percussion extensively, has failed to see cases in which a marked *bruit de pot fêlé* was produced in the absence of other symptoms of a tubercular cavity. Dr. Bennett, for the purpose of rigidly testing the question, examined one hundred patients indiscriminately, and thus found that the cracked-pot sound (alas! that we must employ so grating a term) may be elicited in the perfectly healthy adult chest; and that, as Skoda has before pointed out, it may be produced in young children. The following are briefly the results of an analysis of the hundred cases:—The sound was absent in five cases exhibiting all the signs of cavities, and in two of these the presence of small cavities was proved by the cadaveric inspection; the sound was present in four cases of pleurisy, and several of pneumonia, in which there was no suspicion of a cavity; it was present in several cases where there was no evidence of any disease in the lungs at all; and it was observed to come and go in the same individual, "evidently in consequence of changed physical condition in the lungs during the progress of the case."

In all, twenty-nine of the hundred cases presented the cracked-pot sound. Of these, four were affected with pleurisy, five with pneumonia, one with pleuro-pneumonia, six with phthisis, five were other diseases with pulmonary complication, and in eight the pulmonary organs were healthy. The conditions regarded by Dr. Bennett as essential to the production of the cracked-pot sound are—1. A certain amount of confined or tense air in the tissue of the lung; 2. The sudden compression of this air by a solid body in its neighbourhood; 3. Communication of this air with the external atmosphere. It is manifest that these conditions may exist without the presence of a tuberculous cavity. The facts

brought forward by Dr. Bennett and other observers prove that the sound may be produced in health, as well as in diseases not presenting caverns in the lungs; and that therefore it is important in estimating its value in individual instances not to regard it as indicative of one morbid change, but to allow our diagnosis to be fixed by the whole complex of phenomena.

The other subjects discussed in this work are The Diagnostic Importance of Bronchitic Signs as preceding and marking Tubercular Disease of the Lungs, and The Diagnostic Value of a Microscopic Examination of the Sputum.

The 'Lectures on Clinical Medicine' are the last parts of a series commenced in 1850. They treat successively of Rheumatism and Gout, Functional Disease of the Stomach, Organic Diseases of the Stomach, Gangrene of the Lungs, and Dysentery, On the Diagnostic Value of the Absence of Chlorides from the Urine in Pneumonia, On Nephritis and Pyelitis terminating in Extensive Suppuration, On Bright's Disease, On Peritonitis; and the work concludes with an oration, On the Ethics of Medicine, delivered by the author as Promoter of the Medical Faculty in 1840.

The cases contained in the volume are of great interest, and, with the commentaries, are well deserving of the perusal of the earnest student.

ART. II.—*Observations on the Operative Measures necessary in the Treatment of Hare-Lip.* By RICHARD G. H. BUTCHER, Esq., Surgeon to Mercer's Hospital, &c. Illustrated with Coloured Plates and Wood Engravings.—*Dublin.*

THE arrest of development, known as hare-lip, has of late years received very much attention from all surgical writers, no less than from all surgeons who cultivate practical surgery. The state of the deformity, the manner of holding the child for the operation, the operation itself, the after-treatment, and the results, are but some of the subjects discussed in Mr. Butcher's treatise. We shall advert to some of the passages that strike us as being most remarkable. We are happy to find an expression in favour of early operating:

"I have watched over many with great anxiety, and am fully convinced that the safest period to the child for operation is from the termination of the first week to the end of the third month. No doubt it may be undertaken earlier, and has been with success in many instances; but I prefer waiting a few days after birth, in order, as it were, to allow the functions of the body to be healthily in action. During dentition some caution is requisite; but even during this process periods free from irritation and fever will arise which may be taken advantage of for operation. As the child advances in life the deformity is greatly increased, and the operative measures, I believe, become far more formidable; besides, the parts within the mouth, when the palate is widely fissured, become so disproportionately formed, that intonation and voice become spoiled for ever after. By delay, the tongue, as I have already alluded to, may become too large to permit of operation with safety." (p. 31.)

Mr. Butcher prefers the scissors to the knife for paring the edges of the fissure, and argues in their favour that they never contuse, and that they give less pain. To this we must take exception. Perhaps as compared with the large, ill-shaped, and badly-pointed old-fashioned scalpel

of the shops, badly-pointed because the end is round and the point not in the centre, they may be preferred. But most certainly not to the short, small, narrow-bladed and well-pointed modern knife. It is a fallacy to suppose that there are scissors with scalpel edges; if made they could not be used, because the blades would notch each other. We learnt this years ago, when endeavouring to procure such an instrument.

It is a rule of conservative surgery not to remove any part that can be saved, and more especially if it can be made available. This is particularly applicable to operations about the face; and above all, to that for hare-lip, as there is so frequently a want of integument, especially when there is double fissure. Therefore we did not expect to meet with the following passage from the pen of so good a surgeon:

"In addition to what I have already written about the management of the central piece of the lip in the double form, I have only to add, that when it is large enough to come down on a line with the red margins of the lateral portions, and even covered with a red border, I do not save it to make a part of the lip below. I have seen it, when preserved, fail to accept the union on one or the other side; and I have also seen a double notch occasioned by its preservation." (p. 42.)

We have ourselves many times made the piece available, and obtained a result that could never have been got without it, but for this a knife was required. The parts would have been too much bound with scissors to unite.

A good deal of diversity of opinion prevails as to the best mode of dealing with the projected maxilla, and the detached central osseous piece, in cases of complicated hare-lip. Some of the very highest authorities recommend cutting off the projecting piece. Sir A. Cooper says, "when the jaw itself projects, the common preliminary step to the operation consists in cutting away the bony prominence." Chelius likewise inculcates this precept in the following passage:

"If there be a bony growth in the cleft, it must, after the skin covering it has been raised, be removed with the nippers."

And again:

"If the incisive teeth project, they must be extracted if of the first set; but if of the second, it must be attempted to give them their proper direction by continued pressure, and if this be not possible, they also must be extracted. . . . On no account, in my opinion, should the projecting maxilla be cut away. In the youngest infants I have bent it back, rupturing its elastic structure; and in more advanced life, after Gensoul's method, breaking it with a forceps, and thrusting it back; and after either experienced but little difficulty in steadying the piece in its new berth." (pp. 42, 43.)

A method of dealing with protruding alveolus was introduced by Mr. Haynes Walton so long ago as 1848, and as it has escaped Mr. Butcher's notice, we quote it, because we believe it to have been adopted by all who are acquainted with the proceeding. After describing the preliminary operation on the lips, Mr. Walton continues:

"I now cut through the protruding alveolus in its entire thickness, applying the fine forceps at the spot, about corresponding to the space between the first and second incisive teeth (and it has happened in all the cases that the deviation from the natural contour commenced just about there), and bend back to the desired level the partially detached portion. The soft parts can now be brought

together with as much facility as when only fissure of the lip exists. Not much force is thus required in dealing with the bone after the forceps have been applied. There is no fracture, but a yielding, which, I imagine, may be ascribed to the yet imperfectly ossified incisive and palatal portions of the maxilla." (Braithwaite's 'Retrospect.')

Mr. Butcher objects to the use of an anæsthetic. "Once for all, I repeat, the risk is too great for its adoption." This we should scarcely have expected after we are told that one out of the eight cases narrated "died from fright." We think that chloroform would have given a better result.

The coloured lithographs that adorn the letter-press are executed with great taste and neatness.

ART. III.—*The Microscope and its Revelations.* By WILLIAM B. CARPENTER, M.D., F.R.S., F.G.S., Examiner in Physiology and Comparative Anatomy in the University of London, Professor of Medical Jurisprudence in University College, President of the Microscopical Society of London. Illustrated with 345 Wood Engravings.—*London*, 1856. pp. 778.

IT is rather with a view to showing our readers that we have not overlooked Dr. Carpenter's work on the microscope, than for the purpose of discussing it, that we prefer at once to advert to it briefly, than to delay our notice until more leisure and space would enable us to do full justice to it. The author's object is to guide the possessor of a microscope to the "*intelligent* study of any department of natural history that his individual tastes may lead him to follow out, and his particular circumstances may give him facilities for pursuing." Hence the work necessarily excludes the consideration of pathological or industrial microscopy. These subjects would be too special to call for their admission into a book of the kind before us.

After an introductory history of the microscope and microscopic research, five chapters are devoted to the optical principles of the microscope, The Construction of the Microscope, Accessory Apparatus, Management of the Microscope, and The Preparation, Mounting, and Collection of Objects.

The remainder of the work is devoted to the microscopy of vegetable and animal tissues, and of the inorganic kingdom. These subjects are treated successively in fifteen chapters, under the following heads:—Protophytes, The Higher Cryptogamia, Phanerogamic Plants, Protozoa, Foraminifera, Polycystina, Sponges, Zoophytes, Echinodermata, Polyzoa and Compound Tunicata, Molluscous Animals generally, Annulosa, Crustacea, Insects and Arachnida, Vertebrated Animals, Applications of the Microscope to Geology, and Inorganic or Mineral Kingdom, and Polarization.

We have no doubt that the completeness of the information imparted, and the practical utility of the work, which is not a little enhanced by the admirable illustrations profusely scattered through it, will secure to it a reception in every way commensurate with the high reputation of the author.

ART. IV.—*On the Defects with Reference to the Plan of Construction and Ventilation of most of our Hospitals for the Reception of the Sick and Wounded.* By JOHN ROBERTON, Surgeon. (Reprinted from the 'Transactions' of the Manchester Statistical Society.)

ONE of the reasons why, generally speaking, an improvement takes place in the condition of the patients on admission to a public hospital, is to be found in the fact of their removal from a less pure to a better atmosphere. The necessity of proper ventilation in all abodes of the sick is now no longer a matter of speculation, though the best mode of securing that ventilation is still *sub judice.* It appears as if the multiplicity of inventions and ingenious contrivances were, after all, to conduct us to the unity of the fact that the ordinary window, with its vertical or horizontal movement, is the best, the simplest, and surest mode of securing pure air in our apartments and in those of a hospital. This, too, seems to be the view upon which Mr. Roberton urges the propriety of following the very excellent plan for the construction of hospitals adopted by the authorities of Bordeaux. Mr. Roberton, adverting to the prevailing arrangement of our own hospitals, shows that it tends to create a *hospital atmosphere,* owing to all the wards communicating with one another by passages and stairs. The peculiarities of the Bordeaux Hospital, to which the St. John's Hospital at Brussels, the new Lariboisière and the Beaujon Hospitals at Paris, are analogous, are described as follows:—

"Standing in an open space of ground, called the Place du Fort du Hâ, we look on a fine building, the frontage extending perhaps 140 yards. Passing in at the centre gateway, we behold a beautiful court, of about a quarter of an acre, planted with evergreens and flowering shrubs, surrounded by a carriage drive, and beyond the drive, by a light arcade or ambulatory which serves to connect on the two sides facing each other a succession of pavilions or tiers of buildings standing parallel, and having the ends towards the square. We step under the arcade, and above a door opening into the basement at the end of one of these pavilions we see the words 'Salle Seconde.' We enter, and discover a ward about 140 feet long, 30 feet wide, 19 or 20 feet high, having tall, narrow windows, directly facing each other on the opposite side walls, containing thirty-eight beds—nineteen on either side, and each bed—the bedstead small and of iron—hung round with white dimity curtains, but having no tester. Near the entrance is a room for the sisters or nurses, and at the extremity of the ward, a door on the right hand, leading to clean, well-ventilated closets, and a door on the left to a spacious lavatory.

"After surveying the general aspect of the ward, we feel tempted to look out at one of the windows, and are surprised to see a beautiful garden, planted with vines and roses—the garden surrounded by a neat footpath. We cross the ward, to discover what there is on the other side, and find a garden resembling the one we have just seen. The patients, whenever they chance to approach a window on either side, look into a beautiful garden.

"Having surveyed one ward, we ask the way to the next, and receive for reply, 'There is only one door; you go out at the door by which you entered.' Thus we discover the important fact, that as we entered this ward from the open air, so must we emerge from it again into the air. Once more under the arcade, we proceed towards the next pavilion, and find that, extending between the pavilion we have left and this one, is a light iron railing, through which we see one of the gardens already spoken of. There is a gate—we enter: the garden is in length the same as the ward, in breadth about fifty feet. On stepping into the ward next in course, we find it a copy of the former; and now we come to perceive that

as each pair of pavilions has a garden intervening, the windows of the wards look into gardens. Having examined the wards on the basement story of each of the separate pavilions—five in number—on this side of the square, we ask how we are to reach the wards in the second story, and are pointed to a stair at the end of the arcade, which winds up to a like open arcade above, out of which the wards are entered in the same manner as below. Every ward, I repeat, has one—only one door. You enter from the open air, and when you seek to return, your exit must be by the same door again into the open air. In follows, of course, that each ward is itself *a separate hospital,* having no communication with the other wards; so that if we were to suppose one of these to be crowded with the worst kind of surgical maladies for causing foulness, the foul air could not find a passage into any other ward—an hospital atmosphere would be impossible. I need hardly say, however, that the ventilation by windows being such as I have described, no ward need be allowed to become foul.

"Another advantage not to be overlooked attaches to this plan of construction; there is no need to limit the size of the hospital or the number of beds. In England, where nine-tenths of the hospitals are on the plan of an hotel or a large dwelling-house, we are afraid to open a large hospital. It has long been a painful conclusion of experience, that the larger the hospital, the higher the rate of mortality. Our Infirmary has little over 200 beds, rarely that number occupied, and yet, when the house is pretty full, the health of the patients, it is said, soon deteriorates; whereas, on the Continent, several of the finest hospitals accommodate from 600 to 800 patients. That in Bordeaux has 710 beds for the ordinary class of patients, and eighteen beds for paying patients. The number of patients of both classes in the wards, when I was there in 1855, was about 550. In an hospital such as this, if you were only to extend sufficiently the size of the court, and to multiply sufficiently the number of the pavilions containing the wards, thousands of sick might be lodged without inconvenience—without the slightest risk of generating an hospital atmosphere."

We regret that we are unable to add to this description the plan attached to Mr. Roberton's pamphlet. We thank him for drawing the attention of the public and of the medical profession to the advantages of this mode of construction, and hope to find an opportunity of entering more fully into the important questions involved in the selection of the site, arrangement, and general construction of hospitals. In the meantime, we advise a perusal of Mr. Roberton's paper by all who are likely to give an opinion on these matters. We would add a special recommendation for those of our readers, who may visit Paris, to inspect the new Hôpital Lariboisière, which has appeared to us one of the best constructed and most perfectly organized institutions of the kind we have ever examined. It is built upon the system advocated by Mr. Roberton, but having only been completed in 1853, is probably less known to our countrymen than the older hospitals of Paris. It is larger, and in most respects better appointed, than the Hôpital St. Jean at Brussels; though this too, on account of the excellency of its arrangements, and the peculiarity of its construction, merits to be examined by travellers who take an interest in institutions of the kind.

ART. V.—*Ismeer; or, Smyrna and its British Hospital in* 1855. By a Lady.—*London,* 1856. pp. 350.

MANY have doubted as to the success of the experiment of establishing civil hospitals at the seat of the late war; many have doubted the benefits

to be conferred upon the sick and wounded soldier by ladies who went out to tend them, following that yearning which impels all noble and energetic minds to succour the distressed. The simple and unaffected tale contained in 'Ismeer' can scarcely, we think, fail to convince the sceptic, that the ladies who accepted the mission executed it in the highest spirit of self-devotion, and without shrinking from the many painful and irksome duties which it necessarily entailed. There is a truthfulness and reality, with so complete an absence of all pretentious egotism, in the narrative, that the reader has a difficulty in laying the book down before he has perused the whole. The descriptions are so well coloured, and the anecdotes are so cleverly told, that we become well acquainted with the persons to whom we are introduced, and, for the time, inmates of the Smyrna Hospital itself; occasionally obtaining a glance at the verdure and richness of the surrounding country, a peep into the house of a Turk, or a glimpse of robber-life in the East.

The terms upon which the lady-nurses were with the staff and with the soldiers under their care is shown, by numerous anecdotes, poetic effusions, and letters of soldiers, scattered here and there through the book, to have been all that could be desired. After quoting a touching poem by an artilleryman, who suffered severely from chronic dysentery, which ends thus,

> "Might I march through life again,
> In spite of every bygone ill,
> To the end of life's campaign,
> I would be a soldier still;"

the authoress relates, that—

> "Another poor man of the same corps, who was crippled from chronic rheumatism, his hands being doubled up and perfectly dry and useless, two of the ladies used to rub them till a slight degree of moisture was perceptible. When he recovered the use of them slightly, he was ordered home, and he entreated to be allowed to remain, saying, 'that he should nowhere be so well attended to; and that his mother even, if he went home, could not do for him all the ladies were doing.' Another man wrote to his mother, saying, 'fine ladies and the best of doctors had come out from London to attend on him.'" (p. 130.)

The chief difficulties that the ladies had to deal with in the first instance, and, though not great in reality, probably the hardest to bear, was the prevailing error that, "as the ladies were undertaking an unusual work, they ought, as it were, to lay aside their position, habits, and feelings, and descend to the level of servants." Some little mortifying incidents took place at first, but, of necessity, the proper balance of society was soon established; and though serving, the ladies resumed their position in the eyes of the world. But the error did not so much affect the ladies as the nurses:

> "The real evil was done to the nurses, who fancied that according to our descent in the social scale was to be their ascent; and that by some process unknown, on their going out to the East they were to become ladies; and this for a time produced ill-will and bad feeling in some, but many of them were too sensible not to see things very soon in their proper light."

How the Smyrna Hospital was established, how Dr. Meyer governed the hospital, and the subordinate officers co-operated with him and the ladies—how they all worked—and how, too, they employed their time

when hospital work was slack, must be read in the book. We have only to add the wish, that the authoress, who must long to employ the tact and knowledge she has acquired at the Smyrna Hospital in a similar way in her native country, may find a suitable sphere for her talents—a sphere which it cannot be difficult to meet with, now that public attention is so largely directed towards "ameliorating the condition" of aimless ladies.

ART. VI.—1. *Manual of Chemical Physiology.* From the German of Professor C. G. LEHMANN, M.D. Translated, with Notes and Additions, by J. CHESTON MORRIS, M.D. With an *Introductory Essay on Vital Force,* by SAMUEL JACKSON, M.D., Professor of Institutes of Medicine in the University of Pennsylvania, &c. Illustrated with 40 Woodcuts.—*Philadelphia,* 1856. 8vo, pp. 331.

2. *A Handbook of Organic Chemistry; for the Use of Students.* By WILLIAM GREGORY, M.D., F.R.S.E., Professor of Chemistry in the University of Edinburgh. Fourth Edition.—*London,* 1856. Small 8vo, pp. 627.

WE have, in a former article,* noticed Professor Lehmann's 'Handbuch der Physiologischen Chemie,' an abridgment, by the author himself, of his great work of Physiological Chemistry. Of this abridgment the greater part of the volume now before us is a translation. The reasons for the transposition of the title of the original are thus assigned in the preface:

"To adapt the work for the use of students of physiology, I have incorporated in the text additional matter (derived mainly from notes on Dr. Jackson's 'Lectures,' Carpenter's 'Human Physiology,' Todd and Bowman's 'Physiological Anatomy,' Kölliker's 'Microscopic Anatomy,' &c.) of a more purely physiological nature, which will be found included in brackets. Short notes have also been added, in the shape of an Appendix, on kindred subjects not treated of by the author; and illustrations selected from various sources have been introduced, instead of referring, as the author has done, to the 'Atlas of Physiological Chemistry,' by Otto Funke. These alterations have so changed the character of the work, as to render the title of 'Chemical Physiology' more applicable than that originally given to it of 'Handbook of Physiological Chemistry,' which has, however, been retained for Dr. Lehmann's portion of it."

The translation appears, on the whole, to be correct. Some passages are too literally rendered, and in some the meaning is obscure; but the work is, in the main, highly creditable to the editor. The publishers have done their part well; the type is clear and distinct, and the volume altogether is well "got up." Dissenting from the author's doctrine of Vital Force, the translator prefixes to the 'Handbook' an essay, prepared at his request by Dr. Jackson, On the Human Organism and its Forces.

In our tenth volume we noticed the third edition of Professor Gregory's excellent and comprehensive manual; the appearance of a fourth at a diminished interval is a sufficient indication that the original approval of the work has been more than ratified by the test of time. The rapid progress, during the last four years, of the science of which it treats, has rendered it necessary to make important changes throughout the volume, so as to increase its size by nearly one hundred pages. With

* Vol. xv. p. 102.

the aid of a short supplement, the work is made to embrace the most recent discoveries in organic chemistry; while, as in the former editions, a full index and a well-arranged table of contents render it a most convenient book of reference.

ART. VII.—*Physicians and Physic. Three Addresses.* 1. *On the Duties of Young Physicians.* 2. *On the Prospects of Young Physicians.* 3. *On the Modern Advancement of Physic.* By JAMES Y. SIMPSON, M.D., F.R.S.E., Professor of Medicine and Midwifery in the University of Edinburgh, and Physician-Accoucheur to the Queen for Scotland, &c.—*Edinburgh*, 1856. pp. 133.

IN the present restless and unsettled state of the profession, it is peculiarly gratifying to snatch a moment of repose for the purpose of reflecting on the picture drawn of our sacred calling by the masterly hand of Dr. Simpson. He speaks earnestly of the duties of the medical man, as every one must who is addressing an assemblage of young graduates; he warns them against entertaining undue and over-ambitious hopes of success, though he promises them an amount commensurate with their own labour and perseverance; Dr. Simpson holds up to the young physician the most gratifying assurance of satisfaction, pleasure, and happiness which the continued study of our profession and the conscientious fulfilment of its calls, carry with it. We think, with Dr. Simpson, that a glance at the past, a past which shows us that medical art has, in our country, during the last two hundred years, all but removed plague, ague, dysentery, scurvy, small-pox, and the dangers incident to child-birth—we think that the glance is calculated to fill us with cheering and bright hopes for the future; such encouragement is indeed necessary, for though much has been done, very much more requires to be effected, and will be effected.

ART. VIII.—*Letters to a Young Physician just entering upon Practice.* By JAMES JACKSON, M.D., LL.D., Professor Emeritus of the Theory and Practice of Physic in the University of Cambridge, U.S., Honorary Member of the Medico-Chirurgical Society of London. Fourth Edition.—*London* and *Boston*, 1856.

IT is scarcely nine months since we had the satisfaction of speaking in terms of very warm commendation of Dr. Jackson's 'Letters' on their first appearance. That our opinion was just has been amply shown by the favourable manner in which the profession have received the book, of which we now introduce to our readers the fourth edition. We sincerely congratulate the venerable author on a result which cannot fail to afford him a peculiar gratification.

ART. IX.—*The Hospital System of London.*—*London*, 1856. pp. 53.

THE pamphlet bearing the above title is one containing so much useful information, and so sensible a comparison between the French and Vien-

nese hospital system and our own, that we have no hesitation in drawing attention to it. It is simply intended to show, by reference to statistics, that our hospital system is one of great extravagance, that we are in the wrong in making the distinction that now exists between the hospital and the workhouse infirmary, and that the sick poor, as such, having an undoubted claim upon the community, their restoration to health ought not to depend upon the accident of their being acquainted with the governor of a hospital, but that they should all equally have the opportunity of enjoying the best accommodation and advice which the State can afford.

We much doubt indeed whether the French system of centralization is as beneficial as the author opines it to be, but we are satisfied that the manner in which money, time, and labour are frittered away and wasted in most of our hospitals, is injurious to the public at large, and far from advantageous to the profession. That our workhouse infirmaries deserve greater attention and regard, that the principles upon which they are conducted ought to be in every sense more liberal, is as certain as it is true that reforms in an opposite sense are desirable in the hospitals. The public now point to the hospitals as one of the things an Englishman may boast of: the public do not know that they are enacting the old story of St. Crispin, and filching from the medical man, directly and indirectly, what they, the public, ought to pay for fairly and honestly. But we abhor the cry, *ad misericordiam;* we are ourselves to blame for the self-inflicted losses—self-inflicted, because we submit to the vicious system of giving hard labour without the reward that every labourer may justly claim; what we would wish to impress upon the promoters of hospitals, is the necessity of reform in the management of institutions which, with an improved system, might be rendered at least twice as useful by affording aid to twice the number of sick poor, and by excluding those for whom the charities never were intended. We are unable to consider this question fully at present, but we would submit one calculation to our readers, which may induce them to reflect a little more deeply upon the point suggested:

"We find," says the author, "that in London 4,212 beds are maintained, and 37,886 patients treated, at an annual expense of 172,121*l.*; whereas in Paris, 6,854 beds and 87,007 patients only cost 162,732*l.* a-year."

From our acquaintance with hospital statistics, and from the authorities and official documents referred to in the pamphlet, we have reason to believe the author's statistical data to be correct. Our general opinion of the conclusions we have already expressed.

ART. X.—*Diseases of the Heart, their Pathology, Diagnosis, and Treatment.* By W. O. MARKHAM, M.D., Fellow of the Royal College of Physicians, Assistant-Physician to St. Mary's Hospital. — *London,* 1856. pp. 346.

DR. MARKHAM has here, without "pretending to offer the reader a full and didactic relation of all that may be said concerning diseases of the heart," brought together a great deal of information likely to be very useful

to the student and general practitioner. It is matter, however, which is to be found elsewhere. Dr. Markham has an admirable field for original observation at his disposal—he is an able physician; we should therefore have been better pleased to have met him as an original observer. Much has been done towards advancing cardiac pathology; this branch of medical science is by no means behind other departments of medicine. Still, much remains to be done. This has again forcibly suggested itself to us in the perusal of Dr. Markham's volume. The relations existing between cardiac and cerebral symptoms in acute rheumatism, the connexion between albuminuria and cardiac asthma, one of the most distressing and intractable affections to which the heart's functions are liable, are points upon which information is much wanted. The physical signs of diseases of the right side of the heart, again, are open to investigation; and we may point to the pathology and diagnosis of aneurism as well worthy of further inquiry. Nor can we think that fatty degeneration of the heart is a subject upon which some new light may not be thrown. These are some of the points to which we hope that future inquirers will direct their attention, which they can scarcely do without reaping a good harvest. It is in these fields that we wish and expect to meet Dr. Markham again, because, without wishing to detract from the utility of the present volume, we feel satisfied that his talents and abilities will there meet with a greater reward.

Art. XI.—*The Surgeon's Vade Mecum. A Manual of Modern Surgery.* By Robert Druitt, Licentiate of the Royal College of Physicians, London, Fellow of the Royal Medical and Chirurgical Society, of the Medical Society of London, &c. Seventh Edition. Re-written, much Improved, and Illustrated by Three Hundred highly-finished Wood Engravings.—*London*, 1856. pp. 760.

It is unnecessary for us to say more of this—the seventh edition of a work deservedly popular—than that it contains a large amount of new matter, and that the most recent improvements in surgery receive full consideration and illustration. Dr. Druitt's 'Vade Mecum' in every way merits the confidence of the student and practitioner, and has our very warm commendation.

Art. XII.—*Traité Pratique des Propriétés Curatives des Eaux Thermales Sulfureuses d'Aix-la-Chapelle, et du Mode de leur Emploi.* Par L. Wetzlar, D.M., Médecin aux Eaux d'Aix-la-Chapelle, Membre de plusieurs Sociétés Savantes.—*Bonn*, 1856. pp. 82.

A Practical Treatise on the Curative Powers of the Sulphurous Thermal Springs of Aix-la-Chapelle, and on the Mode of Administering them. By L. Wetzlar, D.M., Physician to the Baths, &c.

We apprehend that the daily increasing facilities of travel will cause the annual migration of our countrymen to the baths lying in the volcanic regions of the Rhine and Bohemia to present a corresponding increase

after each session of Parliament. It behoves the medical man of this country to make himself acquainted with the characters and properties of those watering-places which are most in request among our countrymen, or, rather, offer the most probability of aiding the invalid in the recovery of his health. Among these, Aix-la-Chapelle occupies a prominent place. Situated on the great route to the antiquities of Cologne, and the yet more fascinating charms of the "wide and winding Rhine," it is one of the most accessible of the German watering-places, while the chemical and physical constitution of its water renders it of great value in a variety of chronic affections, which we frequently attack in vain by the ordinary forms of medication.

Dr. Wetzlar favours us with the results of a personal experience of twenty-three years. The contents of his book entirely accord with the title. The details of numerous interesting cases illustrate the doctrines which he promulgates, and give to the latter a more definite character than is always met with in the writings of balneologists. The diseases in which the sulphurous waters of Aix-la-Chapelle are chiefly beneficial are chronic rheumatism, old-standing syphilitic disease, and chronic affections of the skin. Other morbid conditions are benefited by the external and internal administration of these waters, but there is one disease which has repeatedly been brought under notice of late, and which appears to be more amenable to treatment by the Aix waters than by any of the ordinary pharmaceutical preparations—it is the fatty degeneration of the muscles characterizing what Cruveilhier has termed progressive muscular atrophy. Four cases are given in detail, in which the results are extremely favourable.

The use of the waters is inadmissible in fevers, acute inflammatory affections, active congestions, and in plethora; in tubercular disease, diseases of the heart, aneurism, hæmorrhage, cardialgia, recent diarrhœas; and in pregnancy.

To this brief summary of the indications and counter-indications for the therapeutic employment of the Aix waters, we would add our hearty commendation of the tone which pervades Dr. Wetzlar's book. Medical men anxious for information on the subject, will find it a useful and trustworthy guide.

ART. XIII.—*The Medical Remembrancer, or Book of Emergencies; concisely pointing out the immediate Treatment to be adopted in Cases of Poisoning, Drowning, Apoplexy, Burns, and other Accidents, with the Tests for the principal Poisons, and other useful information.* By EDWARD B. L. SHAW, late Surgeon to the Royal Humane Society. Fourth Edition, re-written and much enlarged, by JONATHAN HUTCHINSON, Surgeon to the Metropolitan Free Hospital.—*London*, 1856. pp. 107.

THE subject-matter of this little book is sufficiently indicated by the title, which we have given in full. The contents are arranged alphabetically, so as to be very easy for reference; and the whole constitutes, what it undertakes to be, a compendious and very portable *vade mecum* for the medical man. The information the booklet imparts is correct, suitably selected, and very properly condensed.

PART THIRD.

Original Communications.

ART. I.

The Blood—its Chemistry, Physiology, and Pathology. By THOMAS WILLIAMS, M.D., F.L.S., Physician to the Swansea Infirmary.

(*Continued from No.* 29, *p.* 207.)

IN the animal series, the fluids and the solids exhibit a never-varying relation to each other. Where the machinery of the latter is of low standard, the former is simple in chemical and vital composition.* As new organs are added to the organism in the zoological series, so new principles or elements are developed in the fluids, and conversely as the scale is descended. This proposition was discussed at length in former papers. Of course it is not given as an unexceptionable rule. Thus expressed, it may probably, as details of structures are more and more amassed by special and improved observation, come to wear too absolute a form. It was then stated that, in going down the series, the disappearance of an organ, or a system of organs, from the solids, implied necessarily the cessation in the fluids of those products which such an organ or system was designed to elaborate. But another method of descensive simplification was then also explained. It was shown that even the ultimate histological elements grew more and more simple. This idea was supported by the history of the muscle and nerve-tissue in the scale. It is self evident that the bile of an annelid or an echinoderm cannot be so complex a fluid as the bile of a mammal; because, first, the biliary organ in the former instances, by which that bile is secreted, is more simple than it is in the latter; and secondly, because the fluid from which that organ draws the product of its action is more simple. This argument applies with equal force to all the solid systems of the body. As the animal chain is histologically traced downwards, it is found that the higher class of tissues disappear more and more, and that the less endowed elements acquire a greater and greater predominance in the organism.

Of the *general* truth of these views, thus recapitulated after much subsequent study and observation, the author is still convinced.

The system of the fluids does not constitute, in the zoological series, an independent chain. A study of this system, apart from its con-

* See the author's preceding papers in this Review for October, 1853, January, 1854, January, 1855.

nexions, would lead to no useful results. Viewing it in its reciprocal relations with that of the solids, an intelligent observer cannot ascend or descend a step without acquiring a new idea or seizing a new principle. The fluids act and are acted upon; this is also true of the parenchyma. The reciprocity is an endless tangle. No one point more than another can be signalized as the beginning or the end of a succession. This difficulty renders the present inquiry both long and involved.

If it were required merely to trace the comparative chemistry of the animal fluids, the problem might be readily solved; but it is required that we also determine the varying anatomical relations of them in each class. The anatomical *place* of the nutritional liquid in the organism is an important event to record. From this fact may be inferred its nutritive value. Three distinct anatomical situations have already been indicated. In all the Polyp and Acaleph families it is contained within the digestive system; in the Radiate and Annulose classes it is lodged in the general cavity of the body, and constitutes a well-marked division; while in the Mollusca and Arthropoda it offers all the characters of a perfect circulatory apparatus. Such are the outline landmarks of this inquiry, as far as it relates to the invertebrate animals. It has already been prosecuted upwards to the limit of the Echinodermata.* In proceeding to that class which, on the ground of general affinities, may be placed next in the upward order, many anomalies will be discovered in the systems of the fluids. The *entozoa* present three prominently defined groups — the *Nematoidea*, the *Trematoda*, and the *Cestoidea*. The Nematode entozoa unite themselves most intimately with the Gordiaceæ, coming thus into immediate contact with the annelids.† The trematodes are organized on a plan almost identical with that on which the planariæ are constructed. There are, notwithstanding, several points of difference. In this direction, again, the entozoa are united by a continuous line to the annelids. The Cestoidea form a singularly isolated group. By their zoological affinities they are related only to the trematodes. In the character of their digestive system (if indeed there exist one at all) they are unique; in their fluid system, which has never yet been clearly defined or correctly understood, they stand in the animal series without a parallel. In the progress of the present papers it will be shown that, in the nature and properties of their parenchymatous tissue, the cestoids are also peculiarly distinguished. Destitute of a distinct digestive system, they are supposed to live by fluid imbibition at the general tegumentary surface. Destitute also of an individualized system of nutritive fluids, they may be said to be

* See this Review for January, 1854.

† In indicating this alliance between the Nematodes and the Gordius family of the Nemertine Annelids, I depend upon the accuracy of the details lately published by Meissner (see Zeitschrift für Wissenschaftliche Zoologie, Mai 20, 1855) with reference to the anatomy of the Gordiaceæ. This view, however, is not supported by Meissner himself. It is by Siebold. Meissner denies the nematoid affinities of *Gordius*, on the ground of a striking difference in the character of the digestive system. But if his account of the structure of the integuments and the reproductive organs in *Gordius* and *Mermis* be true, they must stand in very close proximity to the nematoids. It will be my duty, however, soon to show that the reproductive system of these two annelids, as represented by him, differ so widely from that of the Borlasiæ and Nemertidæ (so extraordinarily misunderstood by De Quatrefages), that it would be more consistent to place *Gordius* amongst the Nematoids than in the ill-appointed class of the Turbellaria. Meissner gives no account of the fluid system of Gordius.

reduced to the level of the protozoa. But this is to anticipate what will be afterwards given in detail.

The fluid systems of the entozoa cannot be satisfactorily studied without the previous discussion of some preliminary points. Is there any known animal whose bodily substance consists exclusively of solid or semi-solid parenchyma? Do the amœbiform animalcules fall under such a category? Are the gregarinæ and many of the infusoria to be thus defined? Let these points be passingly considered before entering at length on the subject of the entozoa.

A gregarina approaches, probably, more nearly to the structure and character of a "cell," than the other protozoa. It is, however, more like a cell in its mode of life than in its structure. It is curious that by the most recent observers, while the "vesicles" of all the protozoa are admitted *not* to be nuclei, but vacuoles, the vesicles of the Gregarinæ are recognised in that character.*

This view is not in accordance with the author's observations. If the vesiculæ in the gregarina be cell-nuclei, then the vesiculæ of all the protozoa can be none other. The same definition applies to a gregarina as to any other protozoon. But as Auerbach has lately shown, the nuclei are distinct from, and may co-exist with, the vacuoles in the amœba. This animalcule is a mass of vital parenchyme. Its surface is a pellicle, not a cuticle. It is ciliated in some, smooth in other species.† The vesicule is single (fig. 1) in some species; in others, several are present; but only one nucleus, when seen at all, can be found in one individual. The pelli-

1

Fig. 1. Paramecium (?) from testes of Lumbricus terrestris. Mag. 500 diam.

1②

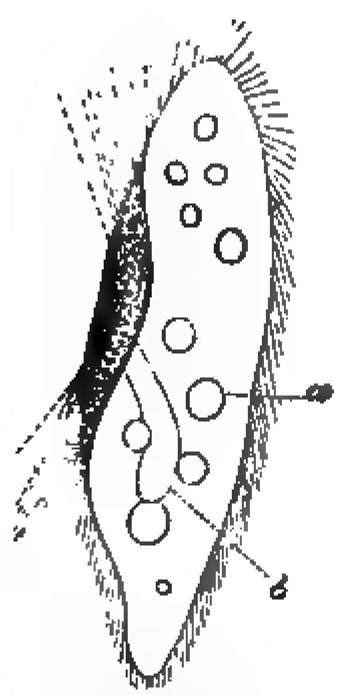


Fig. 1ª. Paramecium caudatum, in outline.

a. Contractile vesicles.
b. Digestive cavity.

* Auerbach (Zeitschrift für W. L., &c., December 31, 1855), in a recent admirable paper thus expresses himself:—"Wie ganz anders verhalten sich in allen jenen Beziehungen die Gregarinen mit ihrer membranösen, ringsum geschlossenen Hülle, mit ihrem bläschenförmigen, kernkörperchen enthaltenden, einem Keimbläschen täuschend ähnlichen Kerne, mit ihrer Ernährung durch Aufsaugung der umgebenden Flüssigkeit."

† I cannot understand how it is that several writers whom I have consulted (Siebold, Stein, Colin, Perty, &c.) should omit all reference to the *ciliated* surface of the gregarinæ. I can only account for this omission by supposing that there are two classes of gregarinæ, in one of which the pellicula is ciliated, in the other it is smooth or non-ciliated. It is most vigorously ciliated in the gregarinæ which at this season (August) abound in the liver of the cephalopods and the ovaria of *Lumbricus* and *Nais*.—*G. Scolopendræ*. (See p. 53 Ver. Anat. Physiol.) The surface is represented by Siebold as unciliated. Several varieties figured by Leydig in Müller's Arch., 1851, are also represented as unciliated. If they should be decided *not* to be Gregarinæ, then they must be admitted to be *agastric* paramecia. But whether they be Gregarinæ or Paramecia, the anatomy of them as described in the text will remain the same.

cula* is highly contractile. In virtue of the vermicular movement of this covering, the animal is observed to be constantly changing its form. In the elongated, band-like varieties, the extremities are curved upwards or downwards, and the body at some points of its length is compressed from the band into a thread-like form.

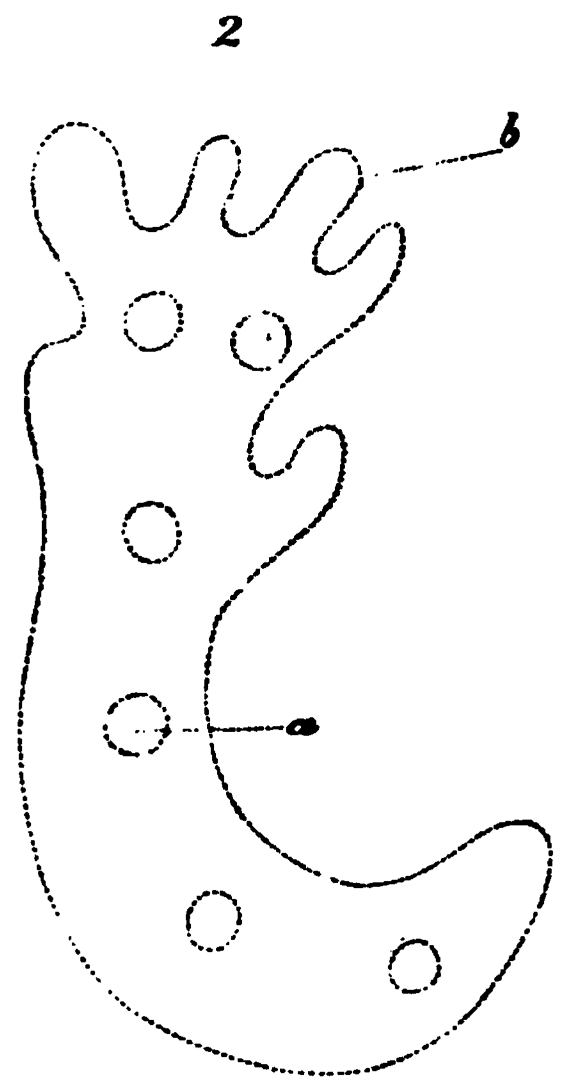


Fig. 2. Gregarina, from the liver of *Octopus vulgaris*. Mag. 500 diam.

a. One of the vesicles.
b. A contracted outline, taken while the object was in the act of changing its shape.

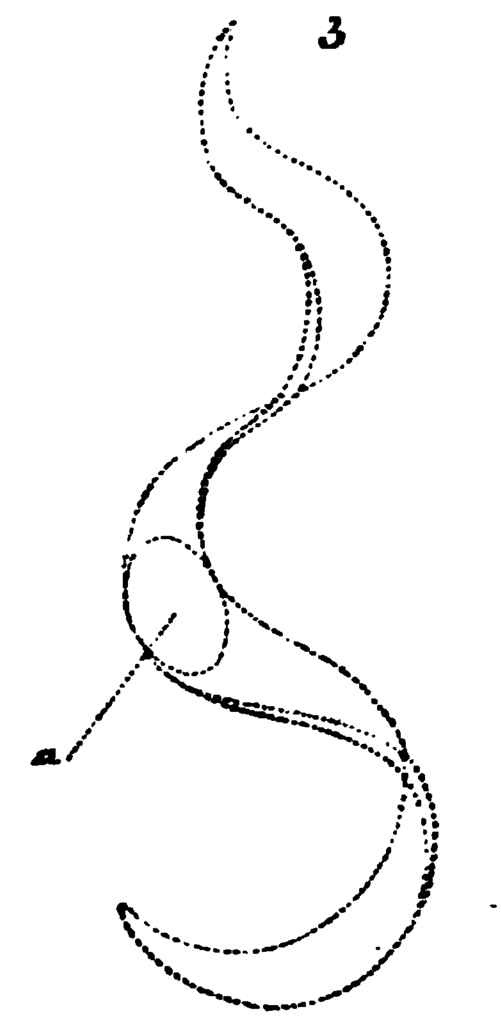


Fig. 3. Gregarina, from the ovaria of *Sabella alveolata*.

a. Vesicle.

If this transverse constriction be strong, the entire substance of the body is cut across. The author has repeatedly witnessed this fissiparous act. It is effected by the powerful circular contraction of the pellicula. The characteristic of the cuticle of a gregarina is extreme irritability. With this property the ciliary coexists. The cilia are prolongations of the *pellicula*—a direct demonstration of the identity of the contractile or irritable and ciliary power!

What is the nature of the semi-fluid vital substance (*Körpermasse*) of which the entire body is composed? Is it a particle of granular protoplasm, in which there is no distinction of parts into fluids and solids? In such case a gregarina may indeed be defined as unicellular—the vesicles as nuclei. This view is certainly untenable. The vesicles are not nuclei. They contain no nucleoli. They are undoubtedly of the same character as the "contractile vesicles" of the infusoria. They do not, however, contract so visibly, but they do undergo a change of outline.

* In describing these animals, I adopt with great pleasure the terminology proposed by Mr. Carter in his recent admirable paper on the Infusoria, published in the Annals and Magazine of Nat. Hist. for Aug. 1st, 1856.

If they were nuclei, one only should exist in each individual. In the large gregarinæ of the cephalopoda several, five or six, of various size, may be seen in every individual (fig. 2). Moreover, they are not always spherical.

In many instances they are elliptical (fig. 4). Sometimes the elongation of the vesicle is carried so far as to give it the form of an alimentary canal in the axis of the animal (fig. 5).

These large specimens are favourable for the determination of some points still *sub judice*, as to the real nature of these parts. In the instance of fig. 5,* the vesicle undoubtedly changes its size. At one moment it is seen as a line, at another it is much more obvious. It is not so easily proved, as in the infusoria, that the change of form is due to the contractile power of the vesicle. It may possibly result from that compressing, waving movement which is incessantly going on in the surrounding sarcode and pellicula. Other reasons, however, may be adduced against the theory which assigns the character of cell-nuclei to the vesicles in question.

4

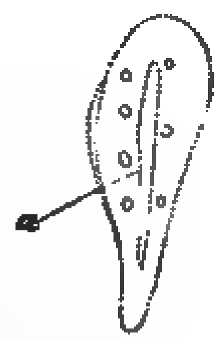


Fig. 4. Paramecium (?) Young. From *Nais Jordanii* (Mihi).

a. Elongated vesicle.

Fig. 5. Paramecium (?) Mag. 500 diam. From ovaria of *Nais Jordanii* (Mihi) The body is oval anteriorly, tapering to a point posteriorly—flat or band-like.

a. Is an elongated clean vesicle. It is surrounded by smaller ones, arranged in rows.

Under certain circumstances, especially those of the partial drying of the animal, clear pellucid lines may be seen diverging in different directions from the vesicles, connecting the larger with the smaller, and appearing to traverse the mid-plane of the parenchyme. A gregarina, especially a ciliated species, may be likened to an oval cell with thick walls, flattened so as to bring the opposite sides into contact. The vesicles and sinuses would then occupy the line of contact. The existence of pellucid tubules radiating from the contractile vesicles has been observed recently by Mr. Carter, and before him by Spallanzani and Eckhard. Mr. Carter describes the entire system in *Paramecium aurelia*. In *Glaucoma scin-*

* I scarcely know whether I am justified in alluding to species not yet described. The annelid which I have called *Nais Jordanii*, is a minute, pure white worm, abundantly present in certain states of the weather in the fine sand of Zanglan Bay. It will, I trust, be both figured and described in my forthcoming Report on the British Annelids in the Trans. of the British Association.

tillans, not only the vesicles, but the radiating canals, have been seen also by Mr. Samuelson.* Mr. Carter also states† that the *vesicula* prolong themselves into canals in amœba, actinophrys, and other rhizopoda. Satisfied as to the real existence of these vesicles and canals generally in the protozoa, this excellent observer commits himself to the belief that they constitute "a great excretory system;" and "should this system have any other uses, they are probably similar to the 'water-vascular system' of Rotifera."‡

It is more in accordance with analogy to suppose that it forms a nutritive system; that the fluid impelled by the contractile vesicles is nutritive; that, although at some points it may escape in a direct manner externally, it is not the less a nutrient liquid. In the infusoria there is no "cavity of the body." Even in the cœlenterata, the gastric canal is adherent externally to the solid parenchyme. Analogy, notwithstanding, strongly supports the idea that "the vesicles and sinuses" of the protozoa represent the perigastric chambers of animals higher in the scale. In the gregarinæ, agastric protozoa, and nearly all the infusoria, a system of contractile vesicles and appended "sinuses" (Carter) or tubuli have, then, according to the concurrent testimony of trustworthy observers, *a real existence.* Existing, what do they signify? Can they mean anything but that they are a nutritive fluid system? Is it possible that an excretory system can exist without a receptive and a circulating? If this apparatus of fluid contained in vesicles and canals constitutes a *necessary* and constant integer of these organisms, it undoubtedly implies a great complexity of structure. Such organisms are, therefore, not unicellular, but policellular. But compare a protozoon with an acknowledged *cell*—what a cell is, it is not easy to explain. It is an organic molecule, a vital unit, susceptible, within *certain limits*, of an independent existence. It consists of a centre, a circumference, and an intermediate protoplasm. Which is the *first*, productive, and necessary element, disputants have not determined. Whether the nucleus accretes the protoplasm and the involucrum, or whether the reverse is the real order of growth, observers cannot tell. But take a detached animal cell, one which floats in a fluid, such as fig. 6. A cellule, pregnant with contents, which arises by a spontaneous act in a protoplasmic fluid, what is its life-history? There are two modes of explanation—a cellule in a protoplasmic fluid may arise from the simple hardening or coagulation of the latter. At first a granule, a *solid* molecule, by imbibition, it may attain to the dimensions of a cell, bounded by an involucrum, and filled by liquid or semi-liquid contents; or, secondly, the cell-membrane may be formed first by the

6

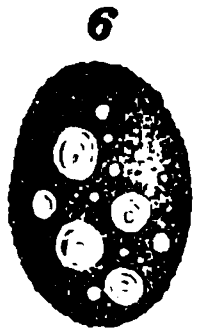


Fig. 6. A single corpuscle from the chylaqueous fluid of *Terebella nebulosa.* It is *filled* with pellucid vesicles and fatty globules. It is bounded by a thin, almost indeterminable pellicle.

* An excellent observer, who read a paper on the metamorphoses of this animalcule at the recent meeting of the British Association.

† Annals, &c., Aug., 1856.

‡ Much of what is very untrue, both anatomically and physiologically, has been palmed upon the credulity of naturalists in this country and in Germany, as to the so-called "water-vascular system" of the Rotifera. In my next paper in this journal, I hope to show that anatomists must change the name of this system and their views as to its uses. The fluid-system of the Infusoria is more like that of the Cestoid Entozoa than any other in the Invertebrate series.

simple hardening of fibrin, by the passage of the protoplasm from the fluid into the solid form. A cavity, a cell, would then be fashioned by the imbibition of fluid from without. In both methods the process would be almost mechanical. It is, *then*, by a *vital* act of the solid tissue thus formed that the cell is shaped into its final figure and size. The imbibing power of the cell-membrane may be easily rendered evident to the eye. The corpuscles of the chylaqueous fluid in *Nais intermedia** are large, granular, globular bodies. They consist of an aggregation of *solidified* protoplasm, and the cell is what it looks, a solid mass (fig. 8). If it be treated with very dilute acetic acid, it assumes the form presented in fig. 7. By a similar treatment the flattened form of fig. 6 may be changed into the globular.

The exosmotic property of these cells is not so readily proved. One of the earliest signs of disease in *Terebella nebulosa* is discoverable in the condition of the cells of the chylaqueous fluid. In the state of full health these bodies are filled with a fatty semi-fluid material of high refracting power. In disease or decay these peculiar secreted contents pass out, and are replaced by the surrounding fluid admitted into the interior of the cell in the unchanged condition. In other words, the cell-wall loses its vital elective power. It is probable, then, that every cell in the living organism is the scene of a never-ceasing double current—the one going in, the other going out of the interior. Osmotic laws, now well established, render such a process readily comprehensible. The mechanism of the nutritive act, therefore, is, in a general sense, the same in the so-called unicellular protozoa as in a simple cell. In the gregarinæ the food is taken in indiscriminately at every point of the surface of the body by imbibition. The food consequently must be in the fluid state. On this point all observers agree. In spongilla, also, this is probably the case. But it is generally agreed that in amœba, actinophrys, the rhizopods, and agastric infusoria, only solid alimentary particles are taken as food. Suppose a particle of solid food, animal or vegetable, to find its way into the substance of the sarcoda. It is closely embraced by the solid parenchyma. It is now liquified. By what? This little globule of fluid is then *imbibed* by the surrounding sarcode—or, by the same process of imbibition, it is first drawn into the vesicles and canals, and by them, aided by pressure, distributed throughout the body. This does not really differ from the surface-absorption as it occurs in the gregarinæ. It is impossible to conceive that the solid particle is *digested* in the substance of the amœba, for this implies a previously secreted special solvent. The nutritive fluid by which the food-particle is resolved is more probably *sucked* in and imbibed by the surrounding parenchyma. It is only *that* process which, in the gregarina, is performed by the surface. If this surface-fluid-absorption

7

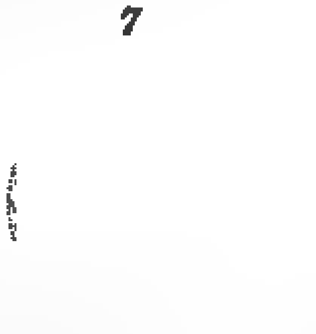


Fig. 7. Corpuscle of chylaqueous fluid of *Nais intermedia*, distended by very dilute acetic acid.

8

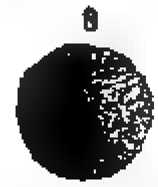


Fig. 8. Corpuscle of the chylaqueous fluid of *Nais intermedia*, much enlarged.

* An undescribed species, very abundant in the fresh-water pools of the neighbourhood of Swansea—distinguished by the disproportionate size of the cells of its cavitary fluid.

does not occur in the case of amœba and actinophrys, it is because in them the *pellicula* differs in its chemical properties from that of the gregarina. This is a strong ground for believing that the cuticle differs structurally as it differs in its properties from the sarcode substance underneath—that it is not the latter simply solidified. These facts clearly prove that there are few points in common between a simple cell and a so-called unicellular protozoon. The simplest animal is far more complex than is implied in the word "unicellular." The faculty of locomotion, even by ciliary agency, is unknown amongst "cells." Cilia occur only on fixed, sedentary cells; never on the floating variety. They are designed to impart motion to an external body, not to carry the cell itself from place to place. The system of contractile vesicles and dependent sinuses, so general in the least organized protozoon, is unknown in the history of cells. They are not comparable even to the circulatory system of the vegetable cell. The "sarcode"—a most indefinite term—is without example amongst "cells," unless the semi-fluid substance which has lately been described by Dr. Walter, of Bonn, as contained in the muscle-cells of the entozoa, be so defined. Its composition may be inferred from that of the food upon which the protozoon subsists. If *many* cells have been engaged in producing the adult amœba, it cannot be said to be a mono-cell.

Fluid-absorption by the surface is the normal method of feeding, then, in these low types of animal life. This absorptive faculty is an inherent property of the substance of which they are composed. It attracts certain aliments as gelatine attracts water. Tissue, distinguished by the same character, prevails throughout the entire class of the entozoa. It is important to remember this fact. Why it should be called "sarcode" more than any form of flesh, is difficult to understand. It forms a chief constituent of the bodily parenchyma in those animals in which the *surface* fulfils an important part in the mechanism of alimentary absorption. In the cestoid entozoa it will be afterwards found to constitute a predominant structure.

It will be rendered probable in the course of this inquiry, that, although imperfectly-defined channels for the reception and distribution of fluid do really exist in the trematode and cestoid worms, as in the protozoa, such channels play a very subordinate part in the general machinery of the nutritive acts.

The reader is now prepared to enter upon the study of the fluid-systems of the *entozoa*. The *nematode* worms are not only zoologically, but histologically distinguished, and that strikingly, from all other entozoa. They differ in the chemical properties of the chief solids; they differ still more remarkably in the characters of the fluid-system. In all cases the body is cylindrical in figure. It is traversed from one end to the other by an alimentary tube. From that of the trematoda, this tube is distinguished by the existence of a posterior orifice. The nematodes are furnished with a general cavity, which is almost entirely filled with a vesicular tissue, in which a chylaqueous fluid is lodged. A vaso-fluid-system does not here exist—*organa genitalia segregata*. These are the leading distinctive features of the nematodes. Each deserves a separate study. Let us begin with the *integuments*.

The design of this course of study is not only to investigate the history of the fluids, but to explore the structure of those solid organs which may explain the anatomical position of the fluids in the organism, or illustrate their chemical composition. The integumentary covering of the nematoids, in this sense, will be found to play an important part. Vibratile cilia do not exist in this class. Neither within, on the viscera, nor on the cutaneous exterior, have they been detected in a single instance. The entire surface is smooth (the spines of some species excepted). There are no express provisions for breathing. In no known class of animals is this office of so low a standard. *The fluids of the body are not in motion as they are in the annelids.* A *fixed* fluid system is an anomaly in the animal kingdom. Why this is the case in the nematoda will be afterwards explained.

The first fact to be noticed in examining the tegumentary system of the nematoid worms, is the complete absence of those peculiar follicles (fig. 16) which constitute so marked a character of the cutaneous surface of the planariæ and the nemertidæ, and less distinguishably of the trematoda.*

No *fluid-bearing* process of any kind can be seen to rise above the plane of the epidermis. This fact is by no means devoid of interest: it proves how little these worms, in a respiratory sense, are under the influence of the medium in which they live; it proves that they do not respire by the surface; it proves specially, that no gas *in the aëriform state* can penetrate through the integument into the visceral cavity. There is no cutaneous plexus of vessels. If, therefore, there be in these entozoa a distinct respiratory function, it can by possibility consist only in this—that oxygen *in solution* is carried into the system of the worm by the surface-absorbed fluid. With this supposition, the entire physiological history of these animals is conformable;—sluggish muscularity, low motive and sensitive powers, a fixed fluid system!

The integuments in this family of entozoa have been described by all writers in nearly the same language. It is "thick and cartilaginous" (Nelson); "chitinous" (Walter); "dense from fibrous corium and structureless epidermis" (Meissner). Siebold† says, "The body of the helminthes is generally surrounded by a firm skin, which may be separated into a thin epidermis and partly hard *dermis*."

Meissner, in his account of *Gordius* and *Mermis*, describes the epidermis as *structureless*. Walter applies the same word to that of *Oxyuris ornata*. If by this word it is to be understood that the epidermis in the nematodes forms one continued *amorphous* sheet over the entire body, the author can only state that is a word which will lead the student to the most egregiously false knowledge. In the genera *Ascaris*, *Strongylus*, *Oxyuris*, and *Trichosoma*, which are so common and familiar, this question can most easily be put to the test of observation. The following account of the integuments has been more especially drawn from the study of the

* It has surprised me to find that Meissner (Zeitschrift für Wissenschaftliche Zoologie, 20 Mai, 1855), in his paper on *Gordius* and *Mermis*, does not allude at all to this point of structure. In *Gordius aquaticus*, which is by no means uncommon in this neighbourhood, the cutaneous follicles are very obvious.

† Anat. of Inv., by Siebold and Stannius, trans. by Burnet, p. 103.

following species—viz., *Ascaris lumbricoides*, *A. megalocephala*, *A. capsularia*, *A. truncalata*, *A. mystax*. But the author believes that it will apply, with slight modifications, to the entire nematoid family.

The epidermis is not a homogeneous sheet, but a highly-organized covering. It consists of two distinct planes of dense cartilaginous scales (fig. 9, *a*, *c*); each scale is a long parallelogram: it is four or five times as long as it is broad. The adjoining scales are united together by minutely granular lines, in the course of which, at regular intervals, are observable a stellar arrangement of granules or threads (*b*). Underneath this superficial layer lies a second, the scales of which course at an oblique angle with reference to those above them. The two sheets thus formed are cemented together at the granular lines and points (*b*). These two strata form a dense, but at the same time very transparent, envelope. Though dense and cartilaginous, each individual scale is perfectly structureless and diaphanous. A large piece of this epidermis may be most readily detached. It is not possible to discover, throughout the extent, of such a piece, any *perforations* whatever. But the entire epidermis is firmly attached to the underlying corium by means of a dense mass of minute threads. It follows that, if any fluid passes from the exterior into the cavity of the body, it must traverse this layer in one of two methods —either by endosmosis through the diaphanous substance of the epidermal scales, or through the lines and points by which they are at once separated and united. That the surface is capable of absorbing fluid in these worms, is beyond disbelief. It is possible that follicular perforations may have eluded the search which has been made for them; but the fact of the faculty of absorption cannot be disputed.

9

Fig. 9. Epidermis of Ascaris.
c. Uppermost layer of parallelogram scales.
a. The undermost layer.
a. b. The connecting lines and dots.

The next layer is called the *corium* or *dermis*. It is described by all authors as consisting of a fibrous structure. Walter, the most recent observer, defines it as chitinous-layer: "Das Corium besteht aus einer glashellen homogenen, dicht mit der Epidermis verbundenen Substanz." It seems as if he saw no distinction between the dermis and Epidermis. There is a very marked one. As this structure constitutes a part of, and is in intimate connexion with, the *vesicular* tissue, immediately to be described, before proceeding further let us dispose of the muscular element of the tegumentary system. It is commonly said to form a dense web of fascicles, running in various directions, but blended into a solid layer. The error of this account may be readily proved. The muscular layer consists of two planes: the outermost is made up of large visible bundles

or fascicles. These latter embrace circularly, and with great regularity, the body of the worm. Each fascicle is constituted of a great number of secondary tubules.

The circular disposition of the primary bundles it is which gives an *annular* wrinkled character to the integuments of the large-sized nematodes. But a clear distinction should be drawn between this annular appearance and those deep-laid segmental formations which characterize this system in the annelids. Underneath the circular layer of muscular stratum lies one whose fascicles observe a longitudinal direction; but this layer is very much less developed than the circular.

The ultimate fibre presents all the characters of a tube or cylinder. The walls are streaked with minute longitudinal threads, between and amid which granules are interspersed (fig. 10, *a*, *b*).

The interior of each cylinder is apparently filled with a homogeneous semi-fluid sarcode substance.

Walter states that he has observed the sarcode flowing out of these tubes in *Oxyuris Ornata!* If this be a fluid, of course it cannot be the seat of the contractile power of the muscle. This must reside in the fibrous wall of the tubule.*

No transverse striations occur in the muscles of the entozoa. The whole type of the organized solids is below such a possibility.

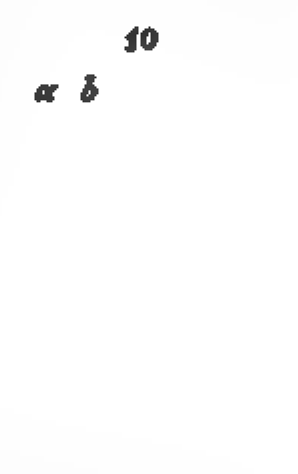


Fig. 10. Ultimate muscular fibres of *Ascaris megalocephala*.

To the right of *b* their tubular character is shown.

At *b* is represented the minute threads or fibres of which the walls of each tubule is composed.

At *a* is seen a tubule in its perfect state, filled with a granular fluid.

In connexion with the tegumentary system, should be spoken of those peculiar reddish lines most evident in the genus ascaris, especially in the lumbricoid species, which run along the sides of the body from the head to the tail. These lines are shown in section in fig. 11, *a a*. (p. 470.)

By all anatomists they are described as *vessels*.

Ascaris lumbricoides of the sheep or lamb is most suitable for an inquiry into their structure. By a little manipulation they may be detached as threads in long pieces. Thus placed under the microscope, it may be supposed that their real nature may be determined with facility. The point, however, is very difficult. The chief bulk is made up of solid muscular and nervous fibres. In the centre courses a hollow, fluid-bearing channel. It is very small, compared with the thickness of the entire band. This hollow channel is undoubtedly filled with the cavitary fluid. This fact is proved by throwing a thin coloured injection along the line of the intestine. It fills this channel. In fig. 12 is represented a longitudinal section of this red line, slightly compressed, under the microscope. It is composed of four elements. The first are the pigmented granules, to the presence of which is due the colour of

* Professor Ellis, in a paper lately laid before the Royal Society, thus describes the difference between voluntary and involuntary muscles:—"In neither voluntary nor involuntary muscles is the fibre of the nature of a cell, but in both is composed of minute threads or fibrils. Its surface-appearance in both kinds of muscle allows of the supposition that in both it is constructed in a similar way—namely, of small particles or 'sarcous elements,' and that a difference in the arrangement of these elements gives a dotted appearance to the involuntary, and a *transverse striation* to the voluntary fibres."—'Proceedings,' No. 22.

these so-called vessels. These granules are chiefly situated on the exterior, but they are also distributed between the fibres. Next is observed

Fig. 11. Transverse section of *Ascaris lumbricoides* of the sheep.

a. a. Lateral red bands in section. *b.* Intestine.
c. Abdominal nervous chord. *d.* Reproductive organs in section.
e e. Innermost longitudinal layer of tegumentary muscles.
f. Second layer, or circular muscles. *g.* Corium.
h. Vesicular tissue.

the fascicles of the muscular element, the structure of which is the same as that of the circular bands already described. But amongst the latter is seen a thread, distinguished from them in structure. It is a small nervous cord, the course of which is traceable by the fatty, high-refracting substance by which it is filled. This optical character belongs to the nerves of all the tissues in the entozoa. In the echinodermata, nearly all nervous threads are accompanied by a more or less distinct deposit of *red* pigment granules. In the absence of *red* blood, it seems to answer the purpose of hæmatosine in attracting oxygen. This is but a supposition. If there be in it any truth, it would equally apply to the corresponding coloured lines in the entozoa, for it should be stated that the abdominal nervous cord in ascaris is also surrounded by red-coloured granules.

12

Fig. 12. Longitudinal section of red lateral lines of *Ascaris lumbricoides*.

The central fatty band indicates the nervous cord; those on either side, the muscular fascicles.

It is thus shown that the lateral reddish lines in *Ascaris lumbri-*

coides are not vessels. If *they* be not vessels, *there is no other vascular system in any species of nematoid worms.* This statement does not rest exclusively upon the facts just stated. It is corroborated by an extensive search among several of the nematoid genera. The author, after numerous and difficult examinations, has concluded, that in the nematoid entozoa neither a water-vascular nor a "blood-vascular" system exists. This conclusion, although negative in its bearing, is of service to science. It determines the zoological standard of this family of animals. It sheds a ray of light upon the ill-understood fluid-system of an interesting class. In presence of a degraded apparatus of organized solids, a complex hydraulic machinery is not found in nature. If in these worms there obtained a vaso-fluid system, then every system of solids in the organism would be raised in standard, and the entozoon would become an anomaly in the animal scale. But these reflections are premature. It should be stated in this place that a structure having apparently the same character as that of the longitudinal lateral bands in *Ascaris*, has been described by Meissner in *Gordius* and *Mermis* as a "Secretions-organ."*

In interpreting these parts, Meissner seems to be guided by the views of Siebold and Van Beneden with reference to the "water-vascular system" and the so-called "caudal vesicles" of the trematode, cystic, and cestoid entozoa. The author will adduce strong evidence in his next communication to this Journal, to show that this analogy is without any foundation in truth. If, as supposed by Meissner, they are excretory organs, there is no *such* excretory organs in any other annelid. In the nematoids, the lateral bands are not an organ of excretion. This point is beyond doubt. In this place, therefore, it is not necessary to say more on this subject. But let it be remembered that the vessel-like channel which is embedded in the lateral longitudinal bands of *Ascaris* communicates freely with the general cavity of the body, and that the fluid by which it is filled is identical with the cavitary fluid.

If these, so-called by the German anatomists "secretions-organe," were organs of secretion or excretion, they should be present under some form or other in all the genera of this order. The truth is, that the lateral bands are detectible only in the genus *Ascaris*. In the smaller nematoids, no trace of them can be discovered. In an account of the anatomy of these worms, Lieberkühn says,—"Die sogenannten Seiten-, Bauch-, und Rücken-linien habe ich nicht mit Sicherheit auffinden können."†

Leidey‡ does not allude to them in his description of Ascaris infecta. The fact is, that in the present imperfect state of knowledge with reference to the *nature* and mechanism of secretion in the lower invertebrata, secretory and excretory offices are assigned to parts and organs which have nothing whatever to do with such functions.

* In *M. albicans* and *M. nigrescens*, Meissner figures three "secretionsorgane," of which one rests on the abdominal nervous cord, the other two being situated one on either side. In *Gordius* there is only one—the abdominal. He gives an extraordinary account of these "secretions-organe." They are tubular ducts, which begin in open orifices near the mouth, and end in open outlets near the tail. These tubes are filled with cells. The cells are excretory products! Chauvet, quoted by Meissner, believed this canal to be connected with the reproductive system. Bertholt and Siebold, again, express different opinions. Vide Zeitschrift für Wissenschaftliche Zoolog., 20 Mai, 1855.

† Müller's Archiv, 1855. ‡ Flora and Fauna of Living Animals, 1853.

At a future stage, this question will be further discussed.

Let us now revert to fig. 11, in order to consider another important element of the integumentary system of the Nematoidea—namely, *the vesicular tissue and the corium*, (*g*, *h*, fig. 11.)

The author would define the "vesicular tissue" as consisting of a mass of pyriform vesicles depending into the cavity of the body from the internal surface of the integuments, each vesicle having its base directed towards the intestine, and its neck towards the cutaneous surface.

In some genera, so considerably developed is this tissue, that it literally fills the splanchnic interval which divides the intestine from the integument; in others, on the contrary, it is much less evident. Under the latter circumstances, a clearly-marked perigastric cavity exists. This tissue does not appear to be unknown to anatomists. By different observers it has been variously interpreted.

Charvet thinks that it forms the glandular origin of two canals which are said to unite in the caudal vesicle. Berthold associates it with the generative system; Dujardin assigns to it a muscular nature; Siebold partakes in the opinion of Charvet, and compares it to the "Pflanzenparenchym." Meissner also speaks of "der grossen Aehnlichkeit der Zellen des Zellkörpers mit Pflanzenzellgewebe." Meissner's description relates to this cellular body only as it occurs in *Gordius* and *Mermis*. He figures it as lined by a distinct membrane, and consisting of cells, polyhedral from pressure, *in each of which is contained a well-marked nucleus*. He then proceeds to show, by the use of various reagents, its chitinous composition. He seems to be of the opinion of Siebold, that it constitutes an organ of secretion, and further suggests that it may have something to do with the formation of cellulose.*

The following description has reference only to this tissue as it occurs in the nematoid entozoa. It has been stated already, that the corium (fig. 11, *g*.) consists of a cellular layer, which lies immediately underneath the thick, leathery, yet transparent epidermis. It is connected above with the epidermis, but its most important relation is with the vesicular tissue beneath. An inspection of fig. 11. will render this evident. It is seen that the *necks* of a large number of the pyriform vesicles (*h*) pass through the layer of muscles (*e* and *f*) outwards, as far as the corium or dermis, in which they are lost. The structure of the dermis is precisely the same as that of vesicular tissue. It forms but a thin layer. The leathery, dense character of the integument is due, not to the corium, but to the epidermis. The great mass of the vesicular tissue is adherent, and forms a flocculent, spongy lining to the internal surface of the integument. Every vesicle does not pass out into the tissue of the dermis. The great majority run together to form an areolar layer on the internal surface of the muscular stratum. The areolæ of this layer

* At a future time, when in the course of these papers it becomes my duty to treat of the fluid system of Nemertine Annelids, it will be rendered very probable that Meissner has mistaken the ovarian system for the "Zell-körper," and that which he calls the reproductive organs, is a part of the alimentary system. De Quatrefages' account of the organization of the Nemertidæ, and Meissner's of the Gordiaceæ, *cannot both be true*. I am at present strongly of opinion *that both are wrong*. De Quatrefages makes no allusion to anything approaching to this cellular body as figured by Meissner in *Gordius* and *Mermis*, in his account of the anatomy of the Nemertine Annelids.

are not so uniformly and regularly Florence-flask-shaped as those of the tissue which depends from it into the cavity of the body. The vesicles of the latter are single and independent. *They are pyriform bags, filled with a fluid.* This fact can be proved incontestably in various ways. The first proof is the optical. To a microscopically-trained eye such an inference would be at once accepted. They are capable of being filled with fluid, either by immersing a fresh, but slightly-dried specimen in a thin coloured fluid, or by injecting such a fluid indiscriminately into the cavity of the body. The broad end or base of each vesicle is turned towards the intestine or central axis of the body. From the base of each vesicle there extends a thread-like process of areolar tissue, which ties it to the viscus. In the genus *Ascaris* this tissue is thus much more extensively and intimately connected with the intestine than with the generative organs, which, like the former, are cylindrical tubules occupying the axis of the body.

This arrangement may be supposed to prove that the fluid contents of the vesicular tissue is derived directly, by absorption, from the intestine. Such a conclusion is not necessarily true. These vesicles are capable of being filled by the absorption of fluid through the cutaneous surface. During life, the nematoid entozoa are endowed with the power of controlling this absorptive power—of increasing or of decreasing it at will.* A directly contrary statement, however, is made by Siebold. He remarks that these worms, *when put into water*, die by bursting. This only proves that they are rapidly killed by the medium in which they are immersed. It does not prove that in their natural element—the animal fluids—they are incapable of controlling this absorptive faculty. What, then, is the physiological office, and what is the homological history of this tissue? It exists in all the nematoidea; but it is less developed in the viviparous than in the oviparous orders. In the former,† the cavity of the body is almost as distinctly marked as it is in the annelids; in many species of the latter, the perigastric space is almost entirely filled by the vesicular tissue. In no case, however, is this cavity completely obliterated. It exists, and is charged with a considerable bulk of fluid, even in *Ascaris lumbricoides*, in which this tissue is most exuberant. This fact may be placed beyond doubt by snipping through the integuments near the tail, and then holding the worm with the head uppermost for some time over a cup. A large quantity of fluid will rapidly flow out. It is quite certain that if there were no free open space between the intestine and integument, the fluid could not thus readily escape; for if it were lodged entirely in the cells of the pyriform tissue, it could but slowly flow out.

* Siebold says, "This absorbent power is particularly prominent in the Acanthocephali. It is here really a vital act." The discovery of the vesicular tissue which I have described, and with which Siebold does not seem to be acquainted in the least, *explains this absorptive power*. This distinguished observer further states, that the *Echynorhynchi*, which naturally absorb only a little liquid into their flattened and wrinkled body, will swell and relax alternately when in contact with water.

† To this fact my attention was first drawn by Professor Busk at the Cheltenham meeting of the British Association. Since then I have carefully repeated many of my former dissections. In *Strongylus auricularis*, *Ascaris acuminata*, and *Ascaris trigonura*, which are viviparous, I find that the vesicular tissue encroaches upon the general cavity less than in the oviparous species. In examining the above species of viviparous entozoa, I could not convince myself that the young were contained *in the general cavity of the body*. Professor Busk, however, stated that in the guinea-worm that was really the fact.

Even by transverse sections, it is, however, possible to show, in several species, that a distinct unoccupied interval exists on one side of the intestine (the tissue being closely attached to the other), in which the cavitary fluid has a free longitudinal channel. In the small ascarides which are found in so great abundance in the intestines of fishes, by a transparent view it is very easy to bring directly under the eye both the disposition of the vesicular tissue and the splanchnic space which intervenes between the intestine and integument.

16

Fig. 16. A transparent view of a portion of the mid-body, a small transparent nematoid worm from the peritoneum of cod.
a. Intestine.
b. Ovary.
c. Same, filled with fat.
d. Spaces, constituting the general cavity of the body.
e. Vesicular tissue.

In many instances (as in fig. 16) the tissue assumes the form of bridle-like processes, extending from the integument to the intestine, and tying the latter so firmly down as to render the longitudinal to-and-fro-motion of the intestine, so characteristic of the alimentary system of the annelida, quite impossible. Regular deposits of fat are observed in many cases (fig. 16, *c*), which may be compared to the segmental developments of the annelida. It is, then, to be understood that *in no species* does the vesicular tissue obliterate the peri-visceral space. It is present in all cases, though more markedly in some instances than in others. In all cases it is occupied by *a fluid, which is the only nutritive fluid in the organism of the nematoid entozoon.* Before proceeding to consider the history of this fluid, let us revert once more to the vesicular tissue. Is it the office of this tissue to draw fluid into the body by vital or mechanical absorption from without, or is it designed to excrete from within? Can it be said to be homologous with that remarkable system of cutaneous follicles (the bases of which being here turned outwards, the neck being directed inwards) which crowd the entire surface of the body in nearly all the turbellaria, and which almost equal in size the membranous processes on the tegumentary surface of the asteridæ? or is it a distinct and special formation? Neither of these questions can be at present solved with certainty. The author, however, strongly inclines to the view which regards the vesicles in the light of *absorbent organs*, as provisions specially destined to replenish the general cavity of the body with fluid. The parietes of each vesicle are studded with minute granules. In no single instance is it possible to discover a "nucleus" such as that which Meissner has described in the falsely-called vegetable celled parenchyme of *Gordius* and *Mermis.* But if this vesicular tissue is *not* an absorptive apparatus, what other means exist for the exercise of this function? Is there any peculiarity in the walls of the intestinal tube which is capable of answering such an end? The wall of the alimentary canal in the nematoids is formed on the type of that of the annelida.

It presents no trace whatever of a vascular system. It is composed of a peritoneal layer, of a very slender muscular, and of a glandular layer (fig. 13). The biliary cells are nucleated capsules filled with molecules. The intes-

tinal wall in the sepuncles is studded with peculiar *ciliated* follicles, the object of which is obviously to take up rapidly from the alimentary canal the liquid required to replenish the chylaqueous fluid. But of such structures no examples are to be discovered on the intestinal walls of the nematoids.

In the present state of knowledge it must therefore be concluded, from the absence of all apparatus at the mouth for sucking, from the immovably fixed and straight alimentary canal ill-adapted thus for holding large quantities of fluid, from the external circumstances under which they live, from the peculiarly suitable structure and arrangement of the vesicular tissue, from the known absorptive capacity of the integuments, that in these worms *a very large proportion* of the cavitary fluid is derived directly by absorption from without at the tegumentary surface of the body.

13

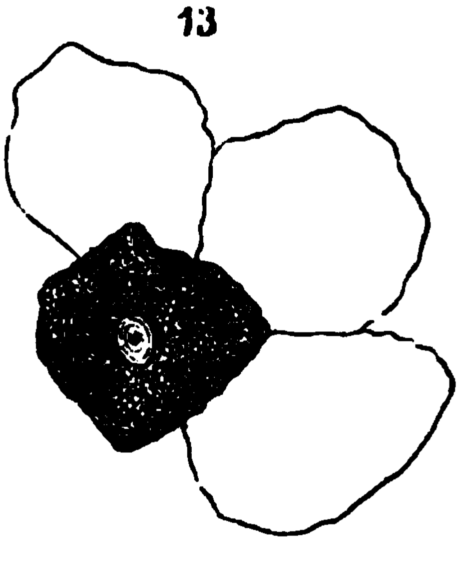


Fig. 13. Biliary cellular layer from the wall of the alimentary canal of *Ascaris lumbricoides*.

One cell is shown in the full state. The others are in outline.

The author is desirous to introduce one more illustration of the histomorphous capacity of the solids in these lowly-organized beings before proceeding to a special consideration of the fluids. Out of one common source, the fluids, different solids are moulded in the lowest as in the highest organisms. In this process of appropriation the fluids are passive, the solids are active and positive. Take corresponding parts of the same organ from Ascaris lumbricoides of the sheep, and note the extraordinary fact that beneath an exterior of perfectly similar conformation there lies a singular histological difference.

The testis of the male has the same general conformation as *one* of the tubular ovaries of the female. Both commence in a slender cæcal tube, which slowly grows larger and thicker until it reaches a dilated portion, which in the female is the uterus, fig. 14 *a* (Nelson); Eiweiss-schlauch (Meissner); in the male, the vesicula seminalis (Nelson), fig. 15 *b*. Dr. Nelson has most accurately described the female organ, but neither he nor Meissner alludes to the following interesting peculiarity in the structure of the male organ. If the *lining membrane* of the male and female organ be compared *stage for stage*, commencing at the fine cæcal end, and ending at the dilated portion, it will be found that at the first stage (that at which the germinal vesicle is formed in the female, the nucleus of the sperm-cell in the male), the *mucous or lining membrane* is precisely the same in both; at the second (corresponding with the "Dotterstock" of Meissner, and the Vitellarium of Nelson), the lining cells of the membrane in the female tube have increased in size, but are still oval in form, having a very conspicuous nucleus; in the male tube they have a very distinct pyriform shape, being attached to the sides of the tube endwise, forming thus a villous coating; at the third stage (uterus, Nelson; Eiweiss-schlauch, Meissner), the two series of cells are found to have diverged from each other to a remarkable degree (compare a'', fig. 14, with b'', fig. 15). In the female series (a'', fig. 14), the cells have deviated little from their original form; they are still large elliptical bodies, having a very evident

clear nucleus in the centre, the space between it and the involucrum being filled with granules. In the male tube at the same stage the cells have acquired an extraordinary spider-like form. The nucleus is so small as scarcely to be detected, but the membrane of the cell-wall (b'', fig. 15) has thrown out singular pseudopod-like processes, which,

14 15

b

Cells of the mucous lining of the female (fig. 14) and male (fig. 15) generative tubes of *Ascaris lumbricoides* of sheep.

a. uterus of female, laid open, showing the large ova-like cells on the internal surface. At a'', a few of these cells are represented, still further magnified.
b. Seminal vesicle of male, opened, in order to show the peculiar cells by which it is lined.
b''. Two or three cells, enlarged.

when the cells are *in situ*, mat themselves together into a thick felt with those of the adjoining cells. These processes are *hollow*, and contain the same granules as those which are seen in the centre of the cell. Higher up in the tube they are projected only from *one side* of the cell. Thus, as the generative tube is traced downwards, they gradually grow from the pair-like into the crab-like form.

These cells respectively constitute the producing or stromatous tissue in the male and female generative tubes. They demonstrate two directly opposite modes of growth in cells—one centripetal, the other centrifugal—one in which the nucleus and contents increase, the other in which, *at the expense of the nucleus and contents*, the cell-membrane acquires a considerable development.*

They show that from a common starting-point two series of cells, produced by tubes of apparently the same precise structure, and by one and the same nutritive fluid, may yet conceal beneath identical exteriors formative powers capable of impelling them, in the march of growth, towards two widely-separated destinations. Such *facts* convey to the physiologist, though distant, yet correct conceptions as to the nature of the "vital force." Certain elements of the blood pass through a simple membrane, yet in one case they emerge as the germ, in the other as the sperm-fluid! But how remarkable it is that the fluids of the lowest animal should be endowed with the same histomorphic power as those of the highest! In both cases, a plain incomplex membrane attracts a protoplasm out of the fluids which *cellulates* into the same germinal vesicles! It follows, that in one sense the fluids of the lowest animal are equal to those of the highest,—that the same elements are present in both. When the physiologist has acquired a correct knowledge as to the number and variety of solid organized parts which the fluids in any given case are capable of producing, he has reached a point of information which no analyses of the fluids themselves would enable him to attain.

A strong argument in favour of this conclusion will afterwards be drawn from the character of the solids in the trematode, cystic, and cestoid entozoa.

The nematoid entozoa, as already stated, are destitute of every trace of a vascular system. An apparatus of vessels in these worms is, notwithstanding, described by all authors. Too trusting confidence has been given to the delineations of Emile Blanchard. They were taken from artificial injections. They have deceived both the operator and his admirers. The proof of the absence of a vaso-fluid system rests upon two methods of examination: in the larger specimens, by dissection and by the microscope, in the smaller, by directly viewing the body as a transparent object.

In this family of worms there exists only *one system* of fluids—which, for brevity sake, may be called the *cavitary system*—in contradistinction to the vaso or vascular system, so often present in the annelids. It is contained in part in the free chamber of the peri-visceral space, which, in the nematoids, as formerly explained, is in all species narrowed, in some almost obliterated, by the encroachment of the vesicular tissue, in part in the vesicular tissue. There is one very marked peculiarity about the cavitary fluid of this class of worms—namely, *that it is altogether destitute of every form of floating corpuscle.* This is the only example of such a peculiarity that the author is acquainted with in the whole history of

* I wish the reader to understand that this is a mere *mode of expression* with reference to the growth of cells. It is highly probable that the views so ably advocated of late by Mr. Wenham will oblige physiologists to substitute for these old modes of expression a more exact terminology.

the *cavitary*-fluid system of the invertebrata. It is the rule as regards the vaso-fluid system of the annelids. So little is known of the nutritive liquids of the trematode and cestoid entozoa, that at present it cannot be affirmed with certainty whether they are corpusculated or not. The cavitary fluid of the nematodes consists of a smooth, oily, homogeneous liquid, having a slightly yellow tinge, and entirely amorphous under the microscope.

It is probably of high specific gravity, as it is of great apparent density. It strongly resembles the *serum* of the blood of a vertebrated animal. It looks very much more as if it were sucked directly into the body of the parasite, than as the product of the digestive agency of an animal so low in the scale. Sufficient quantity in the larger ascarides and strongyli may be readily collected to test its chemical properties. It is a *thick solution of albumen.* If placed in a clean watch-glass, *no coagula* of any description are formed under any circumstances. It leaves a dense smooth skin of solidified albumen on the glass, after complete evaporation. Acids throw down a thick body of albumen. From these facts the inference may be drawn that this fluid *does not contain fibrin* or any analogous self-coagulating principle. Why should the cavitary fluid of the nematodes possess these remarkable characters? Why should it be so much more rich in albumen than its homologue in the annelids, the latter animals being so much higher than the former in the scale? The chylaqueous fluid of the annelid is a watery corpusculated fluid; the cavitary liquid of the nematodes is as dense as the serum of a vertebrate animal. Is it not beyond doubt that, in both cases respectively, the fluid owes its properties to those of the medium from which it is drawn? The annelid lives in water, the nematode in blood! But the fluids of the nematode worms are *motionless, stagnant in the body!* In the annelids they are in constant movement. This is a peculiarity still more striking than the former. The fluids are sluggish, like the parasites themselves. The chemist will at once see that *motion* is an important accessory to all chemical operations. If the densely albuminous cavitary fluid of the nematodes were the product of blood-making processes, having their seat exclusively in the body of the worm, is it not wonderful and contrary to all analogy that these processes should not be accompanied by the mechanical circumstances of *motion?* Why there is no motion of the fluids, it is easy at once to understand. The intestine is so completely tied to the integument, that it can neither roll laterally, nor lengthen and shorten in a longitudinal direction; but the epidermis is so leathery and inflexible, that no liberty of motion is possible in any of the enclosed parts. The integuments of the nematode do not exhibit that undulatory movement which, in the sipuncles, drives the cavitary fluid incessantly and with great force from one end of the body to the other. The absence of fibrin from the nutritional fluid of these entozoa associates itself (causally?) irresistibly with the low development and sluggish character of their muscle-system. The absence of the mechanical circumstances of motion may, too, explain the absence of corpusculation. The vesicular tissue, and its undoubted absorptive power, cannot be separated from the fact, that the fluid by which that tissue is filled resembles most closely the serum of the animal upon which the worm is parasitic.

Art. II.

Influence of the Climates of Peru on Pulmonary Consumption.
By Archibald Smith, M.D.

The various climates of Peru, as changed or modified by the measure of elevation from the sea, and other local causes, are not merely curious to the meteorologist, but more especially interesting to the physician; they are, above all, important in relation to the development in some localities, and the disappearance in others, of phthisis; this point I shall now endeavour very shortly to illustrate.

1. *What are the Localities or Climates in Peru in which Phthisis is most and least Prevalent?*—This disease is properly a product of the warm and humid valleys of the coast, such as that of the Rimac. And from Lima, where you have an extensive view up this valley, to the loftiest snow-clad peaks of the Cordillera, every gradation of climate is unfolded in the intervening distance, that one would have to pass through, in a voyage of many days, from Callao to Cape Horn. And in the inland glens beyond (as in the often-mentioned vale of Huanuco), we have noticed how the extremes of climate are brought within much nearer limits than those embraced even in this picturesque and imposing bird's-eye-view from Lima—especially from the bridge, looking eastward. Nor is there, in all this range of climate, a locality in which phthisis is more prevalent than in the mild and equable temperature of the capital and its immediate environs.

Piura, the most northern province of Peru, though within two or three degrees of the endless moisture and vegetation of the Equator, is yet the most hot and arid in the republic. Its maritime district is also considered the most healthy on all the coast, and remarkably free from pulmonary disease or consumption. In the pastoral sierra—viz., on the lofty slopes and colder plains of the Andes, pleurisy and pneumonia are not unfrequent; and sometimes terminate in the worst manner, by suppuration or gangrene, when left, as usually happens in remote Indian villages, without medical assistance.* Phthisis pulmonalis is, I am persuaded by a long residence in these mountain regions, little known to the native population, except as imported to the hill-land from the coast. In those warmer valleys in the centre of the Andes, where the temperature is sufficiently favourable to the growth of the banana and sugar-cane, we meet with frequent development of hepatic disease; and when the climate is particularly warm and humid, as in the province of Huamalies, on the borders of the Montañer, we even meet with severe examples of ague; and these situations are but ill calculated to restore the health of a consumptive invalid. I resided for several years in the vale of Huanuco, which—as previously mentioned—is dry, and free of

* The Indians have many native remedies for what they call *dolor de costado*, or pleurisy; but I found, in Cerro Pasco, bleeding, followed up by tartar emetic, most efficacious. In the Sierra, bleeding is better supported than on the coast, where twelve ounces of blood is a large bleeding among the white Creoles. In Lima, where the lancet cannot be used safely, tartar emetic, pushed in small doses, alone or combined with morphine, to the extent of from twelve to twenty grains, generally subdues either pleurisy or pneumonia.

malaria, at an elevation varying, as it extends along the banks of the river, from six to seven thousand feet above the level of the sea, with the thermometer rarely, throughout the entire year, above 72°, or below 66° Fahr. in the shade. But this climate, though equable, did not prove favourable to the convalescence of phthisical patients brought there from other parts of the country; and I cannot say that I ever saw a case of phthisis originate in that locality. In like manner the cold—often damp and variable, and always highly rarefied—atmosphere of the mining district of Cerro Pasco, and other localities near the snow-line, is unfavourable to recovery from phthisis. But Huarriaca, which lies in the descent from Cerro to Huanuco, is very favourable to such recovery, as I had frequent occasion to test when at the mines; whence we usually sent our patients from pulmonary affections to convalesce at this desirable place, distant eight leagues of pretty rapid descent from the silver mines of Cerro Pasco. Huarriaca is in climate very like Obrajillo, on the western slope of the Andes, and is one of those recesses in the Andine glens and defiles very productive in maize, corn or wheat, potatoes, beans, and natural pastures on the heights, as well as cultivated lucern on the straths. Such, indeed, are the marked localities, blessed with a steady temperate climate, and a dry air of about 60° Fahr. in the shade, as well as sunny cheerful sky throughout the greater part of the year. Such are the localities where phthisis proper, or tubercular disease of the lungs, is only known as an exotic!

2. *The Proportion of Deaths by Phthisis compared to other Diseases on the Coast of Peru.*—This proportion, for want of satisfactory statistical returns, can only be answered at present in respect to the capital, and even there only approximately. From data before me—let us take the mean of ten years, say from 1841 to 1850, inclusive,—the average may be struck at 3200 deaths annually, of all diseases, in Lima. Of this gross sum, the monthly hospital returns account for 1700; while a somewhat less proportion—viz., 1500, are indiscriminately entered in the report of the general cemetery under the title "various diseases." But from the more specific evidence as to details, furnished by the hospital reports, I will here state the average mortality for the ten years, given, in 1700 cases yearly, as follows:

From fever	600
From dysentery and chronic diarrhœa	480
From pleurisy and pneumonia	160
From phthisis pulmonalis	320
From sundry other diseases	140
Total	1700

Thus, next to fever and dysentery, phthisis was the most fatal disease known in Lima up to the first visitation of yellow fever in that country, from the years 1851 to 1854, as described and recorded by me in No. 203 of the 'Edinburgh Medical and Surgical Journal,' but with which, in our present estimate, we have nothing to do. If it can be shown in this way, that in 1700 hospital cases of fatal termination annually, 320 of these deaths proceed from phthisis, we arrive at an average proportion for the whole mixed population of Lima admitted to

hospital treatment. And further, if we put to one side the indefinite number of deaths from infantile diseases, among the 1500 indiscriminately sent from the different parochial wards of the city, and included under one common head—viz., "various diseases;" there appears no reason why, among the remaining adult population included in the said gross parochial deaths, as distinct from the more detailed and special hospital returns, the ratio of deaths from phthisis, *as compared to other diseases*, should not be approximately the same as it is found to be in the 1700 who died in hospital, where the proportion has been pretty reliably ascertained as above, to be about 3 in 17.

3. *In what Stage or Form of Phthisis is it found Curable by a Change from the Climate of the Coast to the Sierra?*—On the coast generally, the most usual exciting cause of pulmonary affections is observed to be some check to the perspiration (*resfrio*); and not only pulmonary complaints, but rheumatisms, diarrhœa, and fevers acknowledge this origin. It is more particularly in spring that we see the effects of this *resfrio* in hospitals crowded with patients under the influence of febrile catarrh, pneumonia, pleurisy, and phthisis pulmonalis. When the frame becomes much debilitated, and especially when the patient is convalescing from some prior ailment, it is a familiar event that, under these circumstances, incipient phthisis presents itself in the form of such admonitory symptoms as growing debility, failing appetite, a slight dry cough, feverish pulse and heat, with restlessness and wakefulness by night.

In the dry and sultry summer months cases occur under a different aspect, in which, from the beginning, the gastric system is more ostensibly disordered. The tongue whitish-coloured and furred; evening fever and sleepless nights; a short dry cough; depression of spirits, with a foreboding of pulmonary consumption or hæmoptysis on the part of the invalid, are so many symptoms which attend this form of attack. In all cases, whether originally of the gastric or pulmonary type, the patient or physician must not waste time in the employment of unsuccessful special remedies. And the plain reason of this practical admonition, which indeed amounts to a popular maxim in Peru, is that a change from the coast to the mountain climates, graduated as the case may require, will do more to restore health than all the drugs within their ken; and that, if this easy migration be too long deferred, confirmed as well as hopeless phthisis will be the end of disorders so initiated on the coast.

But though it be here necessary to characterize such examples as the above, in pointing out the introductory forms which phthisis assumes in Peru; yet it is important to bear distinctly in mind, that the most common prelude, as well as attendant, of the Lima phthisis pulmonalis, undoubtedly is hæmoptysis; to which there appears to be a remarkable predisposition among all the mixed classes and races of the population, particularly in the white, Creole, and brown females of preponderating Indian caste. The healthy, full-chested, mountain Indian mother, if engaged in the maternal duty of suckling her young on the coast, often acquires a predisposition to hæmoptysis, to which she had shown no tendency whatever so long as she lived and nursed on the mountains. On the hill-land the ordinary functions of the digestive organs are vigorously exercised; while on the coast, the long-continued influence of a

warm and humid atmosphere not only keeps up a relaxation of the skin, but induces a more languid appetite, and a less perfect and healthy action of the stomach and bowels, &c., which soon tells on the whole system. Europeans soon become lazy, and unwilling to take exercise on foot, in the Lima climate, and suffer a great, though gradual, loss of nervous and muscular power. The offspring, especially the male offspring, of the athletic Spaniard, grows up a comparatively delicate man; but the negro race thrive well on the coast, and retain the muscular power of their progenitors. The white family always suffer more or less from a protracted residence in Lima, where congestive diseases are sure to arise in this race; and more particularly the prevailing disorders, hæmorrhoids, blenorrhœa, dysentery, &c.

Whenever hæmoptysis shows itself in Lima—which it often does in the Creole ladies after an evening party (*tertulla*), without any previously perceived sign or suspicion of so great a misfortune—the circumstance is always one of alarm.

The spitting of blood may be very slight at first, and attended with a slight cough; and from so apparently simple a beginning, experience and common observation lead the patient, friends, and physicians together, to fear the approach of phthisis, unless the hæmoptysis and cough can be speedily subdued. As a general rule, in such cases, phthisis is always suspected to lurk in the background, unless its incubation be promptly checked by a change of climate. The ordinary result is, that those so circumstanced, especially when of the delicately-organized, fair, Creole race, very rarely trust to medicine or to the assistance of the physician, but at once order the mules and other necessary arrangements for a journey to the interior. It is only by this decided conduct that they hope permanently to guard against a future and more formidable return of hæmoptysis, with its phthisical consequences; and they seek at first notice of the disease to insure a full reparation of the injured respiratory organs, by an adequate continuance in the well-known and appropriated regions of convalescence.

When cases thus inaugurated—which are far too frequent in Lima and other parts of the coast of Peru—go on for a few weeks, not to say months, without decided amendment under medical treatment, we may expect to find on examination positive signs of pulmonary consumption. Now, then, besides occasional returns of hæmoptysis more or less developed, varying from coloured sanguineous sputa, to mouthfuls or even cupfuls of blood at a time, there is also more or less cough, soon attended by some degree of pain in the chest; depression of spirits, failure of appetite, with loss of flesh, and lassitude; some notable change in the respiratory sound, or perceptible deviation from the normal murmur, with almost always obscurity of sound on percussion under either the right or the left clavicles. No Lima junta of experienced native or well-acclimated European physicians, would for a moment hesitate to order to the sierra a patient in the condition I have just described. They would deem this transfer of climate as the only security for the patient.

Under such conditions I have witnessed the application of all approved European remedies of every school fully tried, where the phthisical patient was, for one reason or another, destined to run his course on the coast

and in the capital, under the eye of able assistants, but always with the same fatal termination.

Cod-liver oil, extensively used of late years, has appeared to alleviate the pulmonary symptoms, by improving the habitual state of the digestive organs, and, as far as I know, it did no more in that country, whatever may have been its success in Europe.

I have sometimes seen cases of pneumonia, imperfectly cured, terminate in chronic phthisis, on the coast of Peru. I have also met with cases of passive and chronic hæmoptysis sustained by pulmonary congestion, or consequent upon heart disease, which never passed into phthisis. But such cases are easily distinguished for the most part, and I may just say in passing, that small doses of spirits of turpentine—say twenty drops thrice a day—have been useful in stopping these passive forms of pulmonary hæmorrhage.

In advanced stages of phthisis, attended with opaque and purulent sputa, colliquative sweats, bronchial and cavernous respiration, with all the aggravated symptoms of hectic fever—even in such a plight, the change from the climate of Lima or the coast to that of the Andine slopes (at moderate elevations relatively to the snow line) has been known to prolong life for years, and allow the patient renewed strength to return from time to time to the coast, with marked improvement in general health, as well as in the condition of the lungs, and quite free from fever. But after a few years, such partial convalescents have succumbed to a fresh accession from cold or other exciting cause. But while I state these facts, and could cite individual instances in point, it should never be forgotten that the timeous removal to the sierra is intended to prevent the advancement of phthisis beyond its first initiatory stage in the hæmoptoic form of invasion so prevalent in Peru, or to cause it to retrograde altogether, even from this primary condition. It must be clearly understood, therefore, that I claim the curative effects of the Andine climates, on the broadest grounds of facts and experience, in favour of the early stage only, and not the more advanced periods of pulmonary consumption, when there is, correctly speaking, no sound lung to rescue.

4. *What are the Inland Localities in Peru approved as the Best for Convalescence from Phthisis?*—I shall speak of the localities best known in, and most convenient to, the capital; other inland positions of corresponding temperature will naturally be resorted to from other points of the coast, according to their contiguity. On the Pacific slope of the Cordillera, and by the Pasco road from Lima, Haraway (usually pronounced Yaraway) and Canta are considered the best localities; and Huamantanga is also considered favourable; but Canta above all, on this route, is allowed to be most desirable, being about twenty-five leagues from Lima, and at an elevation of 10,000 feet, on a height overlooking Obrajillo, which latter is in a hollow locked in by hills, and about 1000 feet lower than Canta. Again, by the Zarma road from Lima, Matucana and San Mateo are favourable climates; the former, according to McLean, is 8026, and the latter 10,984 feet high; and of the two, Matucana is considered the best. But Canta is found preferable to either as a place of permanent convalescence. Culluay, enclosed in a basin-shaped hollow a few leagues above Obrajillo, on the Pasco road, is 12,000 feet above the sea, and

corresponds in climate with Chicla, a few leagues above San Mateo on the Zarma road, and at an elevation of above 12,000 feet, according to McLean and Herndon's reckoning. Both these localities are hostile to the phthisical patient.

When it is determined to pass the Cordillera for convalescence, this is usually done by the pass of Yauli or by Tucto, to the temperate valleys of Zarma, Jauja, and Huancayo. The elevation at the pass of the Viuda mountain above Culluay on the one hand, and of that of Antarangra (also called Antacona) above Chicla on the other, is nearly equal, as far as can be determined by the measurements of different observers. McLean gives the one at 15,543, and Rivero the other—viz., that of the Viuda, at 15,500; the Viuda being 15,968 feet—just 1000 feet above the line of glaciers or permanent snow. Across the Cordillera gates or passes (Portachuelas), the patient, if very weak, is conveyed in a litter, and if his direction be Pasco, he cannot remain there, but must at once pass through to Huarriaca, a climate quite analogous in temperature to that of Obrajillo, only with better ventilation. But physicians from Lima always send their phthisical patients (when ordered across the Cordillera) to Zarma and Jauja as the great sites of convalescence; and on the way to these celebrated localities, Matucana is the favourite resting-place of phthisical and hæmoptoic patients. It is at this point, in the headland of the valley of the Rimac, enjoying a mild atmosphere on the confines of the air of the coast and the sierra, and just within the rain line, without being yet too wet or cold, that the invalids alluded to receive the first kindly impressions of improving health, and after a longer or shorter stay here, proceed to those more favourable climates, in higher elevations, beyond the first Cordillera.

I should state expressly, that the extensive valley of Jauja, rather cooler in temperature, and also of a few hundred feet more elevation above the sea than Zarma (which Herndon gives at 9738), is allowed to have a decided superiority for the recovery of the hæmoptoic and phthisical invalid. The climate of this locality is temperate, and productive of a great variety of grain and green crops. But for the cure of phthisis, the Montana climate, for at least eight months in the year, is too damp, and if the patient be not careful in ordinary ablution—which natives prefer doing when the sun shines—the body is apt to be chilled. Lieutenant Herndon experienced this effect after bathing, and cautions his readers on the subject.

I shall conclude these observations by endeavouring to solve an important problem bearing intimate reference to our present inquiry, and which I find suggested in Dr. James Copland's very elaborate and instructive article on Tubercular Phthisis, recently published in Part 17 of his valuable 'Dictionary of Practical Medicine.'

The problem I allude to is contained in the following extract:—

"Having ascertained the frequency of the disease in the aborigines of a country or climate, it is next of importance to know how far that frequency may be modified, diminished, or increased by change to other countries, either colder or warmer; or of higher or lower elevation, &c., and by the adoption of different food and other habits." (p. 1130, sect. 205.)

I beg the reader's attention to this quotation. I hope I may, without any undue pretension, be allowed to say that I feel not only

authorized, but called upon as a matter of duty to record on this head the result of my long experience in different climates of Peru. I shall therefore remark that, as regards the native white Creole and the brown races of mixed blood, this problem may be considered as solved in cases of incipient phthisis pulmonalis attended with more or less hæmoptysis. By change to other countries—for example, to Chile, which is colder, or to Guayaquil, which is on the *Equator*, and consequently warmer—the effect on the patient from Lima has been so often tried and found injurious, that this is a change of climate which no experienced resident physician would venture to recommend. But by proceeding inland to the valley of Jauja, at the elevation of ten thousand feet above the sea, such incipient phthisical cases—especially of the hæmoptoic type, as I have defined—are always relieved, and almost always cured, provided the patient remain long enough in the uplands to insure this result.

Time is required to bring about a radical organic change, for when individuals apparently quite recovered in Jauja descend to the coast, and particularly to the capital, within a few months the hæmoptic and other phthisical symptoms have been observed to return, rendering a longer residence in the sierra necessary to insure a permanent cure. A year's sojourn in the sanitary climate of the hill-land is usually considered indispensable in serious cases, which have demanded a transandine climate. Milder cases and slighter indications of pulmonary disease, with tubercular development, often yield to a few months' residence at Matucana, Haraway, or Canta, on the western slope of the Andes, and not far from the resources of the capital.

The unvarying experience of centuries, perfectly relied upon by the natives, proclaims this change from the coast to the sierra climates, to afford undoubted beneficial results to the native white, as well as the diverse shades of brown and olive races of the coast, when labouring under hæmoptysis or pulmonary consumption. The negro is less subject to phthisis, and also reluctant to encounter the bracing air of the Cordillera; his favourite element is the warm and humid air of the coast. The influence of race in the cure of disease is wisely considered by Dr. Copland as of greater importance than has been yet bestowed upon it. In Peru, I found this truth constantly illustrated in practice. For instance, in dysentery, calomel and opium properly and timeously administered, are almost infallible in the Indian race, in the white far less certain, and in the negro cannot be depended upon at all. In yellow fever, turpentine cured as many as fifty per cent. of Indians, apparently in a hopeless condition, being sent to the Lazaretto, as it was believed, in an incurable state; but in the whites, turpentine, as administered by us in Peru in the year 1854, was of comparatively little power; and as for the negroes, we had no opportunity of ascertaining its effects, since in them this malady, so fatal to the whites, was scarcely experienced, except as a slight fever with headache, which by the aid of common enemata passed off in a few days, leaving no bad symptom or dregs of disease behind it.

But as regards phthisis, which we have been considering above, I have always seen cases of the character described by me turn out well through migration to the sierra. And I may truly say, that from my own long experience in Peru, and knowledge of these cases, I could easily

recount a multitude of permanent cures, also familiar to many native physicians.

This result, as far as the natives are concerned—a goodly mixture and variety of races, we must admit—is simply conclusive matter-of-fact; and now that the communication by Panama is so easy, it may be worth while to test the effects of the Andine climate of Peru on the European phthisical invalid. I had little opportunity to do so with the English under my charge in that country, but as far as my experience went, it was as favourable to the European as to the native Creole. But allowances, no doubt, must be made for different habits of life and other causes. The benefit received by Peruvians in the instances in question are too evident to admit of cavil, nor can the good effects be explained on the score of mere change of scenery and the pleasures of travelling. All coast-born Peruvians leave the neighbourhood of the sea and their native towns—above all, Lima—with extreme reluctance, and look upon the sierra as a kind of Siberia—a place of privation and exile. But in spite of all these prejudices and dislikes, when they realize the change to the sierra, they are constantly seen to recover there, under conditions of pulmonary tubercular disease which would undoubtedly terminate fatally, and that very soon, on the coast.

On the mountains, the Limeñian habits of diet are necessarily somewhat changed, and the invalids are naturally led to more exercise in the open air; but yet their in-door habits, with their gambling propensities, will ever predominate, whether on the hills or coast. Cards and dice, indeed, are esteemed not merely an amusement, but an indispensable part of a genteel education.

The air of the mountains—in those elevated localities pointed to as suitable to the recovery of the phthisical invalid—is free from the malaria of the coast, and (as we have already learnt) clear, light, cool, and invigorating—alike removed from the extremes of cold or heat, and, upon the whole, remarkably equable.

On the coast, the natives continually drink in abundance cooling acidulated beverages, as lemonades, pineades, &c., and the classes in better circumstances (under the idea that a weakening climate needs strengthening food) use much more animal food than a climate so mild, with an indolent life, would seem to require. Indeed, all grades of the population consume great quantities of lard and pork, and also of fish fried in pans of boiling lard. This kind of cooking goes on in the open squares, corners of streets, and market-places, every evening and morning, for the convenience of the populace or lower classes, who thus feed in the open air at small cost of money and free from domestic trouble. Sweets, pastry, and fruits they eat at all hours, irrespective of their regular meals. On the removal of invalids from such a population to the sierra, the same facilities do not offer. The mountain diet is necessarily more simple, and the habits of life there assumed for the time are more in unison with those of the rural population of the district.

ART. III.

Annual Report of Cases admitted into the Medical Wards of St. George's Hospital during the Year ending December 31*st*, 1855. By G. GODDARD ROGERS, M.D., Medical Registrar to the Hospital.

THIS Report is the fifth presented to the governors of St. George's Hospital since the adoption of an uniform system of registration of cases. The method of classifying and tabulating the various diseases was explained by Dr. Barclay, the former registrar, in his Report for 1853, and a few remarks were also added to show the difficulties in the way of adopting the system of the Registrar-General unchanged. During the past year the cases were taken on a rather more extended plan; and in the classified Index of Diseases, by Dr. Barclay's advice, two fresh subdivisions are made in Division 22—Diseases of the Brain and Spinal Cord, to which allusion is again made in the remarks on this class of affections. Gout and Rheumatic Gout are also separated for the first time. In all other respects the method of registration is the same as during the preceding four years, and no pains have been spared to render this Report as accurate as possible. But the co-operating aid of other metropolitan hospitals is needed before any great statistical facts can be educed respecting the prevalence of various diseases at certain periods, and their more or less favourable rate of mortality.

Cases admitted during the year 1855.						Admitted during five years.	
Nature of Disease.	Admitted.	Died.	Per-centage of mortality.	Complicated with other diseases.	Deaths among complicated cases.	Admissions.	Per-centage of mortality.
1. Fevers:							
Continued fever	188	17	9·04	76	12	747	11·3
Influenza	24	...	...	7	...	61	...
Remittent fever	1	1	?	1	1	1	?
Asiatic Cholera	...	...	...	...	...	140	49·3
2. Eruptive fevers:							
Measles	2	...	...	1	...	19	...
Scarlatina	23	2	8·69	6	...	66	15·1
Varioloid	...	...	...	...	...	16	...
Erysipelas	30	4	13·3	10	3	124	16·1
3. Intermittent fevers:							
Quotidian	4	...	...	1	...	22	...
Tertian	5	...	...	1	...	37	...
Quartan	...	...	...	...	...	5	...
Irregular	3	...	...	1	...	10	...
4. Rheumatism:							
Acute	77	2	2·59	41	...	332	3·89
Subacute and slight	108	3	?	37	3	514	?
Chronic	133	1	?	45	1	683	?
5. Gout:							
Gout	11	3	?	4	3 }	141	...
Rheumatic gout	24	...	...	5	... }		
6. Poisoning:							
Irritant	1	...	...	...	...	10	20·0
Narcotic	...	...	...	...	...	7	14·3
Gaseous	...	...	...	...	...	2	...
Syphilitic	6	...	...	1 }	...	*47	...
Gonorrhœal	5	...	...	4 }			
Hydrophobia	...	...	...	...	...	...	...

Nature of Disease.	Cases admitted during the year 1855. Admitted.	Died.	Per-centage of mortality.	Complicated with other diseases.	Deaths among complicated cases.	Admitted during five years. Admissions.	Per-centage of mortality.
7. Colica pictonum	7	...	...	2	...	37	...
8. Entozoa:							
Intestinal worms	3	1	?	1	1	22	?
Echinococcus hominis	...	...	...	...	...	2	100·0
9. Dropsy:							
Anasarca	109	37	33·9	102	36	510	35·1
Ascites	14	8	57·0	13	8	84	51·1
10. Hæmorrhages:							
Epistaxis	4	1	?	1	1	19	...
Hæmoptysis	18	4	22·2	10	4	97	24·7
Hæmatemesis	9	...	...	1	...	30	13·3
Hæmaturia	8	2	?	6	2	36	11·1
Intestinal	5	1	20·0	4	1	39	17·9
Uterine	12	...	...	3	...	35	...
11. Purpura	1	...	...	...	...	25	32·0
12. Scurvy	1	...	...	...	...	6	...
13. Anæmia	74	1	?	42	1	328	1·82
14. Chlorosis	19	...	...	7	...	98	...
15. Cachæmia	6	2	33·3	3	1	41	41·1
16. Scrofula	16	6	37·5	10	6	55	18·1
17. Tubercular diseases:							
Phthisis	153	35	22·8	83	25	664	33·8
Tubercles in brain	4	4	100·0	2	2	13	92·3
Accretions in peritoneum	2	2	100·0	2	2	22	86·3
Tabes mesenterica	...	...	...	...	...	...	...
18. Morbid growths:							
Hydatids	3	1	33·3	1	1	10	70·0
Encephaloid	12	11	91·6	10	10	48	72·9
Scirrhus	17	4	23·5	2	2	102	17·6
Epithelial cancer	1	1	?	...	...	1	?
Colloid cancer	...	...	...	...	...	...	...
Tumours of bone	...	...	...	...	...	2	...
19. Hysteria	48	...	...	13	...	263	...
20. Chorea	18	1	?	4	...	91	...
21. Delirium tremens	18	4	22·2	6	4	75	14·6
22. Diseases of brain and spinal chord:							
Cephalitis	10	9	90·0	7	6	47	85·1
Chronic disease	14	2	14·2	6	2	45	22·2
Epilepsy	23	2	8·69	11	2	140	15·0
Apoplexy	7	3	42·8	3	1	31	54·8
Functional disturbance	25	...	...	2	...	197	3·53
Coma and convulsions	3	2	66·6	3	2	...	...
Insanity	4	...	...	1	...	...	...
Inflammation of cord	3	1	33·3	2	1	13	44·4
23. Tetanus	1	1	100·0	...	...	3	66·6
24. Paralysis:							
Hemiplegia	32	2	6·25	10	2	149	8·72
Paraplegia	14	1	7·14	6	1	99	13·1
General paralysis	3	...	...	...	...	...	...
Local paralysis	22	...	...	4	...	45	2·22
25. Neuralgia:							
Tic douloureux	4	...	...	3	...	17	...
Sciatica	11	...	...	2	...	59	...
Hemicrania	4	...	...	2	...	9	...
Angina	...	...	...	...	...	2	...
26. Diseases of the heart:							
Carditis	1	1	100·0	1	1	1	100·0
Pericarditis	16	7	43 7	16	7	73	38·3
Endocarditis	21	4	19·04	21	4	74	10·8
Hypertrophy	18	8	44·4	15	8	131	58·01
Dilatation	15	11	73·3	14	10	71	56·3
Valvular disease	72	21	29·1	66	20	296	25·6
27. Arteries and veins:							
Aneurism	10	8	80·0	3	3	27	44·4
Phlebitis	3	3	100·0	3	3	28	35·7
28. Respiratory organs:							
Laryngitis	11	7	63·6	10	7	41	43·9
Tracheitis	1	1	...	...	...	4	...

Cases admitted during the year 1855.						Admitted during five years.	
Nature of Disease.	Admitted.	Died.	Per-centage of mortality.	Complicated with other diseases.	Deaths among complicated cases.	Admissions.	Per-centage of mortality.
Bronchitis	151	27	17·8	92	20	639	14·5
Pneumonia	71	18	25·3	45	16	236	28·3
Pleurisy	58	23	39·6	40	22	266	32·3
Emphysema	18	7	38·8	18	7	66	36·3
Asthma	4	...	...	1	...	7	...
Pertussis	1	1	?	...	...	6	...
Pneumo-thorax	...	...	...	...	...	5	80·0
29. Mouth, &c.:							
Glossitis	1	...	...	...	...	1	...
Quinsy	10	1	10·0	8	1	53	...
Enlarged tonsils	6	...	...	3	...	21	...
Ulceration	11	1	9·09	7	1	26	...
Mumps	3	1	...	3	1	11	...
30. Stomach and œsophagus:							
Dyspepsia	76	1	?	41	1	319	1·25
Ulceration	5	5	100·0	3	3	11	90·9
Stricture of œsophagus	3	1	33·3	1	1	10	20·0
31. Intestinal canal:							
Obstruction	3	2	66·6	1	1	7	57·1
Constipation	20	3	?	3	3	218	...
Diarrhœa	35	3	8 57	26	2	180	7·77
Dysentery	5	3	60·0	3	3	21	42·8
Enteritis	1	...	...	...	...	1	...
Ulceration	5	5	100·0	5	5	15	80·0
Tympanitis	1	...	...	1	...	11	...
32. Peritonitis:							
Acute	15	8	53·3	8	8	105	41·9
Chronic	9	3	33·3	6	3	64	48·4
33. Liver and gall-bladder:							
Inflammation and congestion	11	2	18·1	6	2	36	25·0
Cirrhosis	9	8	88·8	9	8	58	75·8
Enlargement	14	4	28·5	10	4	56	26·7
Jaundice	11	2	18·1	3	1	79	27·8
Abscess	3	3	100·0	3	3	...	...
Gall-stones	...	...	...	...	...	2	...
34. Spleen:							
Enlargement	8	6	?	6	6	22	45·4
35. Urinary organs:							
Nephritis	10	5	50·0	10	5	29	17·2
Abscess	3	1	33·3	2	1	11	27·2
Albuminuria	110	48	43·6	107	48	432	47·4
Cystitis	6	...	...	1	...	29	10·3
Diuresis	1	...	...	1	...	2	...
Ischuria	1	1	100·0	1	1	2	100·0
36. Diabetes	6	...	...	2	...	25	12·0
37. Ovaries:							
Dropsy	4	1	25·0	1	...	23	30·4
Tumours	12	1	8·33	2	1	41	?
38. Uterus:							
Puerperal fever	1	...	...	...	...	1	...
Puerperal mania	1	1	?	1	1	1	?
Amenorrhœa	15	...	...	8	...	65	...
Menorrhagia	15	...	...	...	...	69	...
Leucorrhœa	12	...	...	7	...	57	...
Tumours	13	1	7·69	...	...	29	?
Prolapsus	5	...	...	1	...	26	...
Ulceration	2	...	...	...	...	2	...
Congestion	6	...	...	2	...	13	...
External organs	4	...	...	...	...	9	...
39. Bones and joints	13	3	?	7	3	47	23 4
40. Skin and cellular tissue:							
Erythema	11	1	?	8	1	63	...
Urticaria and roseola	4	...	...	3	...	17	11·7
Lichen and prurigo	5	...	...	4	...	14	...
Squamous eruptions	10	...	...	3	...	47	...
Vesicular eruptions	6	1	?	3	...	74	...
Pustular eruptions	2	...	...	1	...	26	...
Abscess; cellular inflammation	14	2	14·2	8	2	52	40·3
41. Muscles	1	1	?	1	1	2	?
42. Anomalous cases	6	...	...	...	...	27	...

Remarks.

1. *Fevers.*—In the 17 fatal cases of continued fever, 9 presented ulceration of some portion of the ileum, cæcum, or colon; 2 presented patches of unduly elevated glands; 2 were free from all lesion; whilst in the remaining 2 the intestinal canal was not examined.

The 12 fatal complicated cases are thus distributed:

4 cases were complicated with pneumonia.
„ „ „ „ pleurisy.
3 „ „ „ „ phthisis.
1 „ „ „ „ inflammation of brain.
1 „ „ „ „ abscess of the liver and right kidney.
1 „ „ „ „ parotitis and chronic rheumatism.
1 „ „ „ „ ulcerated throat.

The only case of remittent fever was that of a child twelve years of age. Acute peritonitis, from perforation of the lower part of the ileum, was the cause of death.

2. *Eruptive Fevers.*—One case of measles occurred four weeks after admission, when there was no evident source of infection. The patient was suffering from local paralysis. In the other case the eruption appeared the day before admission. The total number of cases of scarlatina is one less than during 1854; the deaths also are less by one.

Scarlatina appeared in one patient who had been in the hospital two months with chorea.

1 case was associated with influenza.
1 „ „ „ hæmoptysis.
1 „ „ „ erysipelas, rheumatism, and syphilis.
1 „ „ „ parotitis.
1 „ „ „ pneumonia.

Death occurred in two cases of a malignant type within forty-eight hours. The number of patients affected with erysipelas is the same as during 1854. The deaths are higher by one. In only one case was erysipelas the cause of death. The other fatal cases were associated—

1 with paraplegia.
1 „ delirium tremens.
1 „ phthisis and abscess of the liver.

Erysipelas occurred in a man with scarlatina who had been in the hospital a week; in a case of ascites and cirrhosis which had been under treatment five weeks; and in a case of prurigo which had been in the house three days.

3. *Intermittent Fevers.*—Bronchitis accompanied one case of tertian, and one of irregular ague.

4. *Rheumatism.*—Of the 41 complicated cases of acute rheumatism, 30 were accompanied by some cardiac affection.

5 were complicated with pericarditis.
11 „ „ „ endocarditis.
14 „ „ „ valvular lesion.

Among 37 complicated cases of subacute rheumatism, 3 proved fatal.

1 was complicated with valvular lesion and pleurisy.
1 „ „ „ phthisis.
1 „ „ „ albuminuria.

The only death amongst the cases of chronic rheumatism, arose from fever. (See 1. Remarks on Fever.)

5. *Gout.*—Dr. Barclay's remark in a previous Report, that "the larger proportion of cases belong more properly to what is called rheumatic gout," is verified, now the separation has been attempted; the cases of rheumatic gout being more than two to one. The presence of chalky deposit has been the guide for placing the case under the head of *gout*. In the sixth column the cases are indiscriminately added together. The same remark applies to syphilis and gonorrhœa, which are only occasionally found in the medical wards. (See 6. Poisoning.)

6. *Poisoning.*—The only case was that of a female, who swallowed a large quantity of sugar of lead.

7. *Colica Pictonum.*—Two cases were complicated; one with epilepsy, the other with subacute rheumatism.

8. *Entozoa.*—Three cases of tænia are enumerated; one, which occurred in a phthisical patient, terminated fatally.

9. *Dropsy.*—In seven cases of anasarca, death arose from exhaustion. No specific local disease was detected to account for the effusion.

In 101 cases, organic disease of the heart, lungs, or kidneys existed. In one there was extensive malignant disease of the stomach.

In 6 cases of ascites, disease of the liver or peritoneum was clearly ascertained. The other complicated cases are thus distributed:

1 was complicated with laryngeal phthisis.
1 „ „ „ bronchitis.
1 „ „ „ disease of the kidney.

In 3 the cause was not satisfactorily determined.

10. *Hæmorrhages.*—In one patient suffering from phthisis and organic disease of the heart, repeated epistaxis hastened the fatal termination.

18 cases of hæmoptysis occurred in the hospital during the past year. In 9 it was associated with phthisis; in 1 with organic disease of the heart. One case of hæmatemesis was associated with profuse uterine hæmorrhage.

In 2 cases of hæmaturia, Bright's kidney was found after death, associated in 1 with encephaloid disease of the organ. Both patients were dropsical.

The fatal case of intestinal hæmorrhage occurred in a patient with albuminuria and enlarged liver. Uterine hæmorrhage is limited to accidental floodings. Menorrhagia has been placed under Division 38, Diseases of Uterus, and includes all cases of increased menstrual secretion.

13. *Anæmia.*—Used to express those forms of impaired health and general weakness which are often found to exist after recovery from fevers, hæmorrhages, or as a result of hyperlactation. The term appears more appropriate than the vague one, so often employed, of "debility." In one anæmic patient, who died somewhat suddenly, endocarditis, confined to the left side of the septum ventriculorum, was discovered at the post-mortem examination. The liver was also considerably enlarged.

14. *Chlorosis.*—This term is restricted to cases of anæmia associated with depraved and irregular menstrual secretion.

15. *Cachæmia*—Includes all cases of unhealthy condition of blood, whether from the presence of pus, the introduction of poisons, or any other extraneous cause which materially alters the character of the fluid. Two cases under this heading proved fatal from pyæmia, which arose in one in consequence of a bite from a glandered horse.

2 other cases depended on starvation.
1 followed an attack of measles.
1 was a case of unhealthy ulcer of the leg.

16. *Scrofula.*—Of 16 patients who were admitted on account of cachexia dependent on the strumous diathesis, six complicated cases proved fatal.

1 was complicated with inflammation of the brain.
1 " " chronic disease of the brain.
1 " " pericarditis and abscess of the heart.
2 " " bronchitis.
1 " " albuminuria.

17. *Tubercular Diseases.*—In both the fatal cases of tubercles in the brain, the same existed in the lungs. Of the two cases of accretions in the peritoneum

1 was associated with chronic peritonitis and effusion.
1 " " cephalitis.

18. *Morbid Growths.*—In a fatal case of pleurisy, hydatid cysts were found in the liver. Epithelial cancer occurred in a man who had always enjoyed good health until two months before admission into the hospital. The first symptom was nausea, soon followed by vomiting, which persisted until within three days of his death. The disease was confined to the cardiac orifice of the stomach, and to a small portion of the left lobe of the liver.

20. *Chorea.*—In the fatal case, the spinal cord was found slightly softened.

21. *Delirium Tremens.*—In no case was the disease alone the cause of death. In the 4 which terminated fatally,

1 was complicated with epilepsy.
1 " " chronic disease of brain and quinsy.
1 " " pneumonia.
1 " " erysipelas.

22. *Diseases of the Brain and Cord.*—Two fresh subdivisions are made in this class of diseases—"functional disturbance" was formerly the term used to include all cases where no change of structure had occurred; consequently the range was unduly wide, and comprehended all kinds of disorder, from cephalalgia to insanity. For the latter, therefore, a separate heading is now employed; and one is added for "coma and convulsions," dependent especially on disease of the kidney.

In 3 cases of cephalitis, tubercles were found in the brain. In 1 the affection was complicated with pericarditis and disease of the kidney, in 1 with fever, in 1 with hemiplegia. In 4 of the fatal cases, tubercles co-existed in the lungs; in 4 no trace of scrofulous matter was detected. The remaining case was not examined.

In one fatal case of chronic disease of the brain, the patient was originally admitted for delirium tremens. The other death occurred in a person of tubercular diathesis.

One epileptic patient had an apoplectic seizure, followed by hemiplegia, which soon proved fatal. The second complicated fatal case was one of bronchitis and disease of the kidney, coupled with an immense carbuncle.

Of the three fatal apoplectic cases,

In 1 the kidneys were congested.
In 1 there was fatty degeneration of the heart.

The third was the epileptic and hemiplegic case just mentioned. Disease of the kidneys existed in both the fatal cases of coma. In one the convulsions supervened on chronic disease of the brain, which was found extensively indurated in many spots. The specific gravity of the healthy part being 10·55, and that of the diseased portion 10·68. Several epileptic fits occurred in one of the patients who was insane. The fatal case of inflammation of the cord was complicated with caries of the spine and nephritis.

Of the others,

1 appeared to arise from exposure.
1 „ „ general privation.

Both of these were paraplegic. No accurate history was obtained of the other two.

23. *Tetanus.*—Only one case was admitted, which proved fatal. The patient had been much exposed to damp four days before coming to the hospital. At the post-mortem examination the brain was found somewhat softened; a large amount of spinal subarachnoid fluid existed; and the right rectus abdominis muscle was lacerated transversely.

24. *Paralysis.*—Erysipelas was the cause of death in a paraplegic patient. The two fatal cases of hemiplegia are recorded under epilepsy and cephalitis. (See Division 22.)

Amongst 22 cases of local paralysis, 4 were complicated—

1 with measles.
1 „ disease of the kidney.
1 „ epilepsy.
1 „ disease of the knee-joint.

26. *Diseases of the Heart.*—In the case of carditis, a large abscess was discovered in the muscular structure of the heart. Pericarditis and scrofulous disease of the hip coexisted. 7 cases of pericarditis were associated with acute rheumatism; 3 had also endocarditis; 1 carditis; in 5 the kidneys were more or less diseased.

1 was also complicated with chorea.
1 „ „ „ „ pneumonia.
3 „ „ „ „ pleurisy.
1 „ „ „ „ fever.
1 „ „ „ „ cephalitis.

Of the fatal cases—

Pleurisy co-existed in 2.
Pneumonia „ 1.
Endocarditis „ 1.
Carditis „ 1.

Cephalitis co-existed in 1.
Albuminuria „ 3.

Endocarditis was associated in 16 cases with acute rheumatism. 2 fatal cases occurred in anæmic patients. (For the account of one, see Division 13.) The other cases were complicated with pericarditis and dilatation, together with albuminuria.

Hypertrophy was accompanied—

In 6 cases by dropsy.
„ 4 „ valvular lesion.
„ 1 „ dilatation.
„ 1 „ pericarditis.
„ 2 „ disease of the kidney.
„ 1 „ phthisis.

Of 15 patients with dilatation—

9 had disease of the kidney.
6 were dropsical.

72 cases of valvular disease came under notice, 19 of which were complicated with dropsy, and 13 with disease of the kidney.

27. *Bloodvessels.*—An unusual number of aneurismal cases was admitted. Of those which terminated fatally—

1 was associated with pleurisy.
1 „ „ „ bronchitis.
1 „ „ „ dropsy and disease of heart and kidneys.

In the other 5 internal hæmorrhage was the immediate cause of death.

Aneurism of some portion of the thoracic aorta occurred in 8; in 1 the abdominal portion of the vessel was affected; the other was an aneurism of the right subclavian artery.

Phlebitis was fatal in 3 cases—

1 was associated with acute rheumatism.
1 „ „ „ cancer of the stomach.
1 „ „ „ ulceration of intestines.

28. *Respiratory Organs.*—All the cases of laryngitis were of a chronic nature. The 7 deaths occurred in phthisical patients. In the case of tracheitis, tracheotomy was performed two days after admission, but without success.

Of 20 fatal cases of bronchitis, emphysema existed in 14.

11 were associated with disease of the heart or kidneys.
6 „ „ „ phthisis.
1 „ „ „ general tuberculosis.
1 „ „ „ cirrhosis of liver.
1 „ „ „ encephaloid deposit in the lungs.

Pneumonia was associated—

In 6 fatal cases with pleurisy.
„ 2 „ „ disease of the heart.
„ 5 „ „ disease of the kidney.
„ 1 „ „ delirium tremens.
„ 1 „ „ jaundice.
„ 1 „ „ malignant disease of the liver.

Of 22 complicated fatal cases of pleurisy—

6 were associated with pneumonia.

6 were associated with disease of the heart.
7 „ „ „ disease of the kidney.
3 „ „ „ phthisis.

Of 7 patients who died from bronchitis and emphysema, pneumonia and disease of the heart also existed in 2. The fatal case of asthma was complicated with dropsy and bronchitis. In the only case of hooping-cough admitted, death occurred from exhaustion.

29. *Mouth and Pharynx.*—Glossitis occurred in one patient who had been profusely salivated before admission. The death recorded under quinsy occurred in a man who came in for an attack of delirium tremens. Chronic disease of the brain was discovered at the post-mortem examination. One fatal case of ulcerated throat occurred in a patient who had been long suffering from fever. The same was the condition of a man whose death is recorded under mumps.

30. *Stomach and Œsophagus.*—Ulceration of the stomach proved fatal in 5 cases. In 1 the ulcer had perforated the posterior wall, the opening being blocked up by the pancreas. In another, ulcers were found all along the smaller curvature. In another, an ulcer of the size of a five-shilling piece had perforated near the pylorus, and here also the pancreas filled up the opening. In another, perforating ulcers existed in the anterior and posterior walls. The remaining case was that of a patient who had suffered from dysphagia nine months. Ulcers existed around the cardiac end of the stomach, and the trachea and œsophagus communicated by a large oval ulcerated opening.

All the cases of stricture of the œsophagus were believed to be the result of malignant disease.

31. *Intestinal Canal.*—In one case of obstruction, acute peritonitis supervened, and proved fatal. The other death was caused by a malignant growth producing the obstruction. Constipation was chiefly associated with dyspepsia. Three deaths are enumerated.

1 was a case of malignant disease of the rectum.
1 „ „ malignant disease of the stomach.
1 „ „ acute peritonitis.

In the deaths from diarrhœa, fever was the accompanying disease. One case of choleraic diarrhœa occurred on the 13th of June, in a man residing in Pimlico. In the three fatal cases of dysentery, immense ulceration and destruction of the mucous lining of the large intestines was found upon examination.

Five deaths are enumerated under ulceration.

1 was complicated with ovarian disease.
2 „ „ „ dysentery.
1 „ „ „ pleurisy and acute peritonitis.
1 „ „ „ dropsy, albuminuria, and phlebitis.

No cases have been entered as "ulceration" of the stomach or intestinal canal, in which the fact was not ascertained by post-mortem examination; and those fatal cases are not enumerated in which ulceration was a symptom of fever or of phthisis.

32. *Peritoneum.* — Among the fatal cases, acute peritonitis was associated—

In 2 cases with malignant growths.

In 1 case with fever.
„ 1 „ ulcerated intestines and pleurisy.
„ 1 „ dropsy and disease of the heart.
„ 1 „ ulceration of the stomach.
„ 1 „ anasarca.

Chronic peritonitis was associated—
in 1 case with pleurisy and bronchitis.
in 1 „ tubercular deposit.
in 1 „ dropsy.

33. *Liver and Gall-bladder.*—One of the fatal cases of hepatitis was associated with dropsy and diseased heart; one with phthisis and chorea.

Out of nine cases of cirrhosis, five had also ascites. In the remaining four, the disease was in an incipient state. Amongst the complicated cases of enlargement of the liver, four proved fatal;
1 was associated with diseased heart.
1 „ „ dropsy and albuminuria.
2 „ „ morbid growths.

The fatal case of jaundice was complicated with pleurisy. Inflammation of the liver, terminating in abscess, was found after death in three instances.
1 was a case of pneumonia and albuminuria.
1 „ phthisis.
1 „ extensive encephaloid disease.

34. *Spleen.*—Eight cases of *great* enlargement of this organ came under notice during the year; in six terminating fatally.
1 was associated with jaundice and pleuro-pneumonia.
1 „ „ disease of the heart and bronchitis.
1 „ „ fever and pyæmia.
1 „ „ phthisis and caries of temporal bone.
1 „ „ cirrhosis of liver.
1 „ „ dysentery.

35. *Urinary Organs.*—The fatal case of abscess of the kidney was associated with fever and pyæmia, and the spleen was of immense size. (See Div. 34.) Two cases of scrofulous abscess of the kidney also occurred.

The following complications existed amongst the cases of albuminuria:—
59 cases were complicated with dropsy.
24 „ „ „ disease of the heart.
29 „ „ „ disease of the lungs.
9 „ „ „ phthisis and scrofula.
4 „ „ „ diseases of the brain or paralysis.
6 „ „ „ rheumatism or gout.
1 „ „ „ diffuse cellular inflammation.
8 „ „ „ fever.
1 „ „ „ erysipelas.
9 „ „ „ disease of some other abdominal viscera.

One case of suppression occurred last year in a patient who had disease of the heart and albuminuria.

Cystitis was associated in one case with disease of the kidney. The

only case of diuresis was accompanied by Bright's disease; twelve to sixteen pints of urine were passed in the twenty-four hours; the specific gravity was always low, and the urine contained abundance of albumen, and exhibited casts under the microscope.

36. *Diabetes.*—An unusually large number of patients with this disease entered the hospital in 1855.

In 1851, five cases came under notice.
In 1852, three „ „ „
In 1853, five „ „ „
In 1854, four „ „ „
In 1855, eight „ „ „

37. *Ovaries.*—One death is enumerated amongst ovarian tumours. The patient was phthisical.

38. *Uterus.*—One case of puerperal fever, and one of puerperal mania, occurred in the ward appropriated to the obstetric physician.

Fifteen cases of amenorrhœa are enumerated, besides nineteen of chlorosis. The absence of anæmia is the reason for making a separate class, under the head of Uterine Affections. (See Remarks on Divisions 13 and 14.)

Besides fifteen cases of menorrhagia, twelve accidental cases of hæmorrhage occurred. (See Division 10, Hæmorrhages.)

39. *Bones and Joints.*—Three cases proved fatal.

1 was associated with phthisis.
1 „ „ scrofula and diseased heart.
1 „ „ paralysis and diseased kidney.

40. *Skin and Cellular Tissue.*—One fatal case of erythema occurred in a phthisical patient. An infant covered with eczema died shortly after admission. No organic disease was detected. Two deaths from cellular inflammation occurred—

1 in a case of paralysis and disease of the heart.
1 „ ulcerated bowels.

41. *Muscles.*—The rectus abdominis muscle was ruptured during tetanic convulsions. (See Remarks, Division 23.)

ART. IV.

On Infecting and Non-Infecting Syphilitic Sores. By HENRY LEE, Surgeon to King's College Hospital and to the Lock Hospital.

WHEN syphilitic matter is applied to the surface of the human body, no appreciable effect, in general, results; but when the poison comes in contact with the thin skin in those situations where it joins the mucous membrane, or when it is applied to the mucous membrane itself, or when applied to the skin in places where the epithelium has been removed, then inoculation may occur.*

When inoculation does take place, the nutrition of the inoculated spot is disturbed. Sometimes the part loses its vitality altogether; the part is

* The susceptibility to this inoculation varies very much in different individuals. A comparative immunity obtains in some persons, either from natural or artificial causes.

thrown off as a slough; the poison and the poisoned tissue together cease to exist. Sometimes the death of the infected part occurs much more slowly: it dies bit by bit, at longer or shorter intervals, or by a continuous action. The result is here generally the same as in the former case; but from the process being much longer continued, other morbid actions, followed by their natural consequences, are more liable to complicate the disease.

Instead of losing its vitality, the nutrition of the affected part may be so influenced, that although no part is thrown off as a slough visible to the naked eye, yet the poisoned tissue is unable to support its vitality. Ulceration takes place, and a loss of substance is the result: this is brought about partly by the action of the absorbents, partly by the breaking down of the tissues of the ill-nourished part, and the discharge of small portions of *débris*, mixed with fluid secretion. When the affected tissue is removed by the absorbents, the activity of the poison is not at once destroyed, as in case of the death of the structure to which it was first applied. Its presence may be clearly proved by its power of again inoculating the living tissues with which it comes in contact. This may happen at any point between the primary inoculation, and the first absorbent gland that the poison would naturally meet with, in its course towards the centre of the circulation. This morbid process will, in the following pages, be termed "ulceration," or "ulcerative inflammation." Again, the presence of the syphilitic poison may determine the formation of pus in the inoculated part. This action is usually preceded by the secretion of a thin serous fluid for a day or two; but as soon as the action is fully established, the secretion consists of pus globules and the fluid in which they float. This morbid action will be designated "suppuration," or "suppurative inflammation."

The more indolent and sluggish process by which lymph is separated, either directly or indirectly, from the blood in the inoculated part, will be termed "adhesion," or "adhesive inflammation."

The fibrin thus separated, if thrown off from the surface, may cease, like any other secretion, to be part of the living organization; but if infiltrated into the affected tissues, and not subsequently converted into pus, it will become permeated by vessels. It then undergoes the changes incident to the growth of other parts, and is finally taken up again into the venous blood.

Jenner,* in describing the effects of the vaccine virus, remarks that pustulous sores often appear spontaneously on the nipples of cows; and that instances have occurred, though very rarely, of the hands of the servant employed in milking being affected with sores in consequence. These pustules, he observes, are of a much milder nature than those which arise from that contagion which constitutes the true cow-pox. They are incapable of producing any specific effects on the human constitution, and are noticed lest a want of discrimination should lead to the idea that the persons affected were in any measure thereby secured from the infection of the small-pox. Jenner believed that the vaccine virus had its origin in a diseased secretion from horses' heels, and that this secretion is most active before it has acquired a pus-like appearance. "I am not confident,"

* Jenner on the Cow-Pox, ed. 1800, p. 7.

he says, "whether this property in the matter does not entirely cease as soon as it is secreted in the form of pus. I am inclined to think that it does."

Again, with regard to variolous matter, the same author says—

"Certain it is that this may undergo such a change from the putrefactive process, as well as from some of the more obscure and latent processes of nature, as will render it incapable of giving the small-pox in such a manner as to secure the human constitution from future infection, although we see, at the same time, it is capable of exciting a disease which bears so strong a resemblance to it, as to produce inflammation and matter in the incised skin, swelling of the axillary glands, general indisposition, and eruptions. This spurious action is often accompanied by more violent inflammation than that which occurs when the variolous matter produces its perfect effect upon the system."*

Willan also remarks, that if the vaccine fluid employed be taken at a late period, it does not always produce the genuine cellular vesicle, but is in some cases wholly inefficient; while in others it suddenly excites a pustule or ulceration, in others an irregular vesicle, and in others erysipelas. Similar appearances are occasionally observed when the lymph is taken at the proper time, and inoculated upon those whose systems are already under the influence of some disturbing cause. The variolous matter, improperly kept, or the thick matter taken from collapsed and scabbing vesicles when used for the purpose of inoculation, does not always produce the small-pox, nor prevent the future occurrence of that disease, although the persons inoculated may have had inflammation and suppuration of the arm and pains in the axilla, with fever and eruption.

Now as the inoculation of the vaccine matter may give rise to different kinds of inflammation which do not impart the natural action of the poison to the general system; and as the application of the variolous poison may produce various kinds of local action, which do not imply that it has infected the patient's constitution, so we find that the contact of syphilitic matter may give rise to different forms of local disease, which are not followed by any constitutional results.

A peculiar induration has been assumed by a large number of surgeons as the characteristic indication of such syphilitic sore, as will, under ordinary circumstances, infect the patient's system. M. Ricord, the principal advocate of this doctrine, is not, however, always quite consistent with himself. Writing in 1845, he says, the absence of induration of the base and edges of a chancre cannot be received as a negative proof; for the sores in which this characteristic does not exist, have both the contagious property of chancres, and the power of producing secondary symptoms.† At a subsequent period, M. Ricord, in his 'Lettres sur la Syphilis,' announces that the non-indurated sore never affects the patient's system, yet he admits that the presence of induration is liable to be deceptive, at least in other hands than his own. This diagnostic sign has not proved satisfactory to other surgeons. M. Cullerier, for instance, believes that the most simple chancre, with the least amount of local induration, may

* On Vaccine Inoculation (ed. 1804), pp. 31, 2.

† Les chancres privés de ce charactère ne conservent pas moins toutes leur propriétés; tant sous le rapport de la contagion que celui de la production des accidents consecutifs."—Traité de la Syphilis. Par J. Hunter. Annoté par Ricord, p. 425.

be followed by constitutional symptoms; while the late M. Vidal affirmed that all chancres were more or less indurated.

In England opinions have been as varied as in France. Thus, in the third edition of Mr. Langston Parker's work, where he speaks of the circumstances which particularly indicate the use of mercury in primary syphilis, the author includes "all sores which have yielded a characteristic pustule by inoculation." The indication for the employment of mercury, Mr. Parker says, is still more pressing if the primary sore be accompanied by bubo. (p. 15.) In support of this opinion, he quotes the following as from M. Ricord: "In such cases, six months never elapse without secondary symptoms manifesting themselves, unless a specific treatment be employed. This is an universal law which there is no means of eluding, but by mercurial treatment." These quotations profess to be taken from Stapleton's translation of Ricord. What M. Ricord himself, however, says, is exactly the reverse of this: "Tout bubon qui suppure spécifiquement, c'est à dire qui fournit du pus inoculable, n'est jamais suivi d'accident d'infection constitutionnelle."* M. Ricord's opinion, as here quoted from the original, is quite in accordance with the author's experience, and in direct opposition to the published views of Mr. Langston Parker.

The test by inoculation, again, is most fallacious as an indication of those sores which require to be treated by mercury. The genuine infecting sores commonly commence as a pimple, or crack, or a simple abrasion, from which there is little or no secretion; their progress is slow: they rarely become much inflamed, except artificially irritated, and they do not furnish a secretion of pus. It is often extremely difficult to inoculate such sores, and sometimes when they are inoculated, not a pustule, but a sore, showing the signs of the adhesive inflammation only, will result.

Case 1.—A medical student became diseased for the first time in the beginning of the present year. He inoculated himself on the thigh, and presented himself to me three or four days afterwards. The inoculation had succeeded, and became a small hard button-shaped sore, exactly resembling the original. A small quantity of white lymph was at first visible in the inoculation, but both sores subsequently remained as small hard circular indurations, furnishing scarcely any secretion from their surfaces.

Often such sores cannot be inoculated at all.

Case 2.—Matilda P., aged fifteen, was admitted into the Lock Hospital on the 9th of November, 1855. She stated that she had been diseased one week only. A well-marked indurated sore existed on the left external labium, secreting a thin fluid from its surface. Inoculation was carefully performed on the same day, with the discharge derived from this sore. The inoculation was followed with no result.

On the 23rd of November a copper-coloured eruption appeared on the body.

Case 3.—Julia B., aged twenty-one, was admitted into the Lock Hospital on the 10th of November, 1855. About three weeks previously

* Lettres sur la Syphilis, p. 199.

she had observed a pimple on the margin of the left external labium, which became a sore. On admission, this presented a circular outline, with a red glazed surface, and was surrounded by great induration.

November 12.—The secretion from the surface of the sore was inoculated upon the thigh.

November 15.—No result followed the inoculation.

Case 4.—James G., aged twenty-five, was admitted into the Lock Hospital on the 25th of October, 1855. He then had a large indurated sore near the orifice of the prepuce, which had appeared as a pimple four weeks previously.

October 30.—Several inoculations were performed on the left thigh with the secretion from this sore, which was abundant.

November 1.—No result from the inoculations.

November 6.—Still no result from the inoculations. A well-marked syphilitic eruption has appeared upon the body.

Case 5.—Thomas C., aged sixteen, was admitted on the 27th of November, 1855, with an extensive indurated sore extending halfway round the margin of the prepuce, causing phymosis. This had commenced a fortnight previously.

November 29.—The secretion from this sore was carefully inoculated on the patient's thigh in several points.

December 6.—No result had followed the inoculation. A copper-coloured eruption now made its appearance.

In none of the above cases had the sores apparently began to heal before the inoculations were performed. On the other hand, the readiness with which sores affording a copious puriform secretion are capable of being communicated by inoculation, is so well known that it would be superfluous to detail any examples. The inoculation of the puriform fluid on the same patient gives rise to a pustule, followed in general by a suppurating sore. The whole series of experiments on syphilisation lately performed on the Continent, show how rarely this kind of sore is followed by secondary symptoms. If, therefore, the production of a *pustule* by inoculation is to be regarded as of any value in determining whether or not mercury is to be given, it must be looked upon as evidence against the necessity of this mode of treatment, rather than for it.

The induration which accompanies syphilitic sores depends upon the effusion of lymph into the affected tissues, and the degree of induration depends upon the degree of effusion. The sense of touch affords a very correct test of the existence of this induration, but it does not give equally satisfactory information with regard to the character of the morbid action which may be taking place. It will not of itself always inform us whether the effused lymph will remain such until it be absorbed, or whether it will be converted into pus; whether, in fact, the inflammation be of the adhesive or the suppurative character.

The information which the degree of induration fails to give to the touch, the nature of the secretion examined by the microscope will often supply. The globules contained in the secretion of a sore accompanied by adhesive inflammation will often, it is true, at first sight, resemble those derived from a suppurating sore. But if the secretion, before being

examined, be treated with acetic acid, a clear distinction may be made between those cases in which the well known nuclei of the pus globules may be seen, and those in which they cannot.

This subject is illustrated by the following cases:

CASE 6.—Emma H., aged eighteen, was admitted into the Lock Hospital with an indurated sore on the 14th of February, 1856. The disease had existed three weeks. Upon the addition of acetic acid to the secretion from this sore, no nuclei of pus globules could be seen. This patient was treated by mercury.

CASE 7.—Anne H., aged twenty-four, was admitted into the Lock Hospital on the 13th of March, 1856. She stated that she had been ill three weeks, and had never been diseased before. An ulcer presented itself on the right external labium, with great general induration. Upon the addition of acetic acid the secretion from the sore presented, under the microscope, the distinct nuclei of pus globules. This patient was treated without mercury. The sore was healed on the 24th of March. Ten days afterwards, the induration was that of a common cicatrix only. On the 17th of April she was admitted, as permanently cured, into the Lock Asylum.

CASE 8.—Ann S., aged twenty-four, admitted into the Lock Hospital January 17, 1856, with sores on the corresponding sides of the labia, surrounded by general induration. The secretion from these sores, examined under the microscope, consisted of numerous globules, of the size and general appearance of pus. After the addition of acetic acid, the distinctive characters of the pus globules became evident. The field of the microscope was studded with circular nuclei, one, two, or three of which occupied the position of each pus globule. This patient was treated without mercury, and left the hospital on the 24th of January.

CASE 9.—Kezia K., aged twenty-two, had been repeatedly diseased, and had secondary eruption eighteen or nineteen months ago. She was admitted into the Lock Hospital with an indurated sore on the external labium on the 14th of February, 1856. The secretion from the sore afforded, upon the addition of acetic acid, numerous well-marked nuclei of pus. This patient was treated without mercury, and discharged cured on the 20th of March, the sore having healed on the 28th of February.

CASE 10.—William D., aged eighteen, admitted on the 14th of February, 1856, with two ulcers on the skin of the penis. These, he stated, had commenced three weeks previously, and had discharged matter during the past week. Upon the addition of acetic acid to the secretion from these sores, numerous distinct nuclei, characteristic of pus, were left. Non-mercurial treatment.

CASE 11.—W. O., aged nineteen, was admitted on the 12th of January, 1856. Had been previously diseased. At the time of admission an indurated sore existed on the prepuce, presenting to the eye all the appearances of an ordinary infecting Hunterian chancre.

January 17.—The secretion from the sore was scanty. Examined under the microscope, it showed a large number of very transparent cells, many of the same size, others of irregular size and form. Upon the

addition of acetic acid, no nuclei characteristic of pus globules were left. Treated by mercury.

CASE 12.—John E., aged twenty-two, admitted the 17th of January, 1856, with several indurated sores on the margin of the prepuce. On the addition of acetic acid to the secretion from these sores, no nuclei of pus globules remained. Treated with mercury.

Left the hospital, apparently cured, February 21st.

CASE 13.—Thomas C., aged nineteen, admitted with an indurated sore on the 17th of January, 1856, accompanied by an enlarged, but not inflamed, gland in the groin. Upon the addition of acetic acid to the secretion from the sore, no nuclei characteristic of pus were left. Treated by mercury.

CASE 14.—Samuel L., aged twenty-one, was admitted on the 4th of February, 1856. Had been previously diseased. On admission he had phymosis from indurated sores and gonorrhœa. When the prepuce could be sufficiently retracted, the discharge from the sore, and from the urethra, were both examined under the microscope, after the addition of acetic acid. The secretion from the urethra showed abundance of globules in which the nuclei characteristic of pus were visible; that from the sore showed no such appearance.

CASE 15.—John G., aged thirty, admitted the 28th of February, 1854, with a superficial indurated sore on margin of prepuce. The secretion from this sore, examined by the microscope, afforded no nuclei characteristic of pus after the addition of acetic acid.

March 18.—Brown stains appeared upon the nates. Treated with mercury.

CASE 16.—G. M., aged eighteen, admitted the 28th of February, with indurated sores on the margin of prepuce. The secretion from these sores showed no characteristic nuclei on the addition of acetic acid. Treated with mercury.

The following is a table of cases in which the secretion from syphilitic sores was determined by microscopical examination, in the manner above mentioned, to be purulent. The observations were made almost exclusively in cases of men, and where there was no apparent possibility of any other secretion becoming mixed with that from the sores. They extended over a period of eight months, ending in July, 1856. The 37 cases contained in the table were carefully registered from hospital practice. They were all treated, as a rule, without mercury; and in none of them were any secondary symptoms known to have followed. To these might be added a large number of suppurating primary sores, which have come under the author's notice during the last three years, which were treated without mercury, and in which no secondary symptoms showed themselves; but as the nature of the secretion from the primary sores in these cases was not examined by the microscope after the addition of acetic acid, the details are omitted.

The conclusion to which our cases and observations point is, that as the syphilitic poison may be, and generally is, destroyed by mortification

of the part in which it is contained, or as the same result may be produced by suppuration in an absorbent gland, consequent upon ulcerative inflammation; so may the deciduous cell-growth, or suppuration on the surface of a poisoned wound effectually eliminate the poison from the part.

Table of Cases of Suppurating Primary Syphilitic Sores.

Name.	Age.	Description of primary affection.	Treatment.	Result.	Remarks.
G. E.	26	Ulcer on prepuce	Tonic	Healed in 12 days	Accompanied by suppurating bubo.
J. B.	18	Ditto	Ditto	....	Ditto.
W. B.	30	Sore on frænum	Local	Cured in 4 weeks	
H. M.	17	Sore	Ditto	Cured in 1 month	Abundance of pus globules in secretion.
J. B.	20	Sore, surrounded by induration	Tonic	Cured in 3 weeks	Nucleoli observed to be very distinct.
J. D.	30	Concealed sores	Ditto	Ditto	Accompanied by bubo.
R. S.	23	Ulcer on base of glans	Local	Cured in 1 month	Suppurating bubo.
W. S.	31	Ulcer on penis ...	Ditto	Cured.	
J. D.	21	Concealed sores	Mercurial, for a week only	Cured in 4 weeks	Accompanied by bubo.
J. F.	24	Superficial ulcer	Local	Cured.	
H. W.	20	Sore	Tonic	Ditto.	
J. S.	23	Sore on frænum	Local	Ditto	Inflamed gland, which did not suppurate.
C. T.	26	Ulcer on prepuce	Ditto	Ditto.	
C. M. C.	22	Sores, accompanied by induration	Ditto	Ditto	Induration had no specific character.
F. W.	24	Sore on frænum	Ditto	Ditto	Pus globules abundant.
C. F.	20	Ulcer on frænum	Mercurial, for orchitis	Ditto.	
E. C.	23	Superficial sore	Tonic	Cured in 14 days.	
A. S.	24	Ulceration, surrounded by general induration	Ditto	Left the hospital nearly well.	
K. K.	22	Indurated sores	Iodine and sarsaparilla	Cured in 5 weeks	Had previously had constitutional disease.
A. H.	24	Sore, accompanied by induration	Tonic	Cured in 5 weeks	Pus globules abundant.
J. S.	21	Superficial ulcer	None	....	Slight case.
A. B.	20	Primary ulcers...	Tonic	Cured in 3 weeks.	
G. D.	23	Primary ulcer, with slight induration	Mercurial, for four days	Cured in 6 weeks.	
H. S.	20	Unindurated sore	Saline......	Cured in 14 days...	Open bubo.
G. F.	21	Primary sores ...	Tonic	Left nearly well ...	Bubo.
J. H.	18	Primary sore, accompanied by slight induration	Ditto	Cured in 8 weeks.	
G. C.	30	Sore on prepuce	Local	Cured in 3 weeks	Bubo in left groin.
C. L.	22	Concealed sores	Tonic	Ditto.	
J. R.	17	Phymosis and sores	....	Cured.	
J. J.	30	....	....	Supposed to be cured.	
R. P.	18	....	....	Ditto	First disease.
R. W.	22	Sore, surrounded by some induration	A few mercurial pills for orchitis	Ditto.	
R. J.	15	Superficial sores under prepuce	Simple ...	Ditto.	
J. B.	...	Primary sores ...	Ditto	Ditto.	
W. S.	24	Several small pustules, surrounded by considerable induration	Local	Ditto	Enlarged glands in both groins.
G. H.	27	Sores of seven weeks' duration	Ditto	Ditto.	
W. A.	...	Sores of two weeks' duration	Ditto	Ditto.	

PART FOURTH.

Chronicle of Medical Science.

HALF-YEARLY REPORT ON MICROLOGY.

By JOHN W. OGLE, M.B. Oxon., F.R.C.P.
Curator of St. George's Hospital Museum, and Physician to St. George and St. James's Dispensary.

PART I.—PHYSIOLOGICAL MICROLOGY.

CELLS—EPITHELIUM, PIGMENT, ETC.

*Epithelium.**—The epithelial cells of the conjunctiva were examined by Krause in the body of a criminal just beheaded, and found by him to be entirely of the pavement variety. None were ciliated. Those of the ependyma of the third, fifth, and lateral ventricles of the brain were also of the pavement form.

Phenomena in the Life of Pigment-Cells.—Busch† has made a series of highly interesting observations upon the various changes which take place in the pigment-cells of the skin. His attention seems to have been first drawn to this subject by portions of coloured skin separated from the larva of the frog and the triton, and by the web of the frog's foot. He found that often the cells put out projections from their walls, which went to form pigment-globules or balls, and as these projections lengthened, these globules became, as it were, pedunculated. The stalk then gradually thinned, and finally altogether retracted, leaving the pigment-globules isolated. The process was repeated in the same cell, and was a second time watched. Sometimes a species of sac was pushed out, which became divided into two, and after increasing greatly, became disjoined from the main pigment-cell, and another one was then pushed out in its place. Changes of form in the pigment-cell and in the peduncles were also observed, and are figured by the author. Busch also found the pigment-cells to be very contractile, and responsive to the stimulus of electricity.

Contractility of Animal Cells.—Busch, in the same paper as the one alluded to immediately above, describes the effect of electricity, which he applied to various cells besides the pigment-cells alluded to. He found that decided contractility was produced by it in the blood-cells of amphibia and in the various forms of recent epithelium-cells; but in cells with much granular contents, or which had undergone any fatty alteration, this effect did not follow the application of electricity.

MUSCLE, TENDONS, ETC.

On the Connexion between Muscular Fibre and Tendons.—Fick‡ details at length many observations on this point, his observations being chiefly made on the muscles of the frog. The condensed results of his examinations may be arranged as follows:—1st. The connexion between muscular fibre and tendon is the same in all

* Henle and Pfeuffer's Zeitschrift, Band vi. Heft 2. 1855.
† Müller's Archiv, No. 4, 1856, p. 415. ‡ Ibid., p. 425.

the muscles of the same animal, and, as a general rule, in various animals. 2ndly. Each muscular fibre has a certain bundle of tendinous fibres connected with it. 3rdly. The bundle of tendinous fibre is always of a smaller diameter than the muscular fibre belonging to it. 4thly. The flask-like projected tendinous bundle is invested by its muscular fibre as a sarcolemma. 5thly. Besides the flask-like investment, other inner threads of the tendinous bundle are connected with the appertaining muscular fibres, which probably extend themselves to a certain extent between the fibrils of the muscular fibre.

ARTICULATIONS.

On the Development of Joints. By Luschka.—The author, from observation, having determined that the pubic articulation was the lowest step in the formation of an articulation, conjectures that it might possibly represent a certain stage of development in other joints. Having found in the human and other fœtus, that a fibrous substance enveloped the articulating cartilage, and moreover that, in the cartilage of the pubic joints, such a material was spread over its hyaline foundation (*grund-masse*), and afterwards that variously-formed small microscopical projections were formed which, along with their substratum, passed into a fluid synovium; and that, finally, a cavity bounded by smooth cartilage was produced, he thought that this might be the ordinary process in the formation of all joints. He took for examination the union between the second and the seventh and intervening ribs with the sternum, and also that between the manubrium and the body of the sternum. In the above-mentioned cases, the union in early life is often by a species of continuity, and effected by means of a fibrous substance instead of by a proper articulation, which becomes lost in the cartilage-mass covering the costal sinus in the edge of the sternum. When a very small cavity exists, the cartilage of the rib and the sternum is covered with an extensive fibrillation, and exhibits on the surface turned towards the cavity, a very uneven appearance, owing to the projection of the fibre bundles undergoing destruction, the whole of the elements in the neighbourhood of the cavity indicating a gradual and progressive process of dissolution. The substance covering the cartilage is not thoroughly dissolved, as a rule, in the development of the sterno-costal joints, since in grown-up people, almost universally, at the rib-cartilage exists a layer, at one time homogeneous, at another time more striped, from which variously-formed leaf-like or branched projections are wont to grow into the cavity. That which, between the ends of the second and seventh, and included cartilages, and the sternum, exists as an exception—namely, the continuity by means of a fibrous substance—exists as a rule in the union between the body and the manubrium of the sternum. Here the union—unless it be, as it often is after the age of forty, bony—consists, almost without exception, of a fibrous substance which joins both the discs, consisting of pure cartilage. The fibrillation proceeds from the mass of the discs, without any line of demarcation, and contains, besides a number of cartilage cells, a very large amount of firm elastic fibre. The fibrous mass generally, in the instance of the second rib, is united with the angle dividing the cartilage into two facettes. Only in very rare cases is there any cavity between the manubrium and the body of the sternum. One case (a child aged two years, and also two adults) showed this. In the former the cavity was of the size of a lentil, and in the latter, of a coffee-bean—the inner surface not being smooth, but beset with leaf-like and fibrous projections growing from the sides, and sometimes almost filling the cavity. In these cases, the cavity is formed by liquidation of the originally solid connective substance, and thus the outgrowth of the remaining tissue takes place in the form of processes. Similar attempts in the formation of a joint are to be seen in what are termed "false" joints, where the connective material between broken fragments of bone, at first solid and fibrous, undergoes solution, and the formation of a thin

* Virchow's Archiv, No. 5, 1855, Heft v. p. 481.

capsule takes place; the synovia-like mass being seen to escape at times under operations. The lower-jaw articulation illustrates the normal maintenance of an earlier developmental stage. The thick elastic fibre-holding material overlying the cartilage substance, generally has a number of fine projections, which, although mainly projecting into the cavity, yet is at times partly grown together with the tissue of the meniscus. Similar illustrations are obtained in examining the connexion of the ribs with the vertebræ. Over a layer of hyaline cartilage at the head of the rib, and corresponding tubercle of the rib, exists a layer of fibre substance, gradually springing from the substructure, and exhibiting in the layer nearest to it a peculiarly-formed branching trabecular fibrous work, giving off forked fibres of various thickness, and finally terminating in pencil-like projections. Round spaces are thus enclosed by the mesh-work; and in the trabecular fibres, as well as in the structureless areolar tissue filling the spaces here and there, cartilage cells and elastic fibres are visible. The fine fibres radiate into the connective tissue towards the surface of the cartilage, which contains only a few cartilage cells, but many elastic fibres. With exception of the above-mentioned joints—namely, of the ribs, the sterno-costal and maxillary joints—in adults and in the natural condition there exists over the cartilages, either no material differing from their own, or only one of extreme tenuity, and free from elementary forms. Quite different is it with the fœtus and newly-born. In these cases, over the cartilages there exists a substance, sometimes fibrous, sometimes homogeneous, or only slightly striped, which proceeds from the substructure so entirely without demarcation, that it may without hesitation be considered as belonging to it, and only resulting from the development of the joint. What chiefly appears, and one of the best instances exists in the joints of the toes in the newly-born, is the variously-formed outgrowths from the surface, in great numbers. In the cartilage of the hip, knee, and shoulder-joints, and others also, they are more scanty. The most ordinary form is the foliaceous. Along with the single projections, bush-like and branching forms also exist, which, like the substance from which they grow, are at one time structureless or finely striped, at others fibrillated and twisted, behaving like ordinary areolar tissue under reagents. In some are to be seen elastic fibres and cartilage cells. Those cases showing only traces of the projections, are specially interesting as giving an insight into the formation of synovia by solution of the tissues and the smoothening of the cartilage in a complete joint.

SECRETING GLANDS.

Kidney.—Busch* has made a communication on the anatomy of the kidney. After alluding to the investigations of Goodsir and Müller regarding the formation of secretion in cells, which burst and allow its outflow, he specially mentions the discovery by Müller, of the formation of secretion vesicles containing clear fluid and uric acid salts in the interior of the cells, which grow and occupy the whole cell eventually, and finally the granules of salts are liberated into the excretory ducts. The snail is particularly referred to. Müller concluded that only the secretion vesicles were excluded, and new ones formed by the cells. The author had observed granules of uric acid salts also between the secretion vesicle and the cell wall, and some cells also with these granules and no secretion vesicles. Hence the latter are not necessary for the filling of the cells with urinary precipitates. In almost all cases, the first amorphous granular urinary material, forms before the vesicle, whose walls are formed out of the cell-contents round the partially precipitated urine. The author enters at length into the discussion regarding the chemical character of the urinary deposit, whether it be a simple acid, or a salt, and what salt; and also regarding the proof that the urine is not brought to the kidneys as a salt, and soluble, but that the cells of the gland produce it by a chemical process out of the material brought to them. He goes on to speak of

* Virchow's Archiv, Band iv. 1855, p. 363.

the different views of the relation between the Malpighian bodies and the urinary tubes, and determines, by his observations on snakes, that they are decidedly enclosed in a capsule, being the enlarged termination of the urinary canals. The snake has the Malpighian body quite at the termination, whilst the triton only has it in a wider part of the canal; and in the snake ciliated epithelium exists at the margin, uniting the body to the tube just as Bowman described in the frog. The chief part of the wall of the capsule is lined by a polygonal epithelium, and on the free surface of the knot of vessels inside the capsule a distinct epithelium may be seen in fortunate cases, which in the embryo of the cellular matrix may occasionally be witnessed crossing bridge-like from one convolution of the vessels to another; but no connective tissue was visible between the cells and the vessels. The vessels seem to pierce the capsule, carrying a layer of epithelium before them. In general, the Malpighian vessels in the lower animals are merely windings of the same vessel, but in some, as in the viper, divisions and ramifications of the vessel existed. At least, in the kidneys of snakes the ciliated epithelium is seen entering low down the tubes, provided that water is not used in the preparation. Each cell is seen, when the movement becomes slow, to have only one cilium, which moves about like a whip-lash. Ciliary movement could not be seen in the kidneys of birds.

VASCULAR GLANDS.

The Spleen.—C. O. Eberhard* considers the function and structure of the spleen in an inaugural dissertation at Erlangen. He considers especially the relation of the Malpighian bodies to the lymph vessels. According to him, the arterial twigs do not lose themselves in the corpuscle, but the union between them consists merely in that of the areolar tissue of their investment and the sheaths of the vessel. Each corpuscle is limited by a colourless investment of a double contour, formed of areolar tissue, with indistinct fibrillation, containing also a fine network and tolerably straight-running elastic fibres. The contents are whitish and albumen-like, showing the same microscopical formation as the surrounding spleen pulp; and the parenchyma of the corpuscle is penetrated by a fine network of capillaries entering from outside, though not directly arising from the arterial twig. These capillaries have very thin walls, so that an injection by the splenic artery often fills the corpuscles. The author then alludes to the similarity between the Malpighian corpuscles and the solitary and Peyer's glands of the intestine, and the connexion, which Brücke has proved by injection, between the lymphatic and Peyer's glands. Yet it is not possible by direct injection of the deep lymphatics of the spleen to fill the corpuscles. In injecting from the artery we very often get not only the corpuscles filled with extravasation, but it also finds its way into surrounding vessels, where projections show them to be lymphatics. Sometimes, too, lymphatic vessels are to be seen departing from a Malpighian body, in opposition to the opinion of Kölliker, who looks on the Malpighian corpuscle of the spleen as a peculiar form of simple terminal lymph-gland. Hence we have direct proof of the communication between the deep splenic lymphatics and the Malpighian corpuscle; and this view receives support from the fact that similar connexion exists in fishes and reptiles. The author then alludes to experiments in proof, made by the removal of the spleen during life, and others, and the results obtained, which may be summed up as follows:—1. The rapid deposit of pigment in mesenteric glands. 2. The regeneration of the spleen in frogs after extirpation; the more interesting, as in frogs lymphatic glands are wanting. 3. That fluid introduced into the stomach entered the liver more quickly than the spleen, and that the time of entering the spleen was the same as in the case of the mesenteric glands.

* Quoted in the Vierteljahrsschrift für Pract. Med., Band iii. p. 32. 1856.

NERVOUS SYSTEM.

On the Reparation of Divided Nerves.—Leut* found that on dividing nerves in animals, the divided portions at both ends showed an increase of the nuclei of the neurilemma, probably arising by division of the normal nuclei. Out of these nuclei proceed most likely new cells, and from these, fibres, establishing a communication between the ends of the nerves. After the union, new nerve-marrow forms in the nerve-sheaths of the primitive fibres of the peripheric part of the nerves, which had been emptied by the section; but the author could not clearly discern any axis-cylinder in these parts, which Schiff asserts can be seen after placing the preparation for several hours in a solution of bichloride of mercury, and then adding acetic acid.

Papillæ.—The papillæ at the tip of the tongue in a criminal beheaded, presented no tactile bodies.†

Part II.—Pathological Micrology.

TUMOURS, MORBID DEPOSITS, EXCRESCENCES, CYSTS, ETC.

Cancerous Deposits.—S. Van der Kolk‡ has a long paper On the Extension of Cancer-Cells to the Neighbourhood of Cancerous Tumours, and its Pathological Consequences. He details his observations on the method of propagation of carcinoma, and for the greater convenience of accurate examination, paid attention to cancer of the lower lip. He says that, having amputated the lip affected with epithelial cancer, he cuts it in a longitudinal direction, so as to have an anterior and a posterior portion, and finds that the lesion extends more in length than depth. On making a fine section parallel to the original one, the tumour will be found to consist of an aggregation of epithelial cells, which are of smaller size in proportion to the distance from the margin of the tumour. On examining the adjacent parts, which to the eye appear healthy, one finds numbers of small cells or nuclei, and finally a granular substance, with molecules of fat among the healthy tissues, in abundance proportionate to the distance from the cancer growth. The extension exists chiefly under the epidermis. One may often trace the passage of nuclei, around which a cellule is hardly formed into complete epithelial cellules. The transformation of granular substance and nuclei into cellules is seen on examining further. The nuclei and cellules are seen to be spread among the fibrous tissue, between the muscular fibres, and some along the muscular fibres themselves. In other cases, nothing but a granular substance and nuclei, in their first stage of development, mixed with fat, are to be seen. The muscular fibres, even where quite covered with nuclei and small cellules, showed their proper structure when cleared. In some places, the transverse striæ were effaced, and a degeneration existed with a kind of dissolution of muscular fibres, which appeared to be resolved into cellules. When the muscular fibres were surrounded by sufficient nuclei to be observed, the fat globules diminished in number, as if repelled by cellules and nuclei in the intermediate tissue. The nerves made great resistance to this degeneration. In some places one found the nervous fasciculi surrounded by epithelial cells, without the least trace of decomposition inside the neurilemma. It is only exceptionally that there exist between the nervous fibres, any nuclei or small cellules. A large proportion of cellules and nuclei occupy the follicles and crypts of the chin, from which the constituent parts had been successively removed after having probably undergone the fatty degeneration and dissolution.

It appears certain that an exchange takes place between the epithelial cellules and the interstitial liquid. The fluid receives from the cellules new principles, which do not exist in the fluid of healthy parts; and the two fluids—that between

* Zeitschrift für Wissench. Zoologie: quoted in Canstatt's Jahresbericht, p. 32. 1855.

† Henle and Pfeuffer's Zeitschrift, Band vi. Heft 2.

‡ Arch. Générales, Jan. 1856: quoted from Henle and Pfeuffer's Zeitschr., Band v. p. 127.

the cellules and that between the healthy parts—appear to become more or less mixed, and thus the liquid penetrates along the course of the fibrous tissue; and it is there that the nuclei and cells of new formation are deposited, and spread to places more distant. The author then goes on to mention cases in which the examination of portions of the surface after extirpation of the lip, confirmed the prognosis made by him. The altered fluid not only infiltrates the tissues, but may be absorbed by lymphatic vessels, and so transplant the malady; and this is the case with epithelial as well as cancers, properly so called.

The author alludes to similar cases mentioned by Donders, Bennett, Lebert, and Hannover, and supposes that the altered and absorbed parenchymatous fluid generates the epithelial cellules within it, not by the transport of cellules ready formed, but by reason of a chemical modification in its constituent elements. In fact, the cancer cellules may be produced inside the sarcolemma of muscular fibre, which could not give passage to a single nucleus. He mentions also a case of epithelial cancer of the tongue, wherein the root of the tongue was affected, and in which a large ulcerated tumour existed in the neck, which presented the characteristics, not of epithelial cancer, but of ordinary cancer, proving the position denied by Hannover and Lebert, but entertained by Bennett and Schrant, who admit the metamorphosis of one form of cancer into another. The author regards ordinary and medullary cancer as only the acute form of epithelioma, and asserts that the parenchymatous fluid acts in the same way in these cases as in epithelioma. He adduces cases which we cannot detail here. In one of them, besides the muscular fibres being transformed into fibrous cells, more or less long and numerous, the tibial nerve was greatly altered, the primitive nerve fibres being atrophied where the nerve appeared to be transformed into a fibrous tissue, mixed with cellules and molecules of fat. The cancer cells may penetrate the walls of veins, and, though rarely, of arteries. In two cases of medullary cancer of the liver, in a part of the tumour developed in the portal vein adjacent—which is often the seat of medullary cancer—the capillaries of this part, enclosed in the cavity of the vein, were injected a fine red colour through the arteries. In those cases in which the fungus has penetrated the mucous walls, no membrane of any kind was seen to envelope it, or separate it from the blood's current. In the propagation of cancer by the fluid before alluded to, very often many nuclei are developed in a single cellule, which breaks and allows of their escape; but this latter formation never takes place in epithelioma. As regards the implication of nerves, the author then mentions a case of extensive cancer of one-half of the tongue, in which he examined the lingual and hypoglossal nerves of the other side, neither of which showed the presence of any tubules within them; yet on removing a very small portion from the middle of the hypoglossal nerve, cancer cells were seen. A nerve which will resist surrounding suppuration will be apparently occupied by cancer; and the author thinks that the extreme pain so often felt in parts attacked by cancer, is the result of the deposition of cellules within the nerves, and that the time for operation is probably passed when this occurs. He proceeds to counsel the immediate extirpation of any tumour or induration which might give rise to cancer; as, when once the cancer is formed, the cellules increase very rapidly in size and number, and are no longer separated from surrounding parts by a thick fibrous layer, the mass softens, being entirely composed of cells, and is past the relief of any operation.

On Scrofulous Deposit.—Küss* considers that pulmonary tubercle, so called, only consists in the heaping together of epithelial cells in the various vesicles of the lungs, and that all its metamorphoses arise from their destruction. In this way, he asserts that tubercle in the various glands, the intestinal mucous membrane, and the medulla of bone, &c., is easily to be explained as arising from the globules existing therein. Thus, according to him, tubercle is not a foreign heteromorphous material, but simply the results of proliferous cell elements. By Mandl†

* Canstatt's Jahresbericht, 1855, p. 39: from Gaz. Méd. de Strasbourg, Août 25, 1855.
† Archives Générales de Med., Avril, 1855.

scrofulous deposit is considered to be a crude mass, only exhibiting corpuscular and other arrangement as the result of the method in which it is torn up. He determines that it has no specific histological character, and that the fatty and shrivelled elements of reticulated cancer, and many other products, are to be compared exactly to tubercle corpuscles. According to Engel,* tubercle at first consists of an amorphous exudation, in which cells are formed at a later period, which gradually undergoes a fatty or calcareous change, and softens.

Pathology of Lupus.—K. H. Mohs,† in a Dissertation published at Leipsic, describes this affection as consisting of a true hypertrophy of the cutis, and ranks it amongst the so-called sarcomatous formations. The hypertrophy seems to arise from the division of the normal cutis cells, and not from a free and independent cell-formation. The author examined with the microscope a perfectly recent portion of tuberculous lupus of the face. The cutis was thickened and transparent, and of the consistence of fine glue. In the subcutaneous fatty tissue, some knotlike places existed, of the same character as the substance occupying the corium. The epidermis was thin. The peripheric part of the diseased places was tuberous, beset with hair, the middle portion being smooth and devoid of hair. The minute examination of the diseased tissue showed a granulated mass, beset with trabeculated areolar tissue-material, containing nuclei, round and oblong, and spindle and biscuit-shaped, and of a yellow colour, with one or more nucleoli, and having an intervening, transparent, hyaline, slightly-granulated material; and the knotted parts in the fatty tissue before spoken of had the same microscopical characters as the general mass. Moreover, the latter tissue was occupied by much connective tissue, having an areolar arrangement, and containing capillaries but no nerves. The papillæ in the peripheric parts were normal or slightly enlarged, but in the central parts they were in small numbers of various sizes, flattened or lobate, and containing one or more vascular loops. The hair and the skin follicles in the peripheric parts were normal; but towards the central parts, elongated bodies, corresponding to the hair follicles, and consisting of united group-like epidermis cells, existed. No follicles existed in the middle portions. On tearing up the fibrous tissue, irregular masses were observed, of a clear granular material—so clear indeed in outline, that they were to be looked upon as cells of a round, oval, or spindle shape, and mostly of about half the size of epidermis cells, being probably formative cells of areolar tissue. Similar appearances were observed in an ulcerated lupus, but in the latter case the nuclei were larger and more numerous, and the areolar tissue fibres more scarce.

Spermatic Cyst, with Hæmatocele.—M. Chassaignac‡ relates a case, in the person of a man, aged sixty-nine, who came into the Hôpital Lariboisière with a tumour of seven or eight years' standing at the right side of the scrotum, for which no cause could be assigned. Its greatest diameter was twenty-two centimètres; it was elongated vertically, and largest at the lower part; its consistence being that of a hydrocele moderately distended. The position of the testicle could not be ascertained. The upper part of the tumour was close to, but distinct from, the external inguinal ring, and the skin of the penis was drawn up, helping to form the covering of the tumour. On puncturing the tumour, a quantity of reddish fluid escaped, becoming frothy on agitation, but containing no clots. The fluid yielded a very abundant precipitate. On the addition of nitric acid and alcohol, and the application of heat, it exhibited, when examined by the microscope, a very large number of spermatozoids, perfectly developed, but not in motion. Large numbers of spermatic vesicles, but less in number than the spermatozoids, also existed; and also a certain number of more or less attenuated blood corpuscles. Some of the fluid agitated with a few drops of oil, assumed a cloudy grey colour, from the formation of an emulsion. This was done with reference to the view of M. Gos-

* Prager Vierteljahrsschrift, Buch, 1855, Band xii. p. 1.
† Schmidt's Jahrbücher, No. 3, 1856, p. 306. ‡ Gazette des Hôpitaux, Juillet 3, 1856.

selin,* who supposes that the milk-like character of the fluid often seen in such cysts, was owing to the emulsive power of the sperm acting on the fatty matter secreted by the cyst. The testicle was, in the above case, found to be quite healthy, the cord only being a little enlarged. The author believes the tumour was the largest of the kind known, and deems it peculiar, as simulating hydrocele of the tunica vaginalis. We lately had the opportunity of witnessing the fluid removed from a similar cyst by Professor Hewett, of St. George's Hospital, containing similar spermatozoids.

SECRETING GLANDS.

On the Kidney.—Becquerel† treats upon the form of disease named after Bright. He describes the affection as embracing four different varieties, thus summed up:—1. Hyperæmia of the cortical parts and Malpighian bodies, and often with exudation of albumen or blood into the urinary tubes. 2. Fatty degeneration of the urinary epithelium, the cells being finally destroyed, and the empty canals collapsed or filled with fibrinous exudation. This fatty exudation may also take place into the tissue between the urinary canals, with or without albumen. 3. Albumino-fibrinous deposits, in streaks or masses, forming the so-called granulations, and existing in the urinary canals, Malpighian bodies, or intervening tissue, and capable of organization. 4. Infiltration of the urinary cells with protein molecules, and enlargement of the cells which fill out the urinary canals.

All the above forms may be, according to the author, isolated or combined.

VASCULAR GLANDS.

On the Spleen.—Förster‡ found hypertrophic growth of the Malpighian bodies in one instance of great enlargement of the spleen. The viscus was also very indurated, and beset with round white knots, which were situated partly in the deep parts, and partly at the periphery. Each of these knots was composed of a number of smaller ones. Besides these knots, others, isolated, and equal in size to a hemp seed, were seen. These proved to be Malpighian corpuscles. By increase of the normal cells and of the vascular scaffolding, the corpuscles increased to the size of a hemp seed. Thus the mass grew in a lateral direction, and there arose small irregular and almost dendritic formations, consisting of cells of the corpuscles and capillaries. The larger knots, formed by the grouping together of many such, were partly soft and partly hard and dry, by atrophy and cheesy alteration.

Führer, whose observations on the Anatomy of the Spleen we gave at p. 529 of No. xxxii., October, 1855, in a late communication to the Dutch Medical Society at Paris, determines four general diseased conditions of the spleen. 1st. The plethoric form, in which the microscope showed the Malpighian bodies to be highly developed, the organ being dark, granular, and large. 2nd. The puerperal spleen, compact, and of large circumference. 3rd. The chlorotic spleen, containing numerous gelatine-like transparent corpuscles, of a light red colour, and large. And 4th. The atrophic spleen of the aged and emaciated.

On Fœtal Glandular Tissue in Tumours of the Thyroid Gland.—Billroth§ describes at length the above-named condition. After alluding to the natural form of development of the thyroid gland, specially examined by Remak, but confirmed by his own observations, he applies the results of observations on this point to its pathological anatomy. In detailing the development of the thyroid gland, he describes the single vesicles as arising from cell-composing cylinders,

* Gazette des Hôp., Août, 1855.
† L'Union Médicale, Mai, 1855.
‡ Canstatt's Jahresbericht, 1855, p. 43.
§ Müller's Archiv, 1856, Nos. 1 & 2, p. 144.

arranged in a radial direction. The cell-layer composing the wall of the single vesicle thickens, forms clavate processes, in which is developed a hollow space, and the process separates itself as a new vesicle. The formation of the hollow space in the process seems to be often unconnected with that of the original vesicle. The author details a case of a large thyroid tumour in a woman, aged sixty-seven, punctured during life, and removed after death. The fluid evacuated during life showed, under the microscope, molecular matter, fatty granules, granular pigment, but no pigment crystals. A quantity of flocculent material also escaped, consisting partly of amorphous clumps of destroyed tissue, and partly showing the same appearances as the general mass when cut into. The mass of the tumour was elastic, its colour being yellowish white, but it contained small dark apoplectic-like spots, and on section gave out a thick whitish granular material containing variously-shaped cells, some being homogeneous, some being finely granular and fatty, and many nucleated, increasing by division; others were without nuclei. By far the chief part was composed of large globules and cylinders, consisting of cells, and containing a hollow space. The peripheric layer of these cylinders and globules was composed of cylindrical cells. The tumour was evidently not one of ordinary hypertrophy of the thyroid gland, but made up of a peculiar tissue, which had many analogies with cystoid of the kidneys, and so called cysto-sarcoma of the mammary gland. Nothing could be gathered from the small part of sound gland remaining on the affected side of the neck, showing how the embryonal textural elements formed themselves out of normal follicles. The foundation of the embryonal glandular pouches and vesicles was laid down in solid club-shaped bodies, consisting of cells and sprout-like processes, into which the cavity extended, in part arising from the canal of the mother tissue, and partly arising alone and isolated. These processes were often very small, and might consist of a row of cells lying one behind the other, to which was connected a vesicle made up of a circle of cells. In the spaces no longer filled with cells, a fluid or slimy homogeneous substance existed, arising either from the dissolution of the central cells, or by a kind of secreting activity of the cells. Along with this increase of the gland-elements by sprouts, they also increased by the excessive growth of the parietal cells of the glandular vesicles; and before an outgrowth was produced in the centre of this cell-mass, a new hollow space was formed. These embryonic elements, as in the case of gland tumours generally, did not attain their full development, but only certain steps, and these underwent fatty or colloid changes, large pale globules with an irregular cavity, like colloid globules, being formed, undergoing dissolution, and corresponding to an association of cells in glandular vesicles.

RESPIRATORY ORGANS.

The Lungs.—The subject of induration of the lungs, and consequent changes in the bloodvessels, has been commented upon at length by Heschl, of Krakow.* The instances of simple induration were chiefly in the bodies of those who were enfeebled by long-continued disease of the liver, spleen, or kidneys; but it was difficult to make out whether it resulted from the disease. He adverts to the observations of Rokitansky, who denies that the change is owing to chronic inflammation, although he does not prove its absence—and of Förster, who asserts, but does not prove, its existence. He quotes at length three cases. In the first, a man, aged nineteen, who died of dropsy and ague, the solidified parts of the lung, besides infiltration of the pulmonary vessels, with exudation matter and pus corpuscles, showed a large amount of fatty degeneration of the alveolar epithelium. In the pus-like fluid expressed from it were numbers of granules and pus-cells, connected by a molecular material; but besides these there existed granule-cells, with one or two projections and wedge-shaped, rounded, and spindle-shaped cells, like those of areolar tissue, and also pus and blood-globules. More-

* Vierteljahrsschrift für die Praktische Heilkunde, 1856, Band iii. p. 1.

over there were large roundish globules of a hyaline substance, beset with fat granules and granule-cells, gradually soluble in caustic alkali. In sections of the lung the trabecular work was found to be two or three times thicker than usual, and appeared to consist entirely of connective tissue in various stages of development; and at the margins of the alveoli, corpuscles with nuclei were seen projecting, looking at first like epithelium. The ends of the trabecular work were also found to consist of spindle-shaped elements, covering the elastic tissue, which appeared to be unchanged, excepting that in places it was broader. There can be no doubt that a new connective tissue formation had arisen in close connexion with the trabecular work of the lungs. From the walls of the alveoli of the lung, threads projected into the cavity, simple or branching, and these were beset with cells projecting at the sides, and giving the appearance of a thorn stick. These had double contours, and besides the cells possessed hooked and screw-like outgrowths, with a double contour, and of the same diameter as the threads themselves, and were undoubtedly the lung-capillaries. In places these contained true blood-corpuscles, and many trabeculæ of the lung parenchyma contained a network of such vessels, in whose meshes were the spindle-shaped cells. The nerves of the lung were often quite clear. The walls of the bronchi, as well as the interlobular cell material, had no connexion with the new elements, the capillaries, owing to the new formation being separated and isolated. As regards the question of the first origin of the cells which were changed into the spindle-shaped corpuscles, the author relates the case of a girl, aged twenty, in whose lung, which was becoming indurated, the cells were full of exudation matter, and the epithelium and alveoli gone. In the lower part of the lung the parenchyma was more than usually transparent, and in the walls of the alveoli, besides the nuclei of the vessels, round and oval nuclei of from $\frac{1}{450}$ to $\frac{1}{500}''''$ in size were seen; and in the upper lobe they existed in heaps of from two to four in number. Nuclei, with two nucleoli, were also seen. Very often beyond the boundary of an alveolus very delicate meandering lines passed, within which nuclei, from one to three lay close together; and similar accumulations were also found within the trabeculæ themselves. Within the same lines bulging out blood-corpuscles were seen, and there seemed to be no doubt that the finely-contoured lines were the boundaries of corpuscles. No question arose as to what became of the fast-growing and increasing nuclei. No doubt the spindle-shaped cells have all the properties of formative cells of connective tissue, and finally pass into areolar tissue. By degrees the lung obtains almost a tendinous coherence, and nothing remains visible but areolar tissue, and a few vessels and pigment particles. The closure of the alveoli seems dependent on the shrinking of the newly-formed areolar tissue. The bronchial tubes appear to be thickened as to their walls in the later stages, their calibre being increased when only small parts of the lung are affected, and narrowed when larger parts are affected.

The author finds the nerves of the indurated part of the lung obsolete; but as he has never found the entire lung affected, he has not found the trunk of the vagus nerve affected.

OSSEOUS SYSTEM.

On Osteomalakia.—Swaagman* describes at length a case of this affection which he had the opportunity of examining, the subject of it being a woman, aged forty. The bones, which were bent, could, when recent, be easily cut with a knife; but when dried could not be bent, and were white and hard. When dry they were very light in weight—the entire arm, and scapula, and clavicle only weighing four ounces three drachms; and the whole lower extremity only five ounces six drachms.

* From the Tijdschr. der Nederl. Maatschapij.: quoted in Canstatt's Jahresbericht, 1856, p. 51.

All the bones swam in water; they were moreover diminished in size. The compact bony substance of the long bones, and the outside of the spongy bones, were very porous; and this porosity was seen by the microscope to depend upon an enlargement of the Haversian canals; but no other changes could be observed in the structure. Chemical examination showed a diminution of all the elements, and only a slight increase of the saline over the organic portions. Hence the changes undergone might properly be described as atrophy or osteoporosis; and the various accidents of bending, fracture, &c., as depending upon the amount of porosity produced. This porosity seems to be a quickening of the process of conversion of the compact into spongy tissue, and is probably owing to inflammatory hyperæmia, and therefore by no means to be confounded with the affection termed rachitis.

The following papers, bearing upon physiological and pathological micrology, are interesting, but we have not space to do more than allude to them:—

H. Müller on the Retina. (Zeitschrift für Wissensch. Zool., Band vii. Heft 1.)
R. Blessig on the Retina. (Inaugural Dissertation, Dorpat, 1855.)
Leydig on Textile Corpuscles and Muscular Fibre. (Müller's Archiv, 1856, i. and ii. p. 150.)
E. Hirts, of Zittau, on the Numerical Proportions between the White and the Red Blood-Cells. (Müller's Archiv, 1856, i. and ii. p. 174.)
Notices of Parasites, by Krämer, of Gottingen. (Illustrirte Medizin. Zeitung. Band iii. Heft 6, p. 1.)
Müller on Morbid Deposits on the Inner Surface of the Choroid. (Verhandlungen der Physicalish. Med. Gesellssch. in Würzburg, Band vi. Heft. 2.)
On Cysts of the Kidneys, by Otto Beckmann. (Virchow's Archiv, Feb. 1856.)
On the Anatomy of Mucous Polypi, by T. Billroth. (Virchow's Archiv, Feb. 1856.)

HALF-YEARLY REPORT ON FORENSIC MEDICINE & TOXICOLOGY.

By Benjamin W. Richardson, M.D.
Physician to the Royal Infirmary for Diseases of the Chest, and Lecturer on Forensic Medicine at the Grosvenor-place Medical School.

I. Toxicology.

We have grouped together, in this Toxicological Report, sets of cases illustrating, by varied examples, the effects of a few individual poisons. The reader will thus receive at a glance many important practical points, opening a wide scope for reflection.

We wish specially to say, that out of the numerous cases before us which the literature of the past few months affords, we have selected none but *boná fide* cases for illustration; we mean cases where the evidence was positive that the symptoms arose from poison, and nothing else, the poison itself being detected. This rule we consider as absolutely necessary in strict science. First, because the symptoms produced by poisonous agents being always pretty correct copies of one or other forms of natural disease; the mere symptoms of natural disease, if carelessly observed, or if tampered with and touched up by interested forensic persons, admit, on the bare evidence of symptoms, of being at any time transformed into effects arising from some poison. Secondly, because the symptoms arising from the same poison in different cases vary so materially that it becomes utterly impossible to establish any diagnostic system or rule regarding the effects of poisons. Our present report proves this fact abundantly. We have several cases of arsenical poisoning, without any appearance of epileptic symptoms. We have one case in which epilepsy is the marked symptom, and the seeming veritable and final cause

of death. We have three cases of sulphuric acid poisoning—in two, the symptoms and pathology are all referrible to the stomach, in the third they are all referrible to the larynx and lungs, the stomach escaping entirely. We have nine cases of strychnine poisoning; in some of these there is no sign of trismus, which some suppose to offer a point of diagnosis between strychnism and tetanus; in three cases we have trismus as one of the most characteristic symptoms. In one case, Dr. Kirk's, the symptoms collectively might easily have been confounded with those of tetanus; in another case, that related by Dr. Shaw, the symptoms might as easily have been confounded with those of hysteria.

A knowledge and an acknowledgment of these variations in the action of the same poison is then necessary. For, although it is not to be expected that diagnosis can ever lead to more than the suspicion that a poison is at work, still it is important in the observations of disease to be able to detect suspicious symptoms, so that their positive causes may be sought for and removed with greater certainty and expedition.

ARSENIC.

Poisoning by Arsenic—Treatment by Hydrated Sesquioxide of Iron.—Dr. James Walsh records several remarkable cases of poisoning by arsenic, which tend to indicate some value in the treatment by hydrated sesquioxide of iron.

Case I.—A stout man, aged twenty-three, purchased two hundred and fifty grains of arsenic, ostensibly for poisoning rats. He took the whole, and rinsing the cup, swallowed the dregs. He had taken breakfast at six A.M., and took the arsenic at noon. A girl saw him take the poison, and alarmed his friends, who made him swallow some melted butter and salt. Within an hour after he had taken the poison he was seen by Drs. Walsh and Betts and Mr. Evans. A drachm of sulphate of zinc was given, which produced free vomiting; the vomited matter was found subsequently to contain arsenic. Carbonate of iron was given, in molasses, in ounce doses, and being allowed three minutes to act on the arsenic, was then evacuated by half a drachm of sulphate of zinc administered in a pint of warm water. This was repeated six times, before the arrival of some hydrated sesquioxide.

A burning pain in the stomach, of which the patient complained, and the restless nervousness and pinched face that accompanied the pain, disappeared on his swallowing the first dose (eight fluid ounces) of the hydrated sesquioxide. This was followed by the zinc, as above mentioned, and repeated in doses of four ounces every five minutes, until there were good grounds for believing that the arsenic was nearly all neutralized and removed. To prevent the evil effects of any portion of the poison that might have passed the pylorus, Dr. Walsh gave four ounces more of sesquioxide, followed by an ounce and a half of castor oil. The oil not only acted freely on the bowels, but carried the iron before it through the whole intestinal canal. The man was about his business next morning, and had no further bad symptoms. The quantity of the hydrate used was two pounds, of the carbonate seven ounces, and of sulphate of zinc two ounces.

Case II.—A delicate man, aged twenty-five, a tailor, took six hundred grains of arsenious acid. He mistook the arsenic for cream of tartar, and had no idea of suicide. He breakfasted at eight A.M., took the arsenic two hours after, dined at noon, and, having learned the dangerous mistake he had made, called on Dr. Walsh at a quarter past twelve. Carbonate of iron was first given, followed by sulphate of zinc, and afterwards the hydrated sesquioxide of iron, with zinc and oil, were given in precisely the same manner as in the preceding case. Not a bad symptom was left, and the man was at his work next day. The remaining contents of the paper, and some of the vomited matter, both gave satisfactory proof of arsenic.

Case III.—A woman took half-an-ounce of arsenic in Jersey City, to kill herself, and crossed the ferry to New York. The police were informed of her

attempt, and brought her to Dr. Scoville, of the police-force. The treatment was the same as that pursued in the previous two cases, and with the same success. Neither stupor nor convulsions followed in any of these cases, though these symptoms are maintained by some writers to be present in all cases where large quantities of the poison are taken.

CASE IV.—A. M., aged fourteen, swallowed half-an-ounce of Fowler's solution. Dr. Walsh was called in half-an-hour after the accident. An ounce of tincture of iron and one ounce of liquor potassæ mixed, was given in doses of a tablespoonful, followed by sulphate of zinc every five minutes, for six repetitions of the dose. The pain ceased. Another dose of iron, followed by an ounce of castor-oil, operated freely, and left the patient quite well.

CASE V.—A. B., aged six years, took two teaspoonfuls of Fowler's solution, and soon suffered from pain in the stomach and vomiting. Two ounces of tincture of iron and half-an-ounce of carbonate of soda were administered in teaspoonful doses every five minutes, followed by ten-grain doses of sulphate of zinc in warm water, until one ounce of the iron mixture had been taken. Two teaspoonfuls of iron mixture and half-an-ounce of castor-oil finished the course. The patient was well next day.—*New York Journal of Medicine*, May, 1856.

[The above results are, perhaps, more satisfactory than any that have been published by one author, in regard to the hydrated sesquioxide of iron as an antidote for arsenic. It is to be presumed that the success depended on the freedom with which the iron was exhibited, and on the copious vomiting induced by the zinc.]

Poisoning by Arsenic: Treatment by Hydrated Magnesia: Recovery.—Dr. Pool relates a case of poisoning by arsenic occurring in eight persons (three men, four women, and a child a year and a half old), treated successfully by the early administration of hydrated magnesia. All the patients had partaken at dinner of a sauce with which a spoonful of arsenic had been mixed by mistake for flour. Immediately after the meal, most acute symptoms of poisoning were manifested, first in the child, and then in the others. Dr. Pool endeavoured, by the copious administration of warm milk-and-water, to encourage vomiting, which had already occurred in several of the patients, and had been provoked in the rest by inserting the finger into the throat. As an antidote, a warm mixture of calcined magnesia with water (one part in fifteen or twenty) was administered in teaspoonfuls every five or ten minutes. Half-an-hour later, the burning sensation in the œsophagus and stomach, and the severe colicky pains and the vomiting, had gradually ceased, and had given place to copious stools, which in some were bloody. The general symptoms, too, produced by the absorption of the poison into the blood, gradually disappeared, so that by eleven P.M., those even in whom the most severe symptoms had been manifested were out of danger, and most of the patients lay in a quiet sleep. In all, strong reaction set in, manifested by violent action of the heart and pulse, headache, and burning thirst; but there were no further traces of pain in the intestines or in the limbs. Copious drinking, and the use of the magnesia, were continued. On the following morning, no symptoms remained beyond some headache, lassitude, and pain in the limbs; and nearly all returned to their ordinary occupations.

Dr. Pool gives the reasons which lead him to ascribe the results observed to the chemical action of the hydrated magnesia, and not to the mere evacuation of the contents of the stomach, nor to the magnesia acting as a covering to the intestines. From a consideration of the mode of action of the hydrated magnesia in poisoning by arsenic, he concludes:—1. That this substance, even in a very advanced stage of poisoning (but always before the accession of inflammation or its results), is a sure antidote, having the power of neutralizing and eliminating the arsenic, both where it meets with it in the alimentary canal and in the more

distant parts of the body. 2. That it may be given warm in large quantity and at short intervals, whereby two important indications are at once fulfilled—those of arresting vomiting and promoting alvine evacuations. Dr. Pool considers the hydrated magnesia preferable to the hydrated oxide of iron. M. Van Hulsteyn made a qualitative and quantitative analysis of the remainder of the sauce. The quantity experimented on was four ounces (the poisoned persons had used about three-fourths of the sauce). In the four ounces, about a drachm of pure arsenious acid was found.—(*Nederlandsch Weekblad voor Geneeskundigen; and Nederlandsch Lancet*, July and August, 1855.)

Poisoning by King's Yellow (Impure Sulphide of Arsenic): Epilepsy as a symptom.—Dr. John Crawford records two cases of children—a girl, seven years old, a boy, four—who, at eight o'clock A.M., took by accident king's yellow, which had been mixed with pease meal. The amount taken by the girl was much more than by the boy, but the exact quantity could not be arrived at, though it was possibly as much as from eighty to ninety grains. The children were not seen by a medical man until nearly four hours had elapsed; but meantime emetics, olive oil and a drop of croton oil, were, as it would seem, given to the girl. In spite of the medical treatment adopted, the girl gradually became worse, and at two P.M. she was seized with *an epileptic fit;* the symptoms of irritant poisoning, vomiting, epigastric pain, and tenderness continued; the convulsive fits recurred several times, and in one of them she died at nine P.M., thirteen hours after taking the poison. The boy recovered.

Post-mortem Examination.—The inner coat of the stomach over three-fourths of its extent was highly inflamed, and on its posterior surface there was a patch, irregularly circular in form, and an inch and a half in diameter, where the inflammation had evidently been particularly intense. Over this part there was a layer of newly formed lymph, in which were enveloped a number of shining yellow particles, producing a bright yellow stain, and which, when examined under the microscope, exactly resembled the commercial sulphide of arsenic. The intestines were not inflamed; the other viscera were healthy. Some pultaceous fluid in the stomach, the liver, and the intestine all yielded arsenic by Reinsch's and Marsh's processes. Six drachms of urine exhibited no trace of the poison.

Dr. Crawford remarks that epilepsy occasionally occurs in lingering cases of arsenical poisoning, the fits coming on after the irritant symptoms have subsided. In this case they appeared during the acute stage. The effusion of lymph in the stomach was remarkable, being rarely met with except in cases of irritant poisoning. Beneath the stratum of lymph the villous coat was of a deep violet colour, the rugæ thickened, rounded, and tumid, and even the impress of the reticulated disposition of the lymph was observable, a circumstance to which Dr. Christison has directed attention as being of a striking and decisive character.

Our author also points out with force that the "Act to regulate the sale of arsenic," while it prohibits the retail of arsenic unless mixed with soot or indigo, bears in its last clause, "that in the construction of this act, the word *arsenic* shall include arsenious acid, arsenic acid, the arseniates, and all the *colourless* preparations of arsenic." On the sale of the sulphides, therefore, no restriction is imposed, and the smallest coin may purchase a poisonous dose at any drug or colour shop.—*Glasgow Med. Journal*, April, 1856.

Electro-Chemical Method of Detecting Arsenic.—Professor Edmund Davy has communicated to the Royal Dublin Society the following mode of detecting arsenic in organic solids and fluids. The apparatus is of the simplest kind. It consists of two slips of different metals, generally zinc and platina. The zinc is used in the state of foil or thin sheet, the platina as foil; in some cases a spatula, with or without a spoon at one end of it; and occasionally a small crucible.

The spoon is well adapted for concentrating or boiling, nearly to dryness, fluids which may contain arsenic in extremely minute quantity. The crucible is neces-

sary in cases where it is desirable to convert metallic arsenic attached to its bottom into arsenious acid, and collect it on a surface of glass covering the crucible. The size and thickness of the platina foil may vary; but Professor Davy generally used a foil about two inches in length and two-thirds of an inch wide, and a foil of zinc of one inch or an inch and a half long from one-third to an eighth of an inch wide, and tapering to a point at the end. One slip of platina foil will answer for an indefinite number of experiments; so also will one slip of zinc. It is necessary only to heat the platina foil by the flame of a spirit or candle, if arsenic has been deposited on its surface by previous use, and to wash in water and wipe the zinc, or remove the blackened part by cutting it off.

Muriatic acid or sulphuric have to be used in these researches; they, therefore, must be tested as well as the zinc prior to being brought into play as tests. These are tested as follows:

Muriatic Acid.—One or two drops of the acid being put on the platina foil, keep in contact a point of a slip of zinc with the platina, in about the centre of the acid for one or two minutes; if the acid contain arsenic, a permanent bluish spot will be produced on the platina. The foil being washed and dried, the spot will readily disappear on exposing the foil to the heat of the spirit-lamp. If the zinc contain arsenic, a portion of it will be strongly attached to the platina, and will disappear by heat, producing the characteristic odour of garlic.

Sulphuric Acid.—Two or three drops of the sulphuric acid are to be added to an equal bulk of water; then about six drops of pure muriatic acid are to be added to the dilute sulphuric. Two or three drops of the mixed acids are now to be put on the platina foil, and the zinc applied as before. In this way arsenic may be detected in sulphuric acid when a pint of the acid contains only a grain.

Professor Davy adds many proofs of the delicacy of this test and its mode of application. He mixed five grains of solid arsenious acid in a basin of pea soup. A platina spoonful was boiled nearly to dryness; several drops of muriatic acid were then added, and after mixture, most of the solid matter was dissolved, forming a thickish fluid; the zinc being applied, a whitish coagulum, changing to brown, appeared, and the arsenic soon covered the surface of the spoon.

A fly was killed by a solution of arsenious acid in sugar. It was only necessary, without further preparation, to bruise it, in contact with a few drops of muriatic acid, on a platina surface, and apply a slip of zinc, when the arsenic was readily precipitated on the platina. Arsenious acid was also mixed with butter, lard, oils, bread, paste, starch, syrup, sugar, in powder, wine, vinegar, milk and cream, bile discharged from the stomach, yolk of egg, and other organic substances, and was detected in all these with equal facility. In cases, however, where the proportion of arsenic is very minute, a small interval of time is required to effect the deposition. And there is an advantage in making contact by a point of zinc; for the action seems to be, that one part of the arsenic in solution is precipitated on the platina, being the negative metal; while a very minute part is carried off as arseniuretted hydrogen.

A quantitative analysis may also be made by this process. With a view to gain some approximation as to the actual quantity of arsenic that could be detected by this plan, Professor Davy placed on a new slip of platina, weighing 22·14 grains, five drops of an aqueous solution of arsenious acid, and three drops of muriatic acid: a slip of zinc being applied, the arsenic was soon reduced, and much of it adhered to the platina, which, after being washed with water and dried, was found to have acquired an increase of $\frac{1}{300}$th part of a grain. The foil was heated in a retort, and a delicate white film of arsenious acid rose and condensed in the upper part of the bulb of the retort. The platina was left quite clean, and of the same weight as at first. The quantitative experiment given above affords no idea of the extreme limits to which this microscopic method of detecting arsenic may be carried.

The electro-chemical method of detecting arsenic combines the reduction of arsenious acid to the metallic state, and its subsequent oxidation or reconversion

into arsenious acid. This can be effected where the poison is in the most complex organic mixtures, and in minutest proportions.—*Journal of the Royal Dublin Society*, July, 1856.

Arsenical Poisoning in the Horse.—Mr. Edwin Taylor gives a very interesting history of two horses poisoned by arsenic. The symptoms were great prostration; violent twitching of the muscles all over the frame; constant abdominal pain; animals sometimes lying down and rolling about; purgation violent, about every ten minutes, the dejections being dark-coloured and offensive; the mucous membrane of the eyes and the nose of a bright scarlet colour; the pulse from ninety-four to ninety-six beats per minute.

The post-mortem signs ran as follows, these signs extending to two other horses poisoned also by arsenic and examined after death, but not attended by Mr. Taylor:—The stomach was highly inflamed; the mucous membrane peeled off in places, and forming a coating to the contents. The cæcum and colon were highly inflamed and black in places. The lungs were much congested.

Arsenic was detected in the bodies of these animals, but none in the corn on which they were fed. A charge was brought forward against two servants at Guilford by the owner of the horses, but no evidence of a conclusive kind as to the mode of administration of the arsenic could be brought against any one. In the course of the trial it was elicited that it is no uncommon thing to give arsenic to horses in small doses, for the purpose of improving the coat; a practice on which the judge spoke with proper reproof.—*Veterinarian*, Sept. 1856.

ANTIMONY.

Slow Poisoning by Antimony.—A case of antimonial poisoning at Bolton-le-Moors has attracted much attention. From the depositions placed in our hands, and from the facts adduced at the trial, we infer that the influence of antimony in accelerating the death of the man, McMullen, husband of the prisoner, was proved. The antimony was administered for a long period, at varying intervals, and in doses possibly of from one to five grains, combined with cream of tartar; this combination forming a compound commonly sold in Bolton under the name of "quietness," for women to give to their drunken husbands. The symptoms were at first those of dyspepsia attended with vomiting, but in the last days of the patient they were much more aggravated. There was vomiting, pain in the stomach, prostration, and ultimately typhoid sinking and jaundice. It is worthy of remark that, for the last four days at least, the patient had no antimony, a guard being kept over him. The autopsy disclosed many of the specific signs of antimonial poisoning—viz., injection of the stomach, duodenum, rectum, and inner surface of the bladder; semi-fluidity of the blood and pulmonary congestion. At the same time, there was marked evidence of old-standing organic disease. The lungs were emphysematous. There were pleural adhesions, and a slight effusion into the pericardial cavity. The bowels were loaded with scybala, and had been constipated previous to death. The liver was congested, but the cystic and hepatic ducts were pectalous. The right kidney was congested. The analysis, very carefully made by Mr. Watson, showed antimony to be present in the liver, kidneys, spleen, urine, and in the scybala; but absent in the stomach, intestines, lungs, and heart. The poison was also detected in tea and in some medicine which had been given to the deceased by the prisoner. The liver was the chief *dépôt* of the poison.

There are many points of interest in the symptoms of this case: as the constipation, the seeming absence of diaphoresis, and the jaundiced condition preceding death. An attempt was made by the defence to prove that the case was one of natural gastro-enteritis; but this argument failed, and the prisoner was transported for life on a verdict of manslaughter.

Physiological Deductions regarding Antimony.—A long-continued and careful series of experiments have been made by the writer of the present report, on the

subject of antimonial poisoning. The experiments have now extended over several months, and have formed the subject of two communications to the Medical Society of London. The following are the conclusions arrived at :—

1. That antimony, both as regards the symptoms it induces and the pathological results arising from its administration, excites effects in the dog identical with those which it excites in man; and that experiments on dogs thus afford a fair basis of comparative research. 2. That the skin, peritoneum, cellular tissue, lungs, all absorb antimony in its soluble form with as much certainty as the stomach; and that, whether introduced by any of these channels, or by direct transfusion into the blood through the veins, the diffusion of the poison is equally complete, and its effects specifically the same. (Absolute.) 3. That, after any such mode of introduction, antimony may be detected in the vomited and purged matters, in the stomach and in the contents of the stomach, in the intestines and their contents, and in the lungs, liver, kidneys, blood, urine, heart, and even in serum effused into cavities, if such be present. (Absolute.) 4. That, consequently, the detection of antimony in vomited or purged matters, in the stomach or the contents of the stomach, or in the intestines or in their contents, can no longer be considered as any judicial scientific proof that the poison was introduced into the system by the alimentary canal at any part, as has been assumed. (Absolute.) 5. That antimony, being absorbed with great rapidity wherever introduced, the point of surface at which it is taken into the system may afford slighter indication of the presence of the poison than any other parts of the organism: *ergo*, that the point of introduction can never be proved by mere chemical analysis. (Absolute.) 6. That antimony applied locally, so as to admit of being rapidly absorbed, seems to excite but little amount of local injury, although it exerts marked local effects when brought by the blood to any surface for elimination: *ergo*, that the appearance of intense redness or inflammation in the stomach or other part of the alimentary canal, in supposed cases of death from antimony, is no scientific proof, nor yet indirect evidence, that the poison was received into the system by this canal. (Absolute.) 7. That the symptoms of poisoning by antimony by large doses are, as a general rule, those of vomiting, purging, and rapid collapse; and that the same symptoms, somewhat modified in their course, result from small doses repeated frequently during a prolonged period. 8. That to this rule exceptions occur: to wit, that antimony, when thrown into the system in a large dose, and in such a way as to prevent its digestion, as by direct injection into the veins, may destroy the muscular power so suddenly that the symptoms of vomiting and purging may not present themselves. And, again, that when introduced very slowly, as by application to a small wound, it may also destroy by producing simple exhaustion, without the specific symptoms of purgation or vomiting. 9. That, in all forms of antimonial poisoning, death occurs mainly from failure of the circulation; the respirations being continued after the cessation of the heart's beat. 10. That the pathological appearances incident to antimonial poisoning are—(*a*) general congestion; (*b*) marked fluidity of the blood; (*c*) intense vascularity of the stomach in the course of the greater curvature, and, in some cases, of the rectum and other parts of the canal, but without ulceration; (*d*) a peculiarly pale yellow or occasional dark glairy secretion on the alimentary surface. Lastly, contrary to the statements of Magendie, antimony seems to excite no other pulmonary lesion than simple congestion. 11. That the election of antimony by different parts of the body is as yet an open question; that the liver, however, would appear to be the structure in which it is most collected when the administration is slow and in small doses; and that the elimination of the poison is attempted by all the secreting surfaces. 12. That, in rapid poisoning, the fatal effect seems due to direct chemical change in the blood, and to indirect effect therefrom on the heart; while, in slow poisoning, there is superadded an interference with the assimilative powers, the result of the lesions excited in the stomach and other parts of the alimentary canal.

We have further to remark that, in animals dosed for a few days with antimony,

and then kept for periods of seven, fourteen, and twenty-one days, antimony was found in each case in abundant proportions in the liver, and in smaller proportions in the kidney and heart; and also in the contents of the stomach in cases where the animals were destroyed during digestion of food.

The "tolerance" of antimony seems to us to depend entirely on the free elimination of the poison by the kidney.

LEAD.

Effects of Acetate of Lead on Birds.—From a series of experiments on the effects of acetate of lead on pigeons, Dr. Falck, of Marburg, arrives at the following conclusions:

1. Acetate of lead acts as a poison on pigeons, whether administered for a length of time in small doses, or in large doses when the œsophagus has been ligatured so as to prevent its expulsion. 2. Given in moderately small quantities in the food, it diminishes or destroys the appetite of pigeons. 3. This loss of appetite under the influence of acetate of lead, is by no means a result of inflammation of the *primæ viæ*, but decidedly of dyspepsia produced by the poison. 4. The dyspepsia depends on the precipitation by the acetate of lead of the fermentative principles contained in the gastric fluid. This is in accordance with the experiments of Wasmann, who found that acetate of lead precipitated pepsin, and converted it into an inert compound. 5. While dyspepsia is established under the influence of moderately small doses of acetate of lead, the blood and organs of the animal undergo a gradual decomposition, and are evacuated in the form of excrement and perspiration. 6. As, under the dyspeptic state induced, the proper renovation of the blood and organs cannot take place, the result is, that the organs waste, and are diminished in size. 7. Before pigeons die under the influence of moderately small doses of acetate of lead, they lose a certain proportion of the mass of their body, equal to that which results from deprivation of food. 8. The manifestation of lead toxæmia and cachexia observed in man, do not, with the exception of emaciation, occur in pigeons. In fact, the peculiar colouring of the skin and fætor and discoloration of the mouth are absent. 9. Lead colic, which frequently occurs in man, is not found in pigeons. 10. The disorders of digestion and nutrition produced in pigeons by the introduction of moderate doses of acetate of lead, agree in every respect with those which follow the administration of oxide of lead. 11. Large doses of acetate of lead produce in pigeons vomiting and diarrhœa, and, if the poison partly enters the respiratory passages, cough and difficulty of breathing. 12. When large doses of acetate of lead are prevented by ligature of the œsophagus from being vomited, they produce well marked erosion and inflammation of the *primæ viæ*. 13. The erosion of the œsophagus and *primæ viæ* through large doses of sugar of lead, is the result of a chemical action of the poison on the tissues of these organs, and takes place in dead as well as in living pigeons. 14. If large doses of acetate of lead are frequently administered without preventing the evacuation of the poison by vomiting, the process takes place, and is accompanied, in place of inflammation, by a disordered condition, which ultimately causes death by dyspepsia and wasting.—*Deutsche Klinik*, July 26, 1856.

SULPHURIC ACID.

Poisoning by Sulphuric Acid.—Case I.—M. Benzi relates the case of a man, aged fifty-four, who, on July 23rd, 1855, took upwards of three drachms of commercial sulphuric acid, for the purpose of suicide. The symptoms were remarkable. The muscles of the face were convulsed, the eyes sunken, the countenance fixed; *the muscles of the upper extremities and of the back were in a state of clonic spasm;* he had obstinate vomiting, returning every two or three minutes; he was speechless, but the mind was clear. The mouth was half open; the lower lip was swollen; saliva abundant; the tongue was tumefied, pale, and hard, with black spots at the edges; the whole mucous membrane of the mouth was swollen; he had a burning

sensation in the mouth, pharynx, œsophagus, and stomach. The intestines seemed to be unhurt. The pulse was almost imperceptible; the body was covered with a cold sweat.

Treatment.—Two drachms of carbonate of magnesia were given in about nine ounces of water, but of this the patient only swallowed one-third, and with a great effort. In a few minutes the vomiting returned. At a later period, about three ounces of the antidote were taken. The vomiting returned and lasted four or five minutes, and went off till the following morning. The pulse was larger; he had pain in the alimentary canal, acute pain in the stomach, and constriction of the pharynx. Ice applied externally could not be borne. The salivation was more copious. On the following day (24th), he had nausea with attempts to vomit, and had passed a restless night; he had fever and difficult deglutition. Ice and sugar-water were given, and he was bled twice. On July 26th, he was better; emollients and the ice were continued, and again he was bled. On July 28th, speech and deglutition were easier. An ounce of sulphate of magnesia produced copious stools. July 30th, the epithelium of the mouth was detached; almond emulsion with borax were ordered as a gargle. On the 5th of August, he was able to take solid food, and left hospital well on the fifteenth day.—*Gazetta Medica Italiana, Stati Sardi, et Gazette Médicale de Paris*, March 22, 1856.

Case II.—Dr. Popham records the case of Alfred Winstanley, a soldier, a man of great strength, and about thirty-six years of age, who was admitted into the North Infirmary, Cork, on the morning of June 18th, 1851, with symptoms of corrosive poisoning by some mineral acid. Part of his lips, and the interior of the mouth and pharynx, were stained of a greyish or slightly brownish colour; at the corners, the lips retained their natural appearance. On his soldier's jacket, brown stains existed. He suffered most excruciating pain, moaning loudly, breathing with difficulty, and keeping both hands clasped on the epigastric region, to which he referred all his distress. His entreaties for relief were heartrending; still he was greatly averse to swallow any medicine, from the agony to which it gave rise.

The poison from which the symptoms arose was sulphuric acid, of which the patient had swallowed half-a-pint, with a suicidal intent, owing to a love disappointment. He drank off the contents of the cup, and instantly sprang upwards from the ground, screaming violently. Death ensued about twenty hours after taking the poison.

The main features of the autopsy, which was held twelve hours after death, consisted in intense cadaveric rigidity, engorgement of all the veins with black, tarry blood, congestion of the lungs with dark blood, discoloration of the œsophagus, entire disorganization of the gastric mucous membrane, inflammation of the duodenal mucous membrane, a comparatively healthy state of the remaining small intestines, extensive disorganization of the mucous membrane of the large intestines, and deep injection of the brain, so as to resemble capillary apoplexy.

Dr. Popham, in remarking on this case, which may be regarded as a model of a forensic report, dwells on the protective influence of the epiglottis in preventing the acid from entering the windpipe; on the effects of the poison on the muscles; the sudden convulsive spring after taking the poison; the excessive rigidity of the muscles, as connected with the fluidity of the blood, which persisted long after death; the total loss of coagulation of the blood; the venous colour of the arterial blood, and the unusual darkness of the venous blood; the unclouded state of the intellect with such a state of blood and such congestion of the brain; and the escape of the small intestines, as compared with the duodenum above and the colon and large intestines below.—*Dublin Quarterly Journal of Medical Science*, May, 1856.

Case III.—Dr. John Crawford reports a case of this nature. A young woman

took by accident a mouthful of the acid, mistaking it for vinegar. She instantly spat out the liquid, declaring she was burned. Water was poured down her throat, olive-oil was applied to the lips and external parts, and magnesia was administered as an antidote. The patient, however, continued to get worse, and next morning, about twenty-two hours after the fatal mistake, she died.

The post-mortem examination was made forty-eight hours after death. Externally, a brown streak or stain, exactly of the colour which sulphuric acid produces on the skin, ran downwards from each of the angles of the mouth to the chin, and over the right breast was a patch of the same colour, the skin thus stained having a charred and hardened look. The lips and gums were swollen and soft, and had evidently been affected with violent inflammation, which in spots had made considerable progress towards gangrene. The lining of the mouth was corroded, softened, and of a greyish colour; the surface of the tongue was corroded, softened, and whitish; the pharynx, especially at its upper part, presented nearly the same appearance; but there was no trace of corrosion or inflammation, either in the œsophagus or stomach. Considerable inflammation, but no distinct œdema, surrounded the glottis; the lining of the larynx and trachea was highly injected, that of the bronchial tubes more so, while both lungs presented throughout the well-known appearances of the first stage of pneumonia.

Dr. Crawford observes that toxicologically the main interest of this case consists in the effects of the poison being confined exclusively to the mouth, pharynx, and expiratory passages. Death was caused by the acute laryngitis, bronchitis, and pneumonia. It is very doubtful if a single drop of the acid reached the stomach. Ryland, in his work On the Diseases and Injuries of the Larynx and Trachea, states as a singular fact, that the larynx suffers injury from the swallowing of any of the strong acids only when they are taken accidentally in mistake for some other liquid. In cases of suicide, the larynx is never injured—the epiglottis, in the act of swallowing, completely covers the upper surface of the glottis, and the corrosive acid passes down the œsophagus to the stomach without impairing in any way the organization of the larynx. But if the acid is taken accidentally, immediately that it reaches the gullet, the mistake is discovered, violent action of the pharynx is excited, and the corrosive liquid is rejected through the mouth and nostrils. In this violent and spasmodic effort, the epiglottis is pushed up, and some few drops are readily forced into the glottis. Porter, another writer on the larynx, expresses the same view. In this case, possibly, the inflammation passed downwards from continuity.—*Glasgow Medical Journal*, April, 1856.

The three cases here given are, in their collective sense, most instructive. In the first two cases the patients swallowed the poison intentionally; in them the glottis escaped: in the last case the poison was taken by accident, and the glottis and air passages were the parts mainly affected. Thus Ryland's and Porter's views, above narrated, are strikingly corroborated. In the first case, the quantity of acid swallowed was not less than in the last, yet, owing to the glottis remaining uninjured, recovery took place; the treatment being mainly the same in both cases. In the second case, a half-pint of the poison was swallowed, and yet life was prolonged to within only two hours less time than in the last case, where but a few drachms were taken into the mouth, to be instantly spat out again. It is interesting to compare with Dr. Popham's case a suspected case of poisoning by sulphuric acid, which we gave in our last Report from 'Henke's Zeitschrift.'* In the case there referred to, the brain is also spoken of as being preternaturally *hard*, with effusion of serum both in the ventricles and between the cerebral membranes. The case, it is true, is doubtful, as a long interval elapsed between the supposed time of poisoning and the death. The condensed state of the brain may, neither in this case nor in Dr. Popham's, bear relation to the effects of sulphuric acid as a poison; but the coincidence is worth remembrance.

* See British and Foreign Medico-Chirurgical Review for April, 1856, p. 519.

STRYCHNINE.

Poisoning by Strychnine.—CASE I.—Drs. Lawrie and Cowan record the following case of strychnine poisoning. A medical man, aged twenty-two, while labouring under great excitement, the result of a debauch, took three grains of strychnia in the bedroom of his hotel, concealed the empty bottle behind the grate, undressed, and went to bed. He slept, according to his own account, but not very soundly, for an hour and a half, his rest being interrupted by dreams, some of which were of a delightful description. He awoke in a spasm, uttering loud cries which alarmed the household. On the cessation of the spasm he fainted; and on coming to himself, requested the servant to go for Dr. Montgomery. On that gentleman's arrival, suspecting that poison had been taken, he dissolved some sulphate of zinc in water; but on commencing to administer it, the first drop that touched the patient's tongue induced a violent spasm, accompanied with loud shrieks, and complete opisthotonos. On the subsidence of the spasm, by introducing his finger to the back of the mouth, and carrying the spout of the drinking cup over it, Dr. Montgomery was enabled to get the emetic partially swallowed. He repeated the dose three or four times. Free vomiting having been induced, the inhalation of chloroform was immediately commenced. At 4 A.M. Dr. Lawrie saw the patient, and, in addition to the continuance of the chloroform, administered a stimulant enema. Between the hours of 4 A.M. and 6·30 A.M. nine spasmodic attacks, more or less severe, occurred. The last of these, which seemed to be induced by the application of a cup to the lips, was very intense and prolonged. The patient started suddenly up in bed, his whole frame being in a state of complete rigidity. The respiration, at first impeded, became suspended; and it was only by the long continuance of artificial respiration that it was restored. The limbs were rigid, and the fingers clenched. The pupils were dilated. During the spasms evident relief was afforded by forcible extension of the body. In the intervals there were constant twitches of the extremities. The skin was warm and moist. The pulse was at first extremely rapid, but gradually diminished in frequency. The urine was passed with difficulty. The mind was perfectly collected. From half-past 6 till 2 P.M. the patient was kept by Dr. Cowan almost continuously under the influence of chloroform. The twitches remained till the following day, and the patient then rapidly recovered. It should be remembered that the emetics acted well, and that some of the undigested dinner of the previous day was found among the rejected matters. The patient had taken a hearty meal at 4 P.M., before swallowing the poison.—*Glasgow Medical Journal,* July, 1856.

CASE II.—Dr. Stevens, at the Glasgow Medico-Chirurgical Society, related the following case. The man was a patient in the Glasgow Royal Infirmary, under the care of Dr. Weir. The symptoms of strychnine poisoning commenced on April 1st, 1848. The patient was a paralytic, and at the time that the alarming symptoms appeared was taking the eighth of a grain of strychnine in solution twice a day. About twenty minutes after taking not more than his usual dose, he began to have frequent startings, and slight pain in the lumbar spine, shooting thence down the legs. These did not amount to much more than violent twitchings, but he was much alarmed, and sweated profusely. In this condition Drs. Steele and Stevens saw him. On rising up that the spine might be examined, he was seized with a violent tetanic convulsion, in which there was complete opisthotonos, and great difficulty in breathing, as well as severe trismus. The fit continued about four minutes. During it he was quite aware of his condition, and in much pain, but could not speak. Chloroform was now administered. The pupils, which were before dilated, now became more so, and in a few minutes he was insensible. The spasms in the meanwhile became less and less severe, and the inhalation being shortly discontinued, he awoke free from pain. He could not immediately speak, but after stammering a little, he complained of weariness, and

spoke of the fit as having occurred the night before. After this the twitchings continued for an hour, but less frequent, and ultimately he quite recovered.—*Ibid.*

Case III.—Dr. Kirk relates that on May 10th, 1849, he visited a young man who was supposed to be unwell. He found him in bed, but could discover no signs of disease. Dr. Kirk had barely returned home when he was recalled, a paper having been found in which the patient confessed to having taken poison, but did not say what poison. Sulphate of zinc was given in warm water. The symptoms of poisoning now began to show themselves, and the first noticed was spasm of the neck. The patient could not be raised, and he had to be fed with the emetic in spoonfuls. Next his teeth were so firmly clenched that two men could not separate his jaws; then his knee-joints became as rigid as bars of iron; and lastly, his body was raised off the bed, and rested on the head and the heels. The pupils were widely dilated; he perspired freely, and complained of pain at the pit of the stomach. The spasms were slight at first, but soon became terribly severe, but alternated with intervals of repose. In these intervals the stomach-pump was used, and his stomach speedily washed out. The only other treatment used was friction over the spine. The patient recovered perfectly, and was well next day. The patient appears to have taken six grains of strychnine, which he had from a chemist for the presumed purpose of destroying a dog.—*Ibid.*

Case IV.—Dr. Bruce related the case of a druggist's apprentice, who, on August 12th, 1854, took from one to two ounces of laudanum for the purpose of suicide. He vomited it immediately, and not long after travelled about forty miles. On the morning of the 13th he again attempted suicide by taking about four grains of strychnine in the solid form. Shortly afterwards he confessed having taken the poison, and in about fifteen minutes Mr. Anderson, surgeon, found him suffering from a tetanic spasm, which lasted but a short time. An attempt to give emetics produced a most severe tetanic spasm. The jaw was firmly fixed, the extremities stiff, and the body bent back in a state of complete opisthotonos. Upon cessation of the spasm the stomach-pump was used. The introduction of the tube gave rise to a severe spasm, with marked trismus; upon its subsidence the stomach was thoroughly washed out with tepid water, after which he had no recurrence of symptoms. He was removed to bed, had a purgative, which operated well, and, with the exception of slight weakness, was well in two days.—*Ibid.*

Case V.—Mr. Ryland, of Birmingham, relates the following case. On April 18th, 1831, a stout man, aged forty-six, died in the hospital, five hours after taking a grain and a half of strychnine. He was under treatment for paralysis of the left side, of some standing. About six or seven weeks before his death, he had had a stroke. During a short residence in the hospital, the man had been treated by strychnine—half a grain at first, a grain at one dose each day for a week, and the morning of his death the dose had been increased to a grain and a half. Three hours after taking it, convulsions supervened, which affected both upper and lower extremities, and were much stronger in the sound side than in the paralysed one. After death, while the muscles of the left or paralysed side were in the ordinary state of post-mortem contraction, those of the right arm were, while the body was still warm, and afterwards, excessively rigid, and the fingers of that side were clenched and immovable. Towards the last, the man became comatose; and immediately before death took place, the body became rigid.—*Association Medical Journal*, June 14, 1856.

Cases VI. and VII.—*Treatment with Sweet Oil.*—Dr. Gustavus Shaw gives the history of two cases. On March 7th, 1852, at 6 P.M., a negro woman, to whom he was called, was in convulsions, with slight rigidity. She was very sensible to external impressions, the contact of a cup to her lip being sufficient to produce a severe paroxysm, lasting three or four minutes; the paroxysms recurred every five minutes, when she was left perfectly quiet. The pulse was unchanged; inspiration deep; anxiety; heat of the stomach, and a choking sensation in the throat.

Half a bottle of sweet oil was given, which was vomited; it was repeated immediately; this dose was retained five minutes, and was then vomited; it was repeated again, and in fifteen minutes again vomited. By this time the woman could speak without bringing on a paroxysm, and said, that about four o'clock she had found a piece of dried beef in the cabin, which she tasted of, and finding it extremely bitter, gave it to her children. On going to the crib where the children lay, Dr. Shaw found one of them, aged two years, in the same state as the mother. He gave the oil as to the mother, with the same success; and about twelve o'clock left them, out of danger. The woman went to work the next day. Some of the oil vomited by the woman was licked up by three dogs. These were seized with symptoms of strychnine poisoning, and died in a few hours. The facts of the case were these. Mr. Hannay, the master of the slave, had baits for wolves, and this dried beef was one of them, and contained not less than ten or fifteen grains of strychnine. It must have been in the stomach one hour before she was known to be sick. Mr. Hannay then gave her a dose of castor oil, which prevented the absorption of the poison. The woman took in all three and a half ounce bottles of olive oil, the child nearly one bottle.—*American Journal of the Medical Sciences*, April, 1856.

Dr. Shaw's history acts as a two-edged sword, in reference to oil as an antidote for strychnine. If the oil saved the negro woman, why did the dogs die that licked it up? It is clear the woman vomited the poison, and it seems that the oil excited the emetic action; whether olive oil is the safest emetic in such cases is an open question.

The symptoms in this woman's case are remarkable from their resemblance to hysteria.

Case VIII.—Mr. G. M. Jones, of Jersey, relates the case of Jane D., an under-nurse in the Jersey Hospital, twenty-two years of age. At half-past nine on the evening of August 24th, feeling nausea and sickness, she took four pills, which she obtained from one of the inmates, supposing them to be "bilious pills." In twenty minutes afterwards, being in bed, she was heard to moan piteously, and almost simultaneously uttered a loud shriek. The paroxysmal attacks recurring with much severity, Mr. Jones was sent for, and saw her at half-past eleven P.M. At that time there had been no spasm for some time, nor was there one while Mr. Jones was there, which was more than half an hour. The girl's appearance, voice, and features were as usual. The pulse was more rapid, and the temperature of the body higher, than usual. Mr. Jones ordered a dose of castor oil, and an anti-spasmodic draught, to be taken if the spasms returned. Shortly after he left, the convulsions returned, and continued with slight intermissions until her death, which occurred at half-past two, five hours after taking the strychnine. She retained her faculties to the last, and appeared to have had frightful rigidity of the limbs and opisthotonos, accompanied by a sensation of choking.

On *post mortem* examination, fourteen hours after death, the whole surface of the body, especially the arms and neck, was of a deep livid colour. The anterior parts of the abdomen and legs were less discoloured. The general appearance of the face was that of a person who had been strangled or suffocated. The neck, chest, and upper arms were œdematous. The abdomen was very tense and tympanitic. The rigor mortis had subsided in the upper extremities, but still existed in the lower. The spine was arched. The soles of the feet were flat. The fingers and thumbs of both hands were firmly clenched, except the index fingers, which were extended. The arms were extended along each side. The teeth were firmly set, the jaws rigid, the tongue partly protruded, the gums bloodless, the lips puffy and livid. Much bloody froth issued from both nostrils. Both corneæ were opaque and dull; the pupils were semi-dilated; the conjunctivæ injected. Much blood escaped on cutting through the scalp. The vessels and sinuses of the dura mater were much congested. There was some opacity of the arachnoid and pia mater, and some thick yellowish serum beneath the former. The brain was much softened: the vascular points were numerous. The ventricles contained rather

more serum than usual. The spinal canal was filled with bloody serum: the vessels of the cord were much injected. The spinal membranes were very vascular: the medulla oblongata and cord were soft, but less so than the brain. In the dorsal region of the cord, a clot of semi-fluid blood, an inch and a half in extent, was found under the membranes posteriorly. The convulsions had occurred principally on the right side; but no pathological appearance could be discovered to account for this. The stomach, which contained some half-digested fluid, presented no appearance of disease; the duodenum and jejunum were congested. The liver was large, congested, and in an advanced stage of cirrhosis. The kidneys were congested. The heart was large, flabby, and collapsed. The large blood-vessels and auricles contained a little very dark fluid blood. Each ventricle contained a dark semi-fluid coagulum, weighing about a drachm and a half. The lungs were voluminous and much congested. The blood generally, throughout the body, was fluid, and very dark.

Strychnine was distinctly discovered in the stomach by the tests recommended by Messrs. Rodgers and Girdwood, and Dr. Letheby. On inquiry, Mr. Jones was able to discover the jar in the surgery whence the pills which the girl took had been procured. Two of them, tested by the chloroform process, yielded nearly one quarter of a grain of almost pure strychnine. Rabbits and frogs, also, but especially young toads, when experimented on with the materials composing the pills, distinctly manifested the symptoms of strychnine tetanus.—*Lancet*, September 13, 1856.

Case IX.—The final symptoms in the case of Mrs. Dove, who was poisoned by her husband, may be described in a few words. They were but the repetition of previous paroxysms which had been produced by the frequent administration of the poison. On Friday, February 29, while Mr. Dove was present, she was seized with a violent attack; her breathing was much impeded, and the whole body became very rigid, and was observed to often twitch involuntarily. In the course of a few hours she improved, and continued better during the whole of the night and the next day. In the evening of March 1, when Dove gave his wife some medicine, he poured something into a wineglass, and gave to his wife: she complained of its being hot and bitter. A few minutes afterwards she was again seized with violent spasms, during which she shrieked out, her eyes became fixed, she clenched the hands of her attendants, and her whole body became perfectly rigid and arched, in which state she remained until her death, at twenty minutes before 11 P.M.

Two other interesting cases of poisoning by strychnine and nux vomica will be found in the 'Lancet' for May 17, 1856.

Irregularities in the Action of Strychnine.—Dr. John Roulston, of Harrogate, states that a friend of his, residing in South Africa, wishing to destroy a large cur dog, aged about five months, gave it three grains of strychnine between two pieces of beef, which the animal bolted without mastication. He was then going on horseback into the country, and the dog followed for two miles, leaping and gambolling the whole way, and again home. After his return, the dog, which until that moment had not shown the slightest symptom of illness, and was playing with its mother at the time, suddenly, and without any spasm or tremor, leaped up in the air with a faint cry, and fell dead on the spot. The time which elapsed between the administration of the poison and death, would be at least an hour and a half. This case shows how readily extraneous circumstances may interfere with the known action of this or any other drug. In this case, exercise had no doubt completely arrested the digestive process for the time.

It is a very common plan with the farmers of South Africa to use strychnine in order to get rid of jackals, hyænas, &c., which infest their folds, and which, when dead, become a fatal meal to their successors.—*Association Medical Journal*, June 21, 1856.

Tests for Strychnia.—We have not the space to do more than give the following references on this point:—

Dr. Letheby's mode of testing will be found in the 'Lancet' for June 28 and July 12, 1856.

Messrs. Rodgers' and Girdwood's mode will be found in the 'Lancet' for June 28, 1856.

Mr. Horsley's methods of detecting strychnia and brucia will be found in the 'Association Journal' for August 30, 1856.

Mr. Copney has an excellent paper On the Chemical Tests for Strychnia, in the 'Pharmaceutical Journal' for July 1, 1856.

Physiological Action of Strychnine.—Two valuable papers on this point will be found in the 'Lancet' for June 7 and 14, 1856, by Dr. Harley.

On the questions of the detection of strychnine in the urine, the effects of alcohol and of chloroform in suspending the action of this poison, and on the condition of the heart in such cases, the reader will find some admirable comments by Dr. J. A. Easton, in the 'Glasgow Medical Journal,' July, 1856; and also by Drs. Lawrie, Ritchie, A. and G. Buchanan, and Drummond.

TURPENTINE.

Poisonous Properties of Turpentine Vapour.—Dr. L. C. Roche, referring to the researches of M. Marchal de Calvi, on poisoning by turpentine vapour,* relates a singular fact which was communicated to him forty years ago by M. Thénard. A druggist had placed in a cellar, which was rarely entered, a ton of essence of turpentine. On entering one day, he was seized with a sense of suffocation, and had to retire precipitately to avoid being asphyxiated. M. Thénard being requested to examine the air of the cellar, filled a bladder with it, and found that it contained little more than nitrogen, nearly all the oxygen having disappeared. The air in the cellar was renewed; and when an entrance could be effected, the liquid was found changed into a substance of the consistence of turpentine. M. Thénard was of opinion, as far as Dr. Roche remembers, that the essence of turpentine had rapidly absorbed the oxygen of the atmosphere. If this were so, the action of turpentine would be asphyxiating, by depriving the air of the vital element; and Dr. Roche suggests that it acts in this way rather than by the vaporisation of the essential oil.—*L'Union Médicale*, March 22, 1856.

II. Miscellaneous.

On the Nature of Rabies: its Nosological Position as a Febrile Disease.—Dr. Faber, of Schorndorf, discusses pretty fully the relations and differences between tetanus and rabies. In the course of his essay, he first contrasts these diseases, and then states his reason for assigning to rabies a place among febrile diseases.

The following are the points of differential diagnosis between tetanus and rabies:

1. In *tetanus*, the disease, as a rule, soon follows the receipt of the injury. The early supervention of *rabies* is an exception; it usually takes place at the end of months, or even years. 2. In *tetanus*, the premonitory symptoms are, painful feelings of contraction, tension of the region of the loins and back of the neck; and frequently a feeling of dulness and pain in the wound. In *rabies*, a peculiar painful sensation is felt in the cicatrised wound, extending in the course of the nerves to the back, larynx, and chest. If the wound is not yet healed, the pus becomes sanious; if cicatrisation has taken place, the scar assumes a livid colour, and becomes tumefied. 3. In *tetanus*, the motor nerves are chiefly affected, and the convulsions are of the tonic kind. All voluntary motion is greatly impaired

* See British and Foreign Medico-Chirurgical Review, April, 1856, p. 519.

or entirely destroyed. Trismus is so violent, that the lower jaw cannot be moved either by the patient himself or by others, and the mouth cannot be opened. The contraction of the muscles is permanent; even during the intervals the tonic spasm of individual portions of the muscles continues in an extraordinary degree, even causing rupture. Convulsions of the limbs occur in paroxysms; and when these have subsided, tonic contraction sets in, and the body becomes as stiff as a piece of wood. The contraction principally occurs in the muscles of the trunk, producing opisthotonos and emprosthotonos. In *rabies*, the nerves of sensation are chiefly affected. The contraction is clonic. Trismus, emprosthotonos, and opisthotonos are entirely absent. The voluntary muscles retain the power of motion; but men and animals cannot move without springing, raving, thrusting, or striking; men cannot speak; dogs, wolves, foxes, horses, &c., cannot bite; horned cattle cannot bellow. The spasm is principally in the cardiac region, hence the death anguish, which manifests itself in a very different manner in men and in animals. Paroxysmal convulsions also sometimes occur, but, according to Dr. Faber, there is no tonic contraction; the patient can move himself how and whither he will. 4. In the commencement of *tetanus*, deglutition is impeded by spasm of the pharyngeal muscles; at a later period, because the patient is prevented by trismus from opening his mouth. In *rabies*, deglutition is impeded through the whole course of the disease by spasm of the muscles by which this process is effected; trismus being absent, the mouth can be opened. In the tranquil form of the disease, the lower jaw hangs down paralysed, but can be moved by the finger (Hertwig). 5. In *tetanus*, the psychical disorder is inconsiderable. In *rabies*, the psychical affection reaches a high degree; the patient is nearly in a state of desperation. Here there is an analogy with mental disease. 6. In *tetanus*, the dread of water and the ejection of saliva are absent. In *rabies*, dread of water is very frequent, on account of the impediment to deglutition; and ejection of saliva is generally present. 7. In *tetanus*, dread of light, of glare, and of air are not constant symptoms. They are constant in *rabies*. 8. In *tetanus*, there is no delirium. In *rabies*, delirium and hallucinations are frequent in men; and the latter in animals, especially dogs. 9. In *tetanus*, the voice is frequently changed. In *rabies*, the voice is changed; hoarseness being constant. 10. In *tetanus*, there is no inordinate condition of the genital organs. In *rabies*, this symptom is very frequent. 11. There are no distinct and separate forms of *tetanus*. Of *rabies*, whether spontaneous or arising from inoculation, there are two distinct and easily distinguishable forms, which may be easily recognised in dogs—the raving and the tranquil rabies (*die rasende und die Stillende Wuth.*) 12. The course of *tetanus* is not divisible into stages. *Rabies* has three often very distinct stages—depression, irritation, and paralysis. 13. *Tetanus* may end fatally in three or four days; it may continue a fortnight; and other diseases, as fever, apoplexy, paralysis, &c., may supervene. The duration of *rabies* is always for a few days only, generally three or four; it may end in death or in recovery. Transitions into other diseases have not been observed. Paralysis is the precursor of death. 14. In *tetanus*, death generally occurs suddenly during the most violent tonic convulsion: rigidity continues some time after death. In *rabies*, death is always preceded by more or less paralysis of the lower limbs; convulsion ceases; and there is no rigidity after death. 15. *Tetanus* occurs only after greater or less injuries accompanied by contusion, as after gun-shot wounds, but especially after punctured wounds and bites of fibrous and ligamentous structures, especially of the extremities of the limbs; also after surgical operations, and after the bites of dogs and other animals which are not rabid. *Rabies* supervenes in men and animals on great and small, even on very small, injuries of any part of the body, only after the bite of a rabid animal. It developes itself in a certain family and order of animals. 16. *Tetanus* usually appears during the suppurative stage of wounds, rarely after their cicatrisation. *Rabies* generally appears after cicatrisation, rarely during suppuration. 17. *Tetanus* cannot be communicated to other individuals by inoculation. *Rabies*, as numerous experiments have shown, can be

communicated by inoculation. 18. There is a peculiar predisposition to *tetanus*, consisting in asthenia, as in drunkards, gluttons, sensualists, soldiers in the field who have undergone fatigue or privation of any kind, &c. For *rabies*, there is no further disposition than to any other contagion. Any person who is bitten by a rabid animal is never secure from rabies. 19. *Tetanus* arises very readily from colds and repressed respiration; and therefore occurs frequently in damp climates with cold nights, within the tropics, and in autumn and winter in Germany. It also arises from violent impressions on the mind. Dupuytren observed it to occur during the revolution of July. The outbreak of *rabies* after the bite of a mad animal is altogether independent of climate, season, &c., and sometimes occurs without any cause, sometimes after depressing mental impressions—for instance, the simple remembrance of having been bitten. It also is produced by over-heating, by drink, and exercise, such as immoderate dancing; and follows the contusion of the cicatrix. 20. Recovery is frequent in *tetanus:* very rare in *rabies*. 21. *Tetanus* can be prevented by a simple treatment of the wound, while it is promoted by stimulant applications: of this an instance is known to Dr. Faber. *Rabies* is averted by stimulant treatment, and favoured by mild or sedative applications. On the appearance of the first symptoms, the disease can be cut short by rapidly and energetically re-inducing suppuration in the wound. 22. In *tetanus*, antispasmodics, and especially opium, are approved remedies. In *rabies*, opium is useless, not to say injurious.

Dr. Faber hence draws the conclusions—1. That tetanus and rabies present some points of resemblance, but, when carefully examined, are found to be very different diseases. The diseases to which rabies bears the greatest analogy are erysipelatous inflammation of the pharynx and œsophagus, and certain psychical disorders. 2. That it is not every kind of injury that can induce rabies. 3. That in other diseases, not following the bite of an animal, hydrophobia may appear; this hydrophobia not being a disease, but a symptom. 4. That rabies may be communicated by contagion from one individual to another.

Dr. Faber expresses his opinion that rabies is a fever, presenting two forms—the raving and the tranquil. Originally developed in animals, it produces in them a contagious principle, which may be communicated to other animals and to man, and from these to others, by a bite or by intentional inoculation. The disease in all is the same, but the symptoms vary according to the species and the individual.—Henke's *Zeitschrift für die Staatsarzneikunde*, xxxv. Jahrgang, zweites Heft, 1855.

Dr. T. Lindley Kemp also published last year his opinions on rabies, several of which agree with those expressed by Dr. Faber. He observes that rabies is essentially an epizootic disease, and that it so prevailed in Scotland in 1835 and 1849. He further says that rabies, like all epidemic diseases, is of an acute character; and, as was pointed out by Youatt, and has been long known, it is always characterized by inflammation of the mucous membrane of the fauces, often extending to the windpipe and stomach. It appears, indeed, in the milder form, to be nearly identical with influenza. Subsequently, at least in the severe cases which attract notice, there is violent delirium, which rarely or never accompanies influenza in men.

The occurrence of delirium in animals, Dr. Kemp believes to be explained by the thinness of the ethmoid bone, and the extent and complexity of the nervous matter (especially in the dog) between the mucous membrane of the fauces and the brain. In every fatal case of rabies examined in the Edinburgh Veterinary College, whether in dogs, horses, or cattle, the brain behind the ethmoid bone was found with every mark of severe inflammation. Two morbid appearances, and two only, are constant in all cases of rabies; inflammation of the mucous membrane near the termination of the olfactory nerves, and of that portion of the brain near where these nerves leave that organ. If, then, the delirium be accidental, and the saliva be unchanged and innocuous, there is an end to the belief that the bite of a rabid dog can produce hydrophobia or any specific disease.

Hydrophobia is very rare during an epidemic of rabies. Many animals and persons must be bitten; but few are affected. Dr. Kemp appears of opinion that the symptoms which occasionally come on some time after the bite of a rabid animal, are in some way analogous to traumatic tetanus; yet that there is a difference, the disease being modified by the impression made on the patient's mind, by the nature of the accident, and by his having, in the interval, anxiously read books about hydrophobia, consulted with his friends about it, and brooded over his reflections until his mental powers have become decidedly affected. Dr. Kemp would rather classify hydrophobia with hysteria, catalepsy, and diseases of that class, which occur in those only who possess *mobility* of the nervous system.—*Edinburgh Medical and Surgical Journal*, January, 1855; and *Association Medical Journal*, June 1, 1855.

Hydrophobia from the Bite of a Cat.—Madame Sophia D., aged forty-seven, of strong constitution, was bitten on December 31st, 1855, in the ankle of the left foot, by a cat which was supposed to be mad, as it had already attempted to bite several persons. Instead of having the wound cauterized, Madame D. consulted some quacks, who, after having employed charms, assured her of a perfect cure. On February 10th, 1856, forty-two days after the injury, Dr. Sabatier was called to see the patient at her residence in Bédarieux. She felt pricking pains in the cicatrix; these ceased in a few hours, giving place to dull pains, which extended over both legs. These pains were confined to the lower limbs during February 10th and 11th, but on the evening of the last-named day, the patient began to feel darting pains in the interior of the genital organs. These were not continuous, but left some intermissions. When they recurred, they appeared to violently excite the whole nervous system. Supposing that a prurigo of the labia, to which the patient was subject, had reached the interior of the vagina, Dr. Sabatier prescribed a lead lotion and two hip-baths. The baths were borne ill, but in the evening, the darting pains had given place to disorder of the stomach, denoted by frequent vomiting. Believing that the vomiting was sympathetic with the disease of the genital organs, Dr. Sabatier again ordered baths, and prescribed antispasmodics and opiates. On the following morning, after a restless night, unmistakeable symptoms of hydrophobia were manifested in the form of spasm of the glottis, dread of fluids, impossibility of swallowing, furor, pallidity of the face, convulsive shocks, and foam at the mouth. The patient insulted all who were near her—even her husband. The pulse became quick and irregular. During the night, the symptoms arrived at their height; she escaped from the persons who had charge of her, and dashed her head against the floor of her room. Her face covered with bruises, and her hair disordered, she resembled a real fury. In the midst of the convulsions, the intellect remained perfect: she sent for her husband and son, and gave them advice. In a short time, copious vomiting of black, thick blood set in. The state of excitement continued until eleven o'clock on February 14th, when it was succeeded by a comatose state, with symptoms of asphyxia. The patient died calmly at one P.M. on that day. The treatment, from the time of the manifestation of the hydrophobic symptoms, consisted in the administration of a mixture containing opium, and in some cold affusions to the head. No *post mortem* examination was made. On examining the mouth, Dr. Sabatier found the tonsils and velum palati very red. He could not discover the vesicles described by Marochetti as being present at the sides of the frænum linguæ in cases of hydrophobia.

M. Sabatier adds that the disease had in all probability been spontaneously developed in the cat. The relatives of Madame D. informed him that the animal, after having received a burn, appeared low, and refused food for several days, and gradually became quite mad. A brother-in-law of the patient was bitten three days before her, but when Dr. Sabatier wrote, had presented no symptoms of hydrophobia. He could not, however, be absolutely pronounced free from danger. The uterine symptoms, resembling those of *furor uterinus*, have been described in a case of hydrophobia by Portal.

In speaking of the questions of early cauterization in cases of bite from rabid animals, Dr. Sabatier asks which is the best caustic? Is the actual cautery, strong acid, or the potential caustic? He suggests that a number of animals should be allowed to be bitten by mad dogs, and that then sulphuric acid should be tried on some, red-hot iron on others, Vienna paste on others, at intervals of from two to fifteen days after the bite.—*L'Union Médicale*, March 4th, 1856.

Hypospadias; Fecundation.—The following case is given by Dr. Traxel, of Kremsier:—On April 1, 1856, a new-born infant was brought to Dr. Traxel, that he might determine its sex. The father and mother were servants of a peasant. On an examination of the alleged father, he was found to have all the external characters of a male; the urethra, which was rather shorter than ordinary, but of large size, was imperforate; the scrotum was divided into two pouches, each containing a testicle. The opposed surfaces of the scrotal pouches were covered with a red skin, and the divisions extended through their entire length. At the root of the penis, in the anterior angle of these pouches, was an opening of the size of a lentil. This was the orifice of the urethra. The lower surface of the penis was grooved from the above-mentioned orifice to the end of the glans. There was no prepuce. Almost a line behind the corona glandis, and in the groove, were two elliptical openings, which readily admitted a large hog's bristle; there was a third smaller opening two lines from the orifice of the urethra. This man had always passed for a woman. He lay in the same room with the mother of the child; and they acknowledged having had frequent connexion. The woman declared that she had had no commerce with any other man for three years; and the man did not deny this assertion. The idea of cohabitation with another man was further negatived by the circumstance that the infant had the same conformation of the genital organs as the father.

How did fecundation take place? The three openings in the penis were probably the orifices of the excretory ducts of Cowper's glands. But might not these have been the openings of the ejaculatory ducts? It is to be regretted that Dr. Traxel did not examine these canals: their length and direction would have thrown light on the subject. The fact of fecundation may also be explained, by supposing that during coition the posterior wall of the vagina supplied the place of the absent wall of the urethra, thus forming a complete canal. This is the most probable explanation.—*Wiener Medicinische Wochenschrift*, 1856, No. 19; and *L'Union Médicale*, August 26th, 1856.

Influence of Consanguinity on the Offspring.—Dr. Rilliet, of Geneva, states that he is investigating the subject of the influence of marriage between relatives on their offspring. He delays publishing the whole of his researches till he has rendered them more complete; but in the meantime he gives the following results at which he has arrived.

The lowering of the vital power, as a result of marriage between near relatives, is manifested by results varying in frequency, form, and degree. The following is an enumeration of them in their logical order, though not in that of their relative frequency. 1. Absence of conception. 2. Delay of conception. 3. Imperfect conception (abortions). 4. Incomplete products of conception (monstrosities). 5. Children with imperfect physical and moral constitution. 6. Children especially liable to diseases of the nervous system, in the following order of frequency —epilepsy, imbecility or idiocy, deaf-dumbness, paralysis, various cerebral disorders. 7. Children predisposed to diseases connected with the scrofulo-tubercular diathesis. 8. Children which die at an early age in larger proportion than those born under other conditions. 9. Children which, if they live beyond early infancy, are less able than others to resist disease and death. The health of the family of the married persons, and of these individuals themselves, exercises a certain influence on the degree and kind of depression of the vital force in their children. It

is probable that all the deviations from the laws just laid down are due to the health of the predecessors, as well as to the dynamic conditions in which the relatives are at the time of sexual intercourse. Thus it is incontestable—1. That all the children in a family may escape the effect of consanguinity, but this is rare. 2. That in one family some may be attacked, while others escape. 3. That the form of disease varies in those who are attacked. Thus they are not *all* epileptic, deaf and dumb, paralytic, or scrofulous; but they are variously influenced. For example, there may be observed, in one family, an epileptic, an imbecile, a child only physically and morally retarded, and another who will succumb rapidly to a disease which any other child would have resisted. In another family, there will be two idiots or imbeciles, and two healthy children. In a third, there will be one child with congenital paralysis, and several only scrofulous, &c. If certain families appear to entirely escape the action of consanguinity, it is to be feared that its direful effects will be manifested in succeeding generations, ending in the annihilation of the family.

Although no one can claim the priority of an idea of which it is difficult to determine the origin, it is not uninteresting to call to mind the opinions of legislators, philosophers, and theologians on marriage between relations. By the law of Moses, marriages between relations were forbidden as far as the third degree. In Sparta and Athens, marriages between relations of the second degree were indeed permitted by the law; but Socrates, reasoning on physiological grounds, raised his voice against a practice which he believed to be prejudicial to the healthy propagation of the race. The Roman laws interdicted marriages between all relations of the third degree; and even, though less determinedly, between those of the fourth degree. Saint Augustine, Gregory the Great, and the Council of Trent, also interdicted marriages between relations of the second degree. In Protestant countries, the marriage of relations beyond the second degree is in general not forbidden; and it is consequently more easy to observe the effects of marriages of this kind, in families where conjugal fidelity is conjoined with the facility of union between near relatives.—*L'Union Médicale,* May 24, 1856.

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

By Edward H. Sieveking, M.D.

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS, ETC. ETC.

I. *On Inflammation of the Cerebral Sinuses.* By Professor Lebert. (Virchow's Archiv für Pathol. Anatomie, &c. Band ix. Heft iii. p. 381.)

The main object of this paper is to show that suppurative phlebitis of the cerebral sinuses not unfrequently results from internal otitis by direct continuity of the inflammatory process, and that it may form a connecting link between inflammation of the ear and suppurative inflammation of the meninges, the brain, and the jugulars. The author also argues that this form of phlebitis sufficiently accounts for the pyæmic symptoms and the metastatic abscesses which are presented in the cadaveric inspection of these cases.

Professor Lebert discusses inflammation of the cerebral sinuses under three heads:—1. The spontaneous, or idiopathic variety. 2. The variety dependent upon translation from internal otitis. 3. The traumatic variety.

Of the first variety the author has seen no cases himself, and has only been able to meet with three reported by others—two by Tonnellé,* and one by Castelnau and Ducrest.†

In his remarks upon the second variety we meet a protest against the prevailing

* Archives Générales de Méd., tom xix. p. 610.

† Récherches sur les Absces Multiples, p. 138. Paris, 1848.

assumption that otorrhœa mainly depends upon tubercular or scrofulous disease of the petrous bone. Those acquainted with Professor Lebert's researches may remember that he is disposed generally to deny the formation of scrofulous matter in bones, and that he asserts the cases set down as such to have been instances of ordinary suppuration. He describes inflammation of the petrous bone as the immediate consequence of inflammation of the internal auditory apparatus, from which dangerous results are to be feared; he considers that if the inflammation were confined to the cavity of the tympanum, the comparative facility with which the pus may escape would obviate the more serious symptoms. The petrous bone having become inflamed as far as its inner surface, the dura mater becomes involved, is thickened, and pus forms upon it; and now the disease may extend to the pia mater or brain. At times the dura mater ulcerates. The brain either exhibits diffuse purulent infiltration, or abscesses form, which may discharge into the ventricles. The posterior lobe of the cerebrum and the anterior lobe of the cerebellum are the parts most frequently involved. Hæmorrhage under the pia mater, or in the vicinity of the softened portion of the brain, is rarely met with. No particular predisposition distinguishes one side from the other. The connecting link between the carious affection of the petrous bone and the morbid condition of the brain is to be found in the inflammation of the cerebral sinuses. The transverse and the petrosal sinuses are those most frequently seen filled with pus. The cavernous and circular sinuses, the torcular Herophili, and even the upper part of the jugular, may become involved. The inflammation rarely extends across to the opposite side or into the neck, the extension being limited by the formation of a plug of fibrin. In the case of pyæmia resulting, the presence of pus in the veins, and its consequent introduction into the current of the blood, is sufficient to account for it.

Professor Lebert enters into the etiology and symptomatology of otitis generally. We can only make room for a notice of his description of the phenomena accompanying inflammation of the sinuses. The ordinary symptoms are the sudden supervention upon chronic otorrhœa of rigors, heat, accelerated pulse, loss of strength, oppression, and more or less general pain of the head, furred tongue, loss of appetite, thirst, in short, symptoms resembling those of the first stage of typhoid fever. The headache is more intense than in that disease, and generally soon becomes confined to one side. There is occasional delirium, generally of a quiet character. After a few days or a week, symptoms of compression appear; the patient becomes indifferent and somnolent, and gradually passes into a comatose state, sometimes alternating with periods in which the intellect is unclouded; in the same way a paralytic condition is observed to alternate with a perfect control over the extremities. The face exhibits a similar alternation on the affected side. Convulsive affections are rarely met with. The fever accompanying the disease presents an intermittent character, often marked by so much periodicity that it has even been mistaken for genuine intermittent, and treated accordingly. These febrile paroxysms indicate the pyæmia which has supervened. The duration of the disease, as recorded in 14 cases, was respectively from nine to fifteen days in 4, from twenty-one to twenty-eight days in 5, from twenty-eight to thirty-five days in 3 cases; once, thirty-seven days; once, forty-two days; and once, sixty days.

In considering the etiology of the suppurative inflammation of the cerebral sinuses, Professor Lebert expresses a doubt as to the correctness of Mr. Toynbee's statement, that diseases of the meatus and the mastoid-cells induce disease of the transverse sinus and the cerebellum, and that the cerebrum becomes affected when the disease proceeds from the cavity of the tympanum, while the medulla oblongata is liable to become diseased when the morbid process commences in the vestibulum and cochlea. His objection rests upon the ground of his not having been able, in the cases which he examined, to localize the primary disease, owing to the general destruction of the parts concerned. With regard to sex, it is remarkable that a peculiar liability is observed among males. Of 17 cases, 14 were

males, 2 females, and 1 occurred in a child whose sex was not stated. As regards age, 14 cases were found to be distributed as follows:—7 occurred between the ages of twenty and thirty, 5 from ten to twenty, 1 at nine, and 1 at forty-six years of age.

The prognosis is necessarily unfavourable; still, even in the pyæmic stage, the disease does not appear to be absolutely fatal. The treatment should be specially directed towards curing the early stages of otorrhœa, and our author strongly urges powerful antiphlogistic treatment at that time, to be carried still further when cerebral symptoms are first manifested. Venesection is then to be followed up by counter-irritation, purgatives, cold applications to the head, and opiates, if the pain in the ear is very severe. Abscesses in the vicinity of the mastoid process are to be opened early; and purifying and soothing injections are to be made into the ear. Professor Lebert, though, as we have seen, inclined to regard otitis as essentially simple inflammation, admits that it may depend upon a dyscrasia, and in such cases advises the use of cod-oil and salt-bathing.

After the supervention of the pyæmia, the author advises the continuance of purgatives and the application of the ferrum candens, as lauded by Sédillot and Bonnet, in the vicinity of the ear as well as over the parts where secondary abscesses may be suspected. At the same time, the strength of the patient is to be supported by tonics, wine, and nutritious diet. The general remarks are followed by the details of eighteen cases, partly observed by the author, partly collected from other sources; for which, as well as for his remarks on the traumatic variety of cerebral phlebitis and the illustrative cases, we must refer our readers to the Archiv.

II. *On Pericarditis*. By H. Bamberger, Professor of Clinical Medicine in Würzburg. (Virchow's Archiv für Pathol. Anatomie, &c., Band ix. Heft 3, p. 348.) *On Valvular Diseases*. By the same. Ibid. Heft 4, p. 523.

Fifty-seven cases of pericarditis, which occurred under the eye of Professor Bamberger, and of which twenty-seven proved fatal, are here analysed, with a view of determining the etiological relations of this disease. The list only includes cases of undoubted pericarditis observed by the author himself, while all cases are omitted in which traces of former pericardial inflammation was discovered after death. The first point which results from the analysis, is the infrequency of isolated pericarditis, which only occurred three times, or in about five per cent., and it is to be observed these cases were not fatal; in several of the fatal cases which were during life regarded as simple pericarditis, the post-mortem exhibited morbid conditions of other organs, which were necessarily primary to the heart affection. Professor Bamberger's observations confirm the fact of rheumatism being the most frequent cause of pericarditis; his tables yield 17 cases dependent upon that disease, or 30 per cent.,—a ratio that approaches nearer to that established by Dr. Taylor, than that by Dr. Chambers. The former found rheumatism to be the cause of 21 out of 40 cases, or 52 per cent.; the latter only found 18 out of 135 cases of pericarditis dependent upon rheumatism, or 14 per cent. The period of the supervention of the pericardial affection varied from the fourth day to four weeks after the commencement of the rheumatism; the majority of cases occurred between the sixth and fourteenth days. Next to rheumatism, Professor Bamberger finds tubercular disease to be the most frequent cause of pericarditis; it coexisted in 12 out of the 57 cases; and if we subtract 4 cases in which other complications, as aneurism and granular kidney, coexisted, there still remain 8 cases, or 14 per cent., in which tuberculosis was the undoubted cause of the pericarditis.

Professor Bamberger found 4 cases in which there was aneurism of the aorta, a circumstance at variance with Dr. Stokes' remark, that this complication is extremely rare. He is also opposed to the views of Dr. Taylor, regarding the fre-

quency of granular kidney as a complication of pericarditis, as he only met with it in 7 cases, 4 of which presented other important complications—viz., 2 of tuberculosis, and 2 of cardiac hypertrophy. He only met with 1 case of granular metastatic pericarditis dependent upon pyæmia after traumatic inflammation of the knee-joint. One other case of pericarditis with puerperal fever might be explained in the same way. Professor Bamberger has only once observed pericarditis in connexion with typhus. He adverts to the occurrence of pericarditis after scarlet fever, but does not appear to have observed it himself. The author briefly adverts to the symptomatology and therapeutics of pericarditis. With regard to the latter, we would only note the fact, that he altogether eschews mercury; not one of his patients received it; a circumstance which deserves the serious consideration of those practitioners who place their main reliance upon that drug; since the results obtained by Professor Bamberger are by no means despicable. His treatment consisted in the local application of leeches, and of the administration of digitalis, aqua laurocerasi, acidum hydrocyanicum, bitartrate of potass, acetate of potass, and the like.

The article on valvular disease is based upon the analyses of 211 cases, observed by the author himself, 69 of which were watched up to the time of death. Cases of recent endocarditis are excluded, while only such cases of valvular disease are admitted in which there was insufficiency or contraction of the orifice. The following is the summary of the analysis:—Of the 211 patients, 109 were male, 102 female; of the 69 that died, 32 were male, 37 female. The valves were diseased as follows:

Mitral alone . . .	58 males	......	79 females.
Aorta alone . . .	34 „	......	11 „
Pulmonary alone . .	2 „		
Tricuspid	1 „		

Total of single valves affected: 185 cases.

Mitral and tricuspid	5 males	......	7 females.
Mitral and aortic	6 „	......	3 „
Mitral, tricuspid, and aortic . .	3 „	......	2 „

Total of complicated valvular affections: 26 cases.

The comparison of this analysis with the results obtained by the post-mortems is interesting; the latter showed 52 cases in which single valves were affected—viz.:

Mitral alone . . .	9 males	......	22 females.
Aortic alone . . .	12 „	......	6 „
Pulmonary alone .	2 „		
Tricuspid alone . .	1 „		

The preponderance on the side of females in regard to mitral disease, and of males in regard to aortic disease, is very palpable, and is further confirmed by what is observed in the complicated cases. Among the post-mortems 17 were complicated:

Mitral and tricuspid	4 males	......	5 females.
Mitral and aortic	2 „	......	2 „
Mitral, tricuspid, and aortic . .	2 „	......	2 „

The analysis of the ages of the patients shows the greatest frequency of mitral disease to occur between the tenth and thirtieth years, and of aortic disease between the thirtieth and fiftieth. Acute articular rheumatism was ascertained to have preceded in 51 cases, or about 25 per cent. Professor Bamberger observes, that while his results correspond with those generally obtained by German observers, they differ from those obtained in England, where disease of the aortic valves is found to predominate considerably over mitral disease. It would appear that atheroma, which is the main cause of the former, is of more frequent occurrence in England than in Germany.

We are unable to go more fully into the second paper, but would recommend it to the special notice of our readers, on account of the valuable information it contains on the subject of the pathology and diagnosis of valvular disease.

III. 1. *Notice of Two Cases that gave rise to an inevitable error of Diagnosis.—Intra- and Extra-Thoracic Tumours mistaken for Pleurisies.* By Dr. E. Moutard-Martin, Physician to St. Antoine. (L'Union Médicale, June 18, 1856.)

2. *Notice of a Few Cases of Intra-Thoracic Tumours, presenting all the Symptoms of Chronic Pleurisy.* By Dr. Oulmont, Physician to St. Antoine. (L'Union Médicale, June 28, 1856.)

In the first case given by Dr. Moutard-Martin, an hydatid cyst, contained in the left lung, simulated pleurisy; it occurred in a man aged twenty-eight, who had generally enjoyed good health until a short time before his admission into the hospital, in February, 1856. He had caught cold, and was much alarmed by an attack of hemoptysis at the end of January. The posterior surface of the thorax was resonant throughout, and the respiratory murmur was audible there, mixed with mucous râles on both sides. Anteriorly, the right side was resonant throughout; the percussion was tympanitic at the left apex to the extent of two fingers' breadths under the clavicle. The remainder of the left lung was completely dull; the transition to the dulness was abrupt, and the dulness extended to the mesial line, and laterally to a line drawn from the armpit parallel to the axis of the body. The heart was pushed downwards, and towards the mesian line. There was no trace of the respiratory murmur, or of vocal resonance throughout the dull portion. Two days after admission, an amphoric blowing was audible in this part, accompanying a few deep inspirations; it was very distant, scarcely perceptible; and sometimes there was a feeble metallic tinkling. The diagnosis was general bronchitis of six weeks' date; pleurisy, with effusion circumscribed by false membranes, and limited to the left anterior thorax.

After a brief absence from the hospital, he was readmitted on the 19th of March, when the dulness of the left side had entirely disappeared, and the heart had recovered its normal position. The respiratory murmur was perfect throughout the portion which had previously been dull. Posteriorly there was marked dilatation of the left side, with complete dulness throughout, and absence of the respiratory murmur. There was no fever, the cough and expectoration continuing. The diagnosis now was effusion in the left thorax, probably caused by rupture of the adhesions, which had encysted the primary effusions; the effusion being limited in the large pleural cavity. On the second of April, febrile symptoms having supervened for a few days, the patient expectorated an immense quantity of purulent liquid, which induced a belief that a communication had been established between the bronchi and pleura. The stethoscopic signs remained the same. Repeated attacks of the same kind of cough and expectoration occurred, and on the 6th of April the patient died asphyxiated.

The post-mortem showed, in the anterior portion of the left thorax, firm adhesion of an old date, and disposed circularly; a portion of the anterior surface of the left lung was free from adhesions, and it was here that dulness and the absence of the respiratory murmur had been observed. The remainder of the left lung was completely adherent, and there was no trace of effusion. The volume of the lung was enormous, and yielded the sensation of an enormous pouch with thin parietes, completely filled with liquid. In seeking to detach the adhesions, the pouch gave way, and an enormous quantity of coagulated blood escaped, with the remains of an hydatid cyst. At one part of the pouch a communication was discovered, opening into the first division of the left bronchus. The opening was plugged with a portion of the cyst-membrane. The hydatid cyst was developed in the upper lobe of the lung, which had forced back the lung in every direction, had converted it into a fibrous state, and reduced the lower lobe to a thickness of about two centimètres.

The second case of Dr. Martin's was a young woman, aged twenty-one, in whom a similar error in diagnosis took place; it was caused by a tumour developed between the kidney and supra-renal capsule, which pushed the diaphragm into the thorax up to the fourth rib, and gave rise to all the stethoscopic symptoms characteristic of pleuritic effusion. These were complete dulness posteriorly, and to the right of the inferior half of the right thorax; respiratory murmur absent, or nearly so, in the part; with distinct egophony throughout. It is to be observed, however, that there was no cough.

Dr. Oulmont relates three analogous cases; the first was one of aneurism of the thoracic aorta, occupying the left pleural cavity, and simulating pleuritic effusion. It occurred in a man aged forty-six. The symptoms here were, cough, mucous expectoration; a sense of weight on the left side of the thorax; great dyspnœa on lying on the back or right side. The left side was a little distended, but the intercostal spaces did not project. The left thorax was completely dull below the third rib. In the sonorous part a fine vesicular respiratory murmur was heard; no respiratory murmur was heard throughout the dull portion, where also the vocal resonance was absent. The voice produced a feeble vibration of the thoracic parietes. The beats of the heart were superficial, sufficiently strong to raise the head of the observer, and slightly intermittent. No abnormal sounds were heard, nor was there any venous reflux in the neck. There was slight œdema of the lower extremities, no fever, the pulse strong.

In Dr. Oulmont's second case, the false diagnosis was caused by cartilaginous degeneration of the pleura, accompanied by abscess of the thoracic parietes. The patient was a clerk, aged twenty-one, suffering from cough, which commenced with a stitch in the right thorax. When admitted into the hospital, he presented, near the sixth right rib, below the anterior border of the armpit, a tumour of the size of a nut, elastic, dull on percussion, and fluctuating. It did not diminish on the application of pressure. The thorax was dull from the clavicle to the base of chest. There was crackling and a blowing murmur at the apex of the lung. Laterally there was no respiratory murmur in a space of from twelve to fifteen square centimètres below the tumour. The thorax was not dilated nor malformed. The voice throughout the dull part was indistinct. The dyspnœa was great. There was no fever. Paracentesis was intended, but postponed from the indications not being sufficiently positive. After death the two layers of the right pleura were found united by a dense grey layer, varying in thickness from 2 millimètres to 1 centimètre (0·78 in. to 0·39 in.), 11 centimètres long, by 8 centimètres broad. It extended from the fifth rib to the base. Below this layer was a mass of tubercle (7 centimètres by 5), and from two to four centimètres in thickness.

In Dr. Oulmont's third case, the pleuritic effusion was simulated by disease of the liver. A gardener, aged fifty-five, was admitted with cough, dyspnœa, and puriform expectoration. The dyspnœa was not much affected by position. The two sides were symmetrical. There was complete dulness on the right side, from the level of the nipple downwards; the dulness posteriorly extended from the inferior angle of the scapula downwards. There were sibilant and sonorous râles on the left side, and at the upper part of the right side. Throughout the dull portion no respiratory murmur was audible, nor was there any vocal resonance in the part. The heart presented no alteration in rhythm or sound; the pulse was feeble; there was no fever. During the patient's residence in the hospital, he had an attack of icterus, ascites, and œdema of the inferior extremities supervened. Before death there was much fever. The symptoms were entirely due to enlargement of the liver, which was pushed up into the thorax, and covered the whole left side of the stomach. The organ was of a greyish-white colour; when cut it creaked, and the cut surface did not present the ordinary appearance of hepatic tissue, but resembled in some parts scirrhus, in others it looked as if infiltrated with pus. It should be noted that the liver did not descend into the abdomen, a point of importance in establishing a diagnosis between pleuritic effusion and hepatic enlargement.

IV. *On the Deposit of Urea on the Skin and Mucous Membranes in the Typhoid Stage (Consecutive Fever) of Cholera.* By Dr. A. DRASCHE. (Zeitschrift der k. k. Gesellsch. der Aerzte zu Wien. Jahrgang xii., 3 and 4 Monat. Heft, p. 161.)

During the consecutive fever of cholera, when the urine is suppressed, Dr. Drasche has observed a peculiar fatty, shining perspiration on the forehead and face, which disappears on the return of the renal secretion. This perspiration, on evaporating, leaves a large number of minute crystals on the surface, in the form of micaceous laminæ, scales, and granules. The crystalline deposit is first perceived on the eyebrows, and successively appears on the temples, nostrils, forehead, upper lip, and hairy scalp; the parts look as if dusted over with fine flour; the minute crystals are arranged round the orifices of the sebaceous glands. The crystals are not found until a late period on the neck, chest, upper arms, and abdominal parietes. They were only once seen on the lower extremities. At times, the crystals may be found on the mucous membrane of the mouth and fauces. Dr. Drasche found the crystalline deposit invariably to disappear shortly before death, and to be replaced by a viscid fluid. Examined by the microscope, the crystals were found to consist of broken pieces of an acicular form, some of which still preserved the acuminated termination: they were colourless. The crystals were embedded in a yellowish fatty mass, containing hair and epidermal cells. When allowed to crystallize from an alcoholic solution, long silky, colourless needles, arranged in pencils, formed, which were regarded as, and proved by chemical tests to be, urea.

The symptoms, according to Dr. Drasche, accompanying this secretion of urea on the surface of the skin, are mainly those of a disturbance of the nervous functions, especially of the brain, together with partial or complete arrest of the urinary secretion. The author has not invariably observed this cutaneous secretion of urea to be associated with complete arrest of the renal functions, as he has, in a few cases, succeeded in collecting or obtaining by the catheter a small quantity of urine, of a very low specific gravity (1004·6), with alkaline reaction, and of a dirty yellow colour. In the case of a female, recently confined, who exhibited the cutaneous secretion of urea in a marked form, the milk contained but little casein, much sugar, and a considerable quantity of undecomposed urea, but not a trace of carbonate of ammonia.

The autopsy of the cases presenting the above phenomena showed an extensive tumefaction of the bronchial mucous membrane, with a hepatized condition of the inferior portions of the lungs. The kidneys were very soft, large, and tumefied, their surface presenting arborescent injection, and the renal veins being filled with dark, viscid blood. The entire mucous membrane of the urinary tract presented a catarrhal condition.

Dr. Drasche, among 800 cases of cholera observed during the epidemic of 1855, met with the cutaneous secretion in 12 cases, all of which proved fatal during the consecutive fever of cholera. Only one of the twelve patients was a man, the others all women.

As all the cases in which Dr. Drasche observed the cutaneous evolution of urea proved fatal, it is difficult to avoid the conclusion that the fatal result was due to something else than an accumulation of urea in the blood, since the vicarious action of the skin would have prevented so uniform a mortality in the cases in which it was observed. It is to be apprehended that Dr. Drasche's observations will influence some of the current theories regarding the consecutive fever of cholera.

V. *On Spontaneous Rupture of the Spleen.* By Professor MÖLLER, M.D., Königsberg. (Vierordt's Archiv für Physiologische Heilkunde, 1856. Heft 2.)

The following is an abridged account of a case of spontaneous rupture of the spleen, observed by Professor Möller:—

H., a labouring-man, aged fifty-three, suffered in 1853, for seven weeks, of ague, followed by albuminuria and dropsy. He recovered so far as to be able to return to his work, though slight dropsical symptoms continued to recur. On the 11th August, 1854, he was seized with rigors and epistaxis; on the 15th he came under Dr. Möller's treatment, presenting the yellowish complexion of a malarious patient, great prostration, pains in the head and extremities, vertigo, sleeplessness, and at night slight delirium. There was slight fever; tongue dry, with red tip and edges; abdomen tender; liver and spleen distinctly perceptible under both hypochondria; bowels costive. The treatment consisted in the administration of stimulants and purgatives. The liver diminished, but on the 18th August the spleen still maintained its increased size. On the 19th, he was found *in articulo mortis;* breathing stertorous; pulse 90, small; the spleen was *no longer perceptible.* Death ensued the same evening. *Post-mortem,* Aug. 20:—Pleura adherent throughout; lungs anæmic; left side of heart hypertrophic; a little black blood in the left ventricle; no blood on right side. The abdominal viscera were covered with a thin layer of greasy, dirty, brown-red blood. Five ounces of black coagulated blood lay at the hilus lienis; when this was removed, a transverse rupture of the spleen, three-quarters of an inch long, was found near the hilus. The capsule of the spleen was much thickened throughout, and presented numerous fibrous plates, and was extensively adherent to surrounding parts. The parenchyma was pulpy, and of a reddish-grey colour. The large vessels at the hilus were bloodless. Liver large, anæmic, rather fatty. The mucous membrane of the stomach and duodenum softened. The kidneys in the second stage of Bright's disease. No trace of peritoneal inflammation.

Dr. Möller gives the outlines of a second case, which ran a similar course to the last, but in which the diagnosis could not be verified by a cadaveric inspection.

Dr. Möller then analyses the 25 cases of spontaneous rupture of the spleen, which he has been able to collect, and finds that the main feature presented in the organ was softening. He is unable to gather any indication from which the occurrence of rupture could be prognosticated. In 9 of the cases, the account of the disease which preceded is so meagre, that no conclusion can be drawn as to the etiology. In 6 it occurred in the course of typhus, gastric, yellow, and malignant intermittent fever. The remainder seem to have resulted from ordinary intermittent fever, where the hot stage appears to be that which most predisposes to the occurrence. One case, not included in the above, is also quoted, in which the rupture had resulted from a perforating ulcer of the stomach involving the adherent spleen.

VI. *Practical Remarks on Diseases of the Spleen.* By ALFRED G. TEBAULT, M.D., of London Bridge, Virginia. (The Americal Journal of Medical Sciences, January and April, 1856.)

The seaboard of the State of Virginia is distinguished by the endemic occurrence of diseases of the spleen, which Dr. Tebault considers under the following heads, which he illustrates by sixteen cases. In the introductory remarks he observes, that "infarctions of the spleen occur more frequently in intermittent than in remittent, in the latter than in typhoid fever, and whenever it is manifested in the last there always obtains a marked proclivity to the paroxysmal type." The greatest enlargements are found, not during the cold stage, but in consecutive febrile paroxysms. When chronic, they may last for years without inconvenience to the patient,* except such as arises from its bulk; a florid hue of the countenance, and an apparently healthy re-establishment of all the functions may be observed, but usually a slow degeneration results.

1. *Passive Hyperæmia* occurs during the prevalence of damp easterly winds,

* On this subject the reader may consult with advantage the analysis of Dr. Dempster's Report on Marsh Miasm: Medico-Chirurgical Review, Oct. 1855.

with gastro-intestinal derangement; pale copious urine, lassitude, and sallow skin. There is a dull soreness in the left hypochondrium, increased by pressure and lying on the left side, with more or less increased splenic dulness. The trunk in sitting curves to the left. If the patient be well purged early, the enlargement is reduced speedily; but if neglected, chronic enlargement supervenes.

2. *Active Hyperæmia.*—In this case the enlargement of the spleen takes place with greater rapidity, and is particularly apt to supervene if, on the occurrence of the fourth fit of a bilious intermittent fever, the remittent type be assumed. The tumour advances at first horizontally, and then by its weight descends, and may spread over the abdomen, so as to extend from the left to the right ilium. At the commencement there is generally concomitant congestion of the liver, with vitiated, but not bilious, alvine secretions. When the hepatic function is restored, the spleen begins to contract.

3. *Hyperæmia with partial Organic Change.*—When cases of hyperæmia resulting from fever are left to nature, or imperfectly treated, organic change is manifested in the persistent enlargement, with an irregular surface and notched margins. An induration of the entire mass may result from repeated accessions of fever; but a complete cure may still be effected. This variety ought scarcely to be separated from the next.

4. *Hypertrophy.*—Here we find a tumour of an oblong form, firm to the touch, and of fleshy hardness throughout, and of little elasticity. The organ may increase to an enormous size, and weigh from ten to twelve pounds. The weight may be the only inconvenience; but after a time symptoms of general anæmia supervene, the blood is deficient in fibrin and red corpuscles, and the process of assimilation is generally imperfect. After death, the spleen is found to resemble a dark muscular substance in firmness and colour, and exhibits its cellular tissue much thickened. In one instance the liver was atrophied, and reduced to less than one-third its natural size. Older cases appear to be irremediable; but more or less benefit may generally be obtained by relieving the liver and improving the state of the blood.

5. *Inflammation.*—Though splenitis appears to be chiefly the result of traumatic influences, Dr. Tebault is satisfied that both an acute and chronic idiopathic variety occur. Great nicety of manipulation and care in diagnosis are necessary to determine its existence, and to avoid its being confounded with neuralgia. Dr. Tebault describes the symptoms thus: pretty constant and acute fever, with little or no remissions; pulse somewhat hard and frequent; emesis at times; pain of the spleen persistent, sharp, and increased on the least pressure, pain extending often to the left shoulder and ilium; disagreeable sense of heat in the abdomen. The margin of the spleen dips inwards as it were, and becomes more indistinct than in hypertrophy of the organ; while the central portion presents a rounded, tense, and elevated mass, which cannot be separated ever so little from the margin of the ribs, against which it presses, and is often discernible to the right, above the general level of the abdominal surface, the lower intercostal spaces are usually depressed, the trunk curves towards the left, though the patient cannot lie on that side, and the thigh is habitually semi-flexed. The temper is irritable. The inflammation readily spreads over the peritoneum, and tympany, delirium and death may close the scene. Should recovery, however, take place, plastic adhesions are contracted with the serous lining of the abdomen and viscera. The inflammation is seated primarily in the peritoneal and proper coats, but may involve the parenchyma.

This is the description of the acute form; the chronic form is more obscure, the symptoms all less prominent, the termination may be resolution, suppuration, or degeneration, and the affection may be complicated with ascites or hæmorrhages.

6. *Suppuration.*—When an enlarged, tense, and painful spleen becomes irregularly soft and yielding, while rigors, not periodic, supervene, we may suspect suppuration; and our diagnosis proves the more certain if, in addition, a hectic fever is established, and the cuticle, especially that over the tumour, appears shrivelled

shiny, and furfuraceous. Adhesions may result, and the pus be evacuated by the bowels, the stomach, or the lungs. Instances of the discharge taking place by expectoration, and another per rectum, are given.

7. *Softening.*—Degenerative softening takes place in hypertrophied spleens, unaccompanied by symptoms of inflammation. The general symptoms are those of scurvy; petechiæ form on the extremities and trunk and the mucous membranes; the gums become spongy, sore, purple; the breath hurried and offensive, while the pulse is very feeble and compressible. The organ itself is so soft as even to appear like a bag of fluids, its edge terminating insensibly among the viscera. The spleen, on examination after death, resembles a sac containing grumous or semi-fluid blood. Two cases are given in which cures were obtained.

8. *Neuralgia.*—The author adverts to this as a frequent accompaniment of anæmia, but admits the difficulty in distinguishing it from pain in the intercostal nerves, from pleurodynia, gastralgia, and the passage of renal calculi.

9. *Displacement.*—In consequence of enlargement of the spleen, the organ is occasionally dislocated altogether from its attachments.

In the second paper, contained in the April number, Dr. Tebault treats of some complications and morbid results observable in malarious cachexia, in which affections of the spleen form an important link in the chain of morbid phenomena; he considers them under three heads, hæmorrhage, anæmia, and dropsy. Some interesting remarks are added on diagnosis, prognosis, and treatment. We confine ourselves to extracting the following statement of the numerical proportion of the occurrence of periodic fevers among the white and black populace in four distinct years:

1st year	. . . .	26 blacks	to	100 whites.
2nd „	. . . .	15	„	100 „
3rd „	. . . .	40	„	100 „
4th „	. . . .	33	„	100 „

VII. *On the Presence of Lumbrici in the Biliary Ducts.* By Professor Forget, of Strasburg. (L'Union Médicale, May 29, 1856.)

Professor Forget gives the details of a case of typhoid pneumonia in a female, aged sixty-five, in whom, after death, the ductus communis choledochus was found occupied by a lumbricus, one extremity of which projected into the duodenum, while the opposite end extended into the left division of the hepatic duct. This worm was fresh and well-preserved, and exactly filled out the canal. On introducing a director into the right division of the hepatic duct, it entered into an anfractuous cavity, of the size of a walnut, filled with a pyoid liquid, and containing a lumbricus, coiled up. This worm was softened, and in a state of decomposition. The part which it occupied appeared to be a dilated duct. The surrounding hepatic tissue presented about ten abscesses, varying in size from that of a pea to that of a chestnut, lined with thick pseudo-membrane. The liquid, examined under the microscope, appeared to consist of amorphous granules, fat globules, and a few yellow oval corpuscles, with granular contents in the centre.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E., London.

I. *On the Employment of Cold in Gonorrhœal Epididymitis.* By Prof. Sigmund. (Wiener Wochenschrift, 1855. No. 52.)

Whatever may be the success attendant upon the treatment of gonorrhœa in recent times, the number of cases of epididymitis does not seem to be diminished.

Prof. Sigmund published in 1850 an account of the advantage he had derived from treating it with cold, and all his subsequent experience has confirmed the statements he then made. Under the term he includes the inflammatory condition of the tunica vaginalis, of the epididymis itself, and of the cord, the affection of one of these parts preponderating in different cases. The form in which the tunica vaginalis becomes rapidly distended with exudation is a very painful one; that in which the inflammation of the epididymis preponderates is less so, and when there is considerable effusion into the tunica and around the epididymis, which cases are, however, rare, the suffering is excessive, and is accompanied by general disturbance. In all degrees and combinations of the affection, cold is found to be a powerful remedy, assuaging pain, preventing farther effusion, and, when continuously applied, expediting absorption more than any other means. The patient lying on his back, the scrotum is supported by means of a light suspensory, or a towel placed between the thighs, and then covered with compresses dipped in water. For the first three or four hours, the degree of cold should be only moderate, lowering the temperature then gradually, and in six or eight hours adding ice, if the application acquires heat rapidly. This degree of cold is continued as long as it gives the patient relief, but when it ceases to do so, and still more when it induces an uneasy sensation, the temperature must be raised from cold to merely cool, and the application allowed to remain on until it becomes warm. Finally, lukewarm applications are to be continued until all inflammatory appearances have subsided. The application must be constant, continuing it uninterruptedly day and night, its occasional use not sufficing. There are persons who cannot bear the application of even moderate cold, and especially when made to the abdomen, without colic, diarrhœa, catarrh, rheumatism, &c., being induced; and this is especially the case with those disposed to scrofula, tuberculosis, rheumatism, or gout. It is found, however, by experience, that even very sensitive persons will bear well-wrung compresses, providing that the degree of cold be gradually and slowly increased. Conjointly with this treatment, the patient takes a saline purgative at intervals, so as to induce from two to four fluid stools, one or two such being also procured during the diminution of the inflammation. For diet, the patient is to be limited to thin, easily-digested fluid substances, tea and coffee being prohibited during the acute stage. Young plethoric persons, in whom the symptoms run high, and are attended especially with much exudation around the epididymis, may, exceptionally, first require the application of leeches to the groin. Very severe, enduring, or increasing pain may be relieved by anodynes, and when the tunica vaginalis is much distended, a puncture or subcutaneous incision may be required; but such cases are quite exceptions, the cold proving, in the great majority, the best anodyne and antiphlogistic. At most, an anodyne is required at night, in order to secure sleep. When the inflammatory symptoms have disappeared, and the epididymis will bear the moderate pressure of the hand, we must seek to obtain the absorption of the exudation; and for this purpose, Professor Sigmund prefers Fricke's treatment to any other mode of making compression.

Numerous comparative trials have convinced him that the treatment of this affection by repeated bloodletting in nowise deserves preference, the employment of cold alone proving in its results far more satisfactory in the great majority of cases. When resorted to early, also, it exerts a very rapid effect in arresting the farther development of the affection.

II. *On Induration of the Epididymis subsequent to Gonorrhœal Inflammation.* By Prof. SIGMUND. (Wiener Wochenschrift, 1856. No. 12.)

Induration is a very frequent consequence of gonorrhœal epididymitis, the exudation becoming gradually denser and harder than during the inflammation. The part loses its suppleness and elasticity, but undergoes no increase of size

after the cessation of inflammation; nor is there any formation of new morbid structure. There is but little functional disturbance. Epididymitis most frequently occurs in scrofulous, tuberculous, delicate-skinned persons; and when there is varicose distension of the vessels of the cord. In such, it usually pursues a chronic course, and often terminates in induration. The principal seat of the induration is in the epididymis itself, but the tunica vaginalis always participates in this, if it be only to a small extent. As stated in a former article, the inflammation in epididymitis attacks different parts in particular cases; and in 1342 cases observed by the author since 1849, the seat is thus distinguished:—The epididymis alone, 61; the epididymis and cord, 108; the epididymis and the tunica vaginalis, 856; all the three parts, 317. The testis itself, in the great majority of all these cases, is but little enlarged, and the appearance erroneously denominated orchitis is due to the inflammation of, and exudation from, the internal surface of the tunica vaginalis. This becomes more or less tense and distended, and at a later period hardened and thickened, so that the inexperienced practitioner may easily be deceived as to its true nature. The induration of the epididymis may be either general or partial, and in the former case the part may at first attain more than double its normal size. After the cessation of the inflammation, however, it gradually diminishes again, and may continue to do so until it is much less than the natural size. In this way it is often reduced to the size of a hazle-nut, or less, and after some years, even to that of a pea. With the diminution in size, the induration goes on increasing, proceeding from a cartilaginous to an osseous hardness. Diminution of the testis usually takes place under the influence of general induration, but rarely when this is only partial. The surface of the indurated epididymis has a knotty, irregular, glandular feel, the skin of the scrotum sliding over it as in the natural state. There is no special pain in the indurated part, and only the usual peculiar sensation ensues upon pressure, unless there has been excess in diet, exercise, &c. The patients complain sometimes of general debility and of defective procreative powers. Melancholy also prevails, and is only explicable by the connexion of the spermatic plexus with the sympathetic. The same mental disturbance is produced in cases of complete atrophy and of loss of the testis.

The exudation does not differ in character from that met with in other parts that have been inflamed; but tubercular matter is also frequently deposited in these subjects, invading the whole epididymis, but only exceptionally going on to softening. The author has never met with cancer as a result of gonorrhœal epididymitis.

The epididymitis giving origin to the induration occurring most frequently on the left side, it is rare to meet with induration on both sides, although tubercular disease unpreceded by gonorrhœal epididymitis, is usually both-sided. Of 1342 cases of epididymitis that occurred to the author, he found the left side alone affected in two-thirds, and the right side in one-third; and he has met with it on both sides in seven cases per cent. in hospital and five per cent. in private practice, the inflammation never being simultaneously developed on the two sides, but usually at an interval of some weeks. There is always an exact accordance between the severity, extent, and duration of the epididymitis and the amount of the resulting induration. Although often a troublesome affection, it is, when properly treated, and when not occurring in persons suffering from highly-developed general dyscrasis, not dangerous. Prof. Sigmund knows several persons who have reached advanced age undisturbed; but daily experience shows us that in others its presence produces great mental disturbance. The sooner after the cessation of the inflammation the treatment of the induration is commenced, the sooner is the cure accomplished, or at all events a more rapid absorption of a portion of the exudation achieved. The scrotum must be supported by a well-fitting suspensory bandage, which does not exert compression. If there is even a slight increase of the normal sensibility, from two to four leeches should be applied along the cord every day or two, and cold applications kept to

the part constantly. As soon as this sensibility is diminished, from five to eight grammes of mercurial ointment are to be rubbed along the course of the cord and the inner part of the thigh every evening, painting the diseased half of the scrotum with iodine, for which purpose we may employ either a solution of one drachm of iodine or one ounce of iodide of potassium in six ounces of water, ointments being less useful. If eczema be induced—which may usually be prevented by washing the parts every morning—the ointment must be suspended. Daily evacuations must be particularly secured by means of aperients or clysters, so that the obstructed rectum may offer no impediment to free circulation in the cord. No advantage is derivable from either cold or warm baths; but Dr. Sigmund has found luke-warm baths, prepared with from four to sixteen pounds of salt in four pails of water, or with sea-water, accelerate absorption remarkably, although this is seldom complete, except in recent induration occurring in young persons. The internal employment of iodine, especially of the iodide of potassium or soda, assists in diminishing the size and hardness of the part; but given alone, however long continued, it is of little avail. Mineral waters and baths containing iodine and bromine are more useful, and may be longer continued. The neuralgia that sometimes accompanies the affection was found to be remarkably relieved by the employment of electricity, which yielded no satisfactory results in other respects. Advantage is obtained by the patient wearing a suspensory made of rabbit or other animal's skin.

III. *On Sub-Arterial Cysts of the Wrist.* By M. Chassaignac. (Moniteur des Hôpitaux, 1856. No. 78.)

M. Chassaignac calls attention to a form of ganglion which, placed beneath the radial artery, unless properly understood, may give rise to very serious errors. From excess of labour, or the exertions necessary to raise heavy burthens, the small tumour may acquire considerable development. The fingers of the surgeon, when applied over the cyst, are raised by the pulsations, which are remarkable for their energy and the breadth of space they extend over. This extent of pulsatile surface immediately suggests the idea of radial aneurism, and if the examination be continued with the limb remaining in its ordinary attitude, an error can scarcely be avoided. The differential diagnosis may be established by bringing the wrist into a state of forced flexion, when—whether it is that the artery is displaced, or that it ceases to be stretched over the eminence formed by the cyst—the pulsations no longer exist, and it is evident that no aneurism is present. In treating these cases, M. Chassaignac employs the iodide of potash ointment, rubbing it in every two hours during a week. On the dorsal surface we may treat ganglia with advantage by crushing them, by subcutaneous puncture, seton, or iodine injection; but in the case of these sub-arterial cysts of the wrist, which are in communication with the radio-carpal articulation, these means of treatment are not applicable. The iodine frictions give rise to no accident, and seem possessed of all desirable efficacy.

On one occasion, M. Chassaignac had the opportunity of examining one of these cysts in a subject brought for dissection. The tumour resembled an almond in form and size, and occupied the space comprised between the tendons of the *supinator longus* and the *palmaris longus*, lying on the anterior portion of the *pronator quadratus*. The radial artery in its downward progress having reached the upper part of the tumour, was at first so intimately connected with its front part as to seem to form a portion of its walls. Very soon, however, it deviated obliquely on its external side, and reached the fossette called the anatomical snuff-box. With the object of ascertaining the anatomical origin of the tumour, it was dissected with the greatest care, and separated from all parts with which it had not contracted fixed adhesions. In this way it was circumscribed for four-fifths of its extent, but posteriorly and below it was firmly fixed to the bone by a kind of pedicle proceeding from the anterior part of the lower radio-cubital articulation.

It was only, in fact, a diverticulum of the synovial membrane of this joint, and it had raised up the lower fibres of the *pronator quadratus*, which, forming a kind of arc, produced a sort of strangulation of the pedicle at its upper part. The continuity of the cyst with the articulation was completely demonstrated, a probe freely passing from one to the other.

IV. *Luxation of the Femur on to the Ramus Superior (or Descendens) of the Ischium.* By Dr. Krause. (Henle und Pfeufer's Zeitschrift, Band vii. p. 346.)

Dr. Krause describes the case of a man who had fallen on to the frozen ground from a scaffold fifty feet high, nothing having broken his fall. Among other injuries was found a dislocation of the femur. The left limb was in appearance considerably shortened, having the hip and knee-joints flexed, and the ankle stretched out. It was strongly rotated inwards, the foot being directed towards the tarsus of the sound limb. The flexion forward at the hip amounted to about 30°, the rotation to 90°, so that the anterior surface of the thigh was directed inwards. In this position the thigh was immovably fixed, with the exception of slight adduction and flexion. The trochanter was found to be more projecting than on the other side, while the configuration of the region was considerably changed by reason of a large swelling in the vicinity of the ischiatic notch. The head of the bone could be easily felt under the muscles. The patient soon died, the liver and spleen having been ruptured in the fall.

On examination, the head of the bone was found, at the upper end of the *ramus superior* (or *descendens*) of the ischium above the tuber, on a level with the empty acetabulum, so that during life the limb would have been neither shortened nor lengthened. It was only covered with the *glutæus maximus*, the *g. medius* and *minimus* lying upwards and forwards. The sciatic nerve was dragged somewhat inwardly, but without being much torn. The head of the bone was almost immovably fixed, in consequence of the great tension of the tendons of the *obturator externus* and *internus*, which winded around the neck, the one on the upper, the other on the lower side, and compressed it against the portion of the ischium between the tuber and the under edge of the acetabulum. The *pyriformis* and *gemellus superior* muscles were uninjured, although the latter was very tense; but the pressure of the head had completely crushed and pinched in the middle part of the *gemellus inferior*, while its origin and insertion were nearly uninjured, although very tense. The capsule was torn from the posterior surface of the neck close to its insertion, to an extent of more than one-half the circumference of the neck, the torn portions lying across the opening of the acetabulum. The *ligamentum superius* was uninjured, though much stretched, and carried considerably more posteriorly than its normal position. The *ligamentum teres* was torn at eleven millimètres' distance from its insertion in the head of the bone, an enormously extended thin string of it still remaining, and passing obliquely forwards over the head of the femur. An irregular portion of the acetabulum (twenty-seven millimètres by eighteen) was broken off, and lay upon the tendon of the *obturator internus*, which was expanded over the opening in the acetabulum. The fragment must have been carried away under the operation of a force acting from before backwards, the resulting aperture being filled up by the extended tendon of the *obturator internus*. All other parts of the joints were uninjured, as also were the *quadratus femoris* and the other muscles arising from the tuber.

This kind of fixing the head of the bone between the two obturators, the tendons of which, in a normal condition, both lie behind it, could only be brought about by strong flexion, accompanied or followed by rotation inwards. This winding round the neck, together with luxation on the *ramus ischii*, can only be produced by the impulsion of the neck of the femur (the limb being flexed forwards and rotated inwards), by a force acting in the longitudinal axis of the limb, against the upper anterior edge of the acetabulum. This forms the point of support to the fulcrum, around which the continued movement of the head against the pos-

terior edge of the acetabulum takes place. In the present case this broke away, and allowed of the issue of the head backwards between the two obturators. This view of the mechanism of the accident was confirmed by an experiment which the author instituted, and by the statements of the eye-witnesses of the occurrence, who state that the man, slipping horizontally forwards on the scaffold, fell to the ground with his chin and knees together. Re-position, in the event of survival, could here have been accomplished only by prior rupture of the encircling obturators, as these muscles quite prevented all attempts at reduction.

V. *On Fracture of the Costal Cartilages.* By M. Broca. (Bulletins de la Société Anatomique, tome xxx. pp. 334—9.)

M. Broca recently reported to the Paris Anatomical Society upon a specimen of this accident. No clinical history could be furnished, as it occurred in the person of a madman, of robust constitution, who died of cerebral congestion, at the age of thirty-nine. At the autopsy, the cartilages of the sixth, seventh, and eighth ribs were found fractured, and the fragments were solidly united by an osseous callus. This simultaneous existence of three fractures is not unprecedented, as Magendie has recorded an example, and M. Leudet met with a case in which five of the cartilages were fractured by one violent blow. When only one cartilage is fractured, the general form of the chest does not undergo any notable change; for although the fragments may be displaced in an antero-posterior direction, the neighbouring costal arcs oppose any longitudinal displacement. One of the fragments, usually the internal or sternal, forms more or less relief under the skin, but there is no true riding. In the present case, the two last ribs are deprived of the point of support furnished by the sternum, and the fracture is prolonged to the first false rib, so that nothing prevents the riding of the fragments, which at certain points amounts to a centimètre. Between the fragments of the two lower cartilages, there is also a considerable separation in the antero-posterior direction. In spite of these unfavourable conditions, consolidation has taken place by means of an osseous callus, about a centimètre in thickness, which, to use M. Malgaigne's phrase, both separates and unites the fragments. This new osseous production, secreted by the perichondrium, completely surrounds the external fragments; but it is not prolonged over the anterior surface of the internal fragments. This example of bony union by means of a callus derived from the perichondrium, is the only one on record; and was in this case, where the fragments were entirely separated, the only one possible: but when the fractured surfaces still correspond, a direct union may be established between the two ends of the divided cartilage.

M. Broca has found, in experiments made upon the *articular* cartilages of dogs and rabbits, that, at the end of a month, the process of cicatrization is far from being terminated. But there is already to be observed a plastic, demi-transparent, continuous, plastic layer adhering to the two lips of the incision, and exhibiting under the microscope a manifest fibroid condition, which is the first stage of complete fibrous organization. It is only at the end of three or four months that the cicatrix becomes solid, and merits the name of a fibrous callus. As to the newly-formed substance which becomes organized between the divided edges, it is not of a cartilaginous nature; but is purely fibrous, as are, in fact, most of the cicatrices of soft parts.

These fractures are generally said to be due to the operation of *direct* causes; and, in fact, when the great elasticity and flexibility of these cartilages are considered, it is difficult to suppose that they can be broken by causes operating at a distance. Still, a recent case shows that an indirect cause, a mere muscular movement, may suffice for the fracture of these costal cartilages. A porter, aged forty-two, having placed a sack of peas on his left shoulder, another was too suddenly thrown on this. The man was forced forward, and while he rose again he experienced so violent a pain that he was forced to let go his load. The pain occurred

at the anterior inferior part of the chest, a little towards the right side. On examination, there was here observed, seven or eight centimètres external to the mesial line, on a level with the sixth, seventh, and eighth ribs, a well-marked angular projection. Pressure, coughing, &c., produced great pain, but there was no crepitation, and exploration of the chest, as far as the pain permitted it, did not detect any abnormal movement, although the man was very thin. Nothing of the kind existed upon the opposite side, and the patient was positive that prior to the accident his chest possessed its natural configuration. It is, then, highly probable that muscular contraction was here the determining cause of the fracture; for the patient received the shock of the burthen on the *left* shoulder, while it was at the lower part of the *right* side of the chest that the fracture occurred. The man, bent forwards by the violence of the shock, made a sudden and strong effort to preserve his equilibrium, and this effort overcame the resistance of the cartilages.

VI. *Glycerine and Tannin in Vaginitis.* By M. DEMARQUAY. (Bulletin de Thérapeutique, tome l. p. 541.)

In the treatment of this affection, M. Demarquay has found a composition, consisting of eighty parts of glycerine and twenty of tannin, of great service. When the vaginitis first appears, the inflammatory symptoms should be calmed by appropriate regimen, baths, and frequent emollient injections. When the first stage of the inflammation has passed away, and the careful introduction of the speculum has become possible, abundant injections of water are to be thrown in, so as to remove all the muco-pus which lines the walls of the vagina, and these are then dried by a plug of charpie placed at the end of a long forceps. Then, three plugs of wadding, well soaked in glycerine and tannin, are to be introduced. Next day, after a bath, the plugs are removed, new injections made, and the dressing repeated. M. Demarquay has never had to have recourse to more than four or five such dressings. After discontinuing them, astringent injections, consisting of infusion of walnut leaves, in which one drachm of alum to the quart has been dissolved, are employed two or three times a-day for a week or ten days.

VII. *On Wounds over the Olecranon.* By M. VELPEAU. (Moniteur des Hôpitaux, No. 89.)

In relation to a case in which diffuse inflammation of the arm followed a wound over the olecranon, M. Velpeau took occasion to make some interesting remarks. Such wounds call for particular attention, for, owing to the anatomical composition of the region, they may sometimes induce subepidermic or subcutaneous inflammations; while at others, the bursa of the olecranon, the olecranon itself, or the joint, may suffer. When, under the influence of any cause, as of a contusion, inflammation is set up in the tissues separating the point of the olecranon from the corresponding epidermis, it becomes propagated with the greatest ease towards the arm and fore-arm, meeting with cellular tissue, which is abundant, loose, and vascular, in proportion to its distance from the point to which the inflammation was first limited. When the solution of continuity comprises the thickness of the integuments, it is rare for the bursa to escape participating in the inflammation, whence arises a phlegmasia, which, though in itself nowise serious, leads to an unfavourable prognosis, inasmuch as it may give rise to the denudation of the olecranon, or an extension to the joint. The conformation of the part also necessarily adds to the gravity of its injuries. When the elbow in a fall forcibly presses against a resisting plane, the bony edge of the olecranon divides the tissues from within outwards, in such a manner that their attrition extends farther on the deep-seated than at the superficial parts. The wound is thus narrower at its external aperture than in the rest of its extent, and under the influence of

tumefaction this aperture before long disappears. Pus, however, forms, and is usually of bad quality, its presence being almost always announced by its mischievous effects upon neighbouring parts.

VIII. *On Extraction of Cataract at a Single Stroke.* By M. CHASSAIGNAC. (Moniteur des Hôpitaux, No. 74.)

M. Chassaignac observes that several oculists, and especially Wenzel, have extracted cataracts at a single stroke; opening the capsule as the knife traversed the anterior chamber on its way out. This procedure, which has hitherto been justly considered as an exhibition of a somewhat dangerous dexterity, may be very well accomplished by the aid of chloroform. It offers, indeed, great advantages; for, whatever may be the dilatation at the moment of commencing the operation, it contracts immediately after the escape of the aqueous humour, and the knife introduced subsequently may easily wound the iris. In this operation, the pupil remaining wide open, the accident is not to be feared. In other respects, however, Wenzel's operation was really dangerous, and presented difficulties well nigh insurmountable without the aid of chloroform.

M. Chassaignac habitually employs chloroform in his operations for cataract, and the advantages he has derived from it he thinks are due to the observance of the proper principles that should regulate its employment. No patient can be operated upon with security if he has not been brought to the stage of tolerance, that he sleeps deeply and placidly, without irregular movements, restlessness, cries, or delirium. His respiration is normal, his *facies* excellent, his pulse large and full—presenting, in a word, an assemblage of conditions not only well suited to tranquillize the surgeon, but also eminently fitted to facilitate the execution of the operation. Vastly different is it to operate upon an eye rendered quite immovable, and to act upon one essentially mobile, and incessantly seeking to escape from the action of the instruments. Among the accidents thus avoided, is wounding the hyaloid membrane. It is in fact almost always to this accident, and not to the pressure exerted upon the globe of the eye, that is due the escape of more or less of the vitreous humour after cataract operations. This almost inevitable accident, when operating without chloroform, is easily avoided in the immovable state of the eye. The same may be said of wounds of the iris, and most of the accidents consequent on extraction. The possibility of producing vomiting has been urged as an objection to the use of chloroform in this operation, but, by waiting before commencing the operation until the period of tolerance has been reached, M. Chassaignac has never met with vomiting or struggling.

IX. *Case of Extrusion of a Calculus through a Fistulous Opening.* By Dr. TOSCANO. (Wiener Wochenschrift, No. 28.)

N. P., aged sixty-three, had suffered since his eighth year from difficulty in passing urine, and to avoid punishment on account of enuresis, was in the habit of tying up the penis at night. Becoming a soldier, he experienced the severest suffering from the urine while on march, and a swelling often formed at the upper part of the right side of the scrotum, discharged pus and blood, and then healed again. He would have painless intervals of about three years at most, when the urinary fistula would again appear; and, after continuing awhile, cicatrize over again. He was discharged from the army. Some weeks before he was seen by the author, an urinary fistula formed in the right pubic region, all the urine passing through it, and this time it did not heal, as usual. The opening kept getting larger and larger, and a rough, hard body could be felt at its bottom. This, after violent efforts, was discharged, and proved to be an urinary calculus. It weighed 10 drachms, was pyriform in shape, having a stalk-like process, and measured 2 inches 7 lines in its long diameter, and 1 inch 5 lines in its transverse.

Its chemical composition for the most part was oxalate of lime, with some mucus, uric acid, and magnesia. The patient had often been examined professionally, and was supposed to be suffering from schirrous degeneration. The calculus was discharged on the 2nd of June, and, writing about a month after, Dr. Toscano states, that for his age the patient looked very well, and all the functions were performed with regularity. A small, clean, superficial ulcer, with funnel-like edges, and indurated circumference, existed still in the right pubic region, through which a small quantity of urine was discharged. He would not allow any examination of the urethra to be made.

X. *On Extirpation of Enlarged Glands.* By M. Chassaignac. (Moniteur des Hôpitaux, No. 75.)

M. Chassaignac does not regard as inoffensive to the economy, the presence of more or less large masses of suppurating or tuberculous glands, as in the submaxillary or cervical regions. In place of treating them by resolvents, the effects of which are very uncertain, and always very slow, he cuts down upon them, and enucleates them. If their base is too strongly adherent to subjacent parts, or he fears lest he might open some vessel during the dissection, he applies a ligature around the base, and over this his *écraseur*. He does not think it essential to remove the whole of the ganglionic mass; for it is not with such tumours, as with those of a cancerous nature, that we cannot leave the smallest morsel without fearing it may become the seat of reduced development. He only removes as much as can be got at with ease, and without any laborious dissection, even supposing a new ablation may have to be performed.

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D. (Lond.)

LETTSOMIAN LECTURER ON MIDWIFERY, ETC. ETC.

I. Physiology and Pathology of the Unimpregnated State.

1. *Case illustrative of the Age at which Puberty occurs in Eurasian Females.* By W. H. Ross, M.D. (Indian Annals, April, 1856.)
2. *Case of Fatal Bleeding into the Peritoneal Cavity through Rupture of the Ovary.* By M. Demarquay. (L'Union Médicale, 1855.)
3. *Operation of Ovariotomy successfully performed.* By Ezra P. Bennett, M.D., Conn. (Amer. Jour. of Med. Science, April, 1856.)
4. *On the Operative Treatment of Ovarian Cysts, and especially on the use of Iodine Injections for the Radical Cure of Ovarian Dropsy.* By Dr. Fock. (Monatschr. für Geburtsh., Mai und Juni, 1856.)
5. *On the Removal of a Foreign Body from the Canal of the Cervix Uteri.* By Synd Mahomed Jaun. (Indian Annals, April, 1856.)
6. *A hitherto undescribed Disease of the Uterus, namely, Unnatural Patency of the inner extremity of the Fallopian Tube.* By Dr. Matthews Duncan. (Edinb. Med. Jour., June, 1856.)

1. Dr. Ross's case is illustrative of the age at which puberty occurs in Eurasian females. "E— is about 4 ft. 10 in. high, her body is considerably developed, and each breast as big as a split orange; the nipple is not so well formed as the breast; there is hair under the arm-pits and on the mons veneris nearly half an inch long. The girl is of a very modest character, and very retiring. She began to menstruate on the 15th March, 1856, and cried very much when she found her clothes covered with blood. She had an attack of fluor albus six months ago; and more recently, an attack of severe headache and fever. Her father was a fine European, and her

mother an East Indian. The girl's age was satisfactorily proved to be eleven years and nearly five months. The case excited *alarm* in her mother, who never remembered an instance of such early menstruation before.

2. M. Demarquay's case of rupture of an ovary and fatal hæmorrhage into the peritoneum, we record as a valuable contribution to an affection not yet thoroughly cleared up—namely, retro-uterine hematocele. A washerwoman, aged twenty-seven, of sound constitution, of regular menstruation, was seized, without obvious cause, with hæmorrhage of the genitals, which persisted for two months, and greatly reduced her. She was admitted into hospital. Examination revealed fungoid ulceration of the portio vaginalis uteri, which, after applications of actual cautery and nitrate of silver, disappeared together with the hæmorrhage, when the patient suddenly complained of pains in the abdomen and lumbar regions, could eat nothing, and had strong fever. These sufferings increased; the abdomen became very sensitive; vomiting of soapy matter followed; acute pain in the right side, hiccough, anxiety, tympanitis. Death on the second day.

Autopsy.—Skin white, as in persons who have died of hæmorrhage. In the abdominal cavity a considerable effusion of blood; intestines distended with gas; peritoneum injected. In the pubis, about a quart of fluid, black, partly coagulated blood. The omentum investing the pelvic organs was covered with a soft, recent, red-brown membrane. Uterus normal, but plastered over with old adhesions. Fallopian tubes inflamed, the canals obstructed by little abscesses. Left ovary swollen, softened, fragile, greyish, and infiltrated with pus and plastic matter. In the right ovary, which was still more diseased, there was found a laceration in the direction turned towards the Douglasian space, surrounded with black clots of blood. The ovary dissected showed a largely-developed Graafian vesicle filled with blood, and near this an empty one of considerable circumference. All the rest of the tissue was softened by inflammation.

3. Dr. Bennett, of Danbury, Conn., relates briefly a case of ovariotomy terminating successfully. There are no circumstances calling for detailed report. The incision made was only three inches; there were no adhesions; the sac drawn out, was opened first with a trocar, then with a knife, the patient having been turned over on her face. The sac emptied, a double ligature was passed through the pedicle, which was then cut through. The patient is said to have recovered without a single unpleasant symptom. The operation was performed on the 12th January of this year. It will be desirable to have a further report.

4. Dr. Fock, in a very elaborate article, reviews the results of the various methods of treating ovarian dropsy. He adds 130 cases from various sources to those collected by Dr. Robert Lee. Of the gross number, 292 in which ovariotomy had been attempted, the operation could not be completed in 92 on account of errors of diagnosis; and of these, 31 died in consequence of the attempt. The gross result is as follows:—Of the 292 cases partly attempted, partly carried out, there were 120 deaths, 120 radical cures; 52 recovered, but were not freed from their original disease. Thus, $41\frac{7}{73}$ per cent. were cured; $41\frac{7}{73}$ per cent. died; $17\frac{59}{73}$ were uncured; or, if we add together the two last classes under the common title "unsuccessful," we have $41\frac{7}{73}$ per cent. successful, and $58\frac{66}{73}$ unsuccessful.

Dr. Fock also gives in detail and in tabular form 15 cases of ovarian cysts treated by iodine injections, including the original ones of Boinet. In summary we find that of 15 cases, 9 attained a radical cure without after trace of relapse. Of these 9, 4 were healed by *one* injection, 2 by *two* injections, 1 by *three* injections, 2 by *six* injections. In the remaining 6 cases, repeated injections had no effect, and an elastic catheter was left in the cyst, and iodine injections made as occasion required. Of these 3, 2 were healed; 1 died of pyæmia occasioned by suppuration in the cyst. There thus remain 3 cases; in 2 of these, through an error in diagnosis, the iodine injection was made into cysts complicated with car-

cinoma of the ovarium, and life was probably shortened by the operation. In the last case, the fluid collected again after a single injection.

[In the discussion upon Dr. Fock's paper in the Berlin Obstetrical Society, one or two other successful cases were mentioned. The list of continental cases is far from complete; and a considerable number of iodine injections of ovarian cysts must now have been made in this country. The reporter himself has recently injected four ounces of undiluted tincture of iodine into an ovarian cyst with good effect; but the observation is not yet complete. It is desirable to collect the fullest information upon the results of this method of treatment, which seems to hold out the promise of greater success and less danger than any other hitherto pursued.—Rep.]

5. The case of Synd Mahomed Jaun is a singular example of a practice pursued with the view of promoting fecundation. The woman who was the subject of operation stated that she had been frequently taking different sorts of native medicines in hopes of becoming a mother. A few days ago, she said, a *machin* (synonymous to a *dai* of Bengal) advised her to introduce a bit of *katore fruit*, cut in a certain shape and of a certain length, into the canal of the cervix during menstruation, and to have sexual intercourse after a certain length of time, having of course previously removed it. Agreeably to this instruction, she said, she had introduced a bit of it some fifty hours ago, and had failed in her attempts to remove it. It had caused considerable inflammation, and on this the author was called in. The foreign body extracted was of a dirty brown colour, soft and friable. It was more than three quarters of an inch long, and about the thickness of a goose-quill. The *katore* is said to be a kind of nut, trees of which are found in the Bhawulpore district.

6. Dr. Matthews Duncan describes a case of unnatural patency of the Fallopian tube as a hitherto unrecognised morbid condition of the uterus. He quotes instances from Morgagni, Tyler Smith, and others, showing that this patency has been observed after death, or inferred to exist during life. The evidence on which Dr. Duncan rests his diagnosis of this condition is Fallopian catheterism. He relates two cases where patency was thus inferred from the circumstances that the probe passed easily through the uterus towards the right side for eight inches in one instance, and six or seven in the other; and that the end of the probe could then be felt through the abdominal walls. [Admitting that in certain cases this patency exists, it must still appear that it is not a disease *per se*, but rather a consequence of disease of the Fallopian tube.—Rep.]

II. Pregnancy: Physiology and Pathology.

1. *A New Mode of diagnosing Early Pregnancy.* By Dr. Keiller. (Edinb. Med. Journ., June, 1856.)
2. *Note on a little-known Cause of Vomiting in Pregnant Women.* By Dr. René Brian. (L'Union Méd., July, 1856.)

1. Dr. Keiller speaks favourably of an instrument modified from the metroscope of Nauche, for diagnosing early pregnancy. The difference of Dr. Keiller's instrument consists in the different construction of the *uterine stethoscope*, which is introduced per vaginam, and thus applied not to the os or cervix, but to the walls of the uterus. It is contended by Dr. Keiller that by this instrument the auscultatory signs of pregnancy may be detected at a much more early period than by the usual process of external auscultation. [Dr. Montgomery, in the recent edition of his great work 'On the Signs of Pregnancy,' does not speak well of the metroscope.]

2. Dr. Brian records a case of obstinate vomiting in a pregnant lady, which he saw in consultation with M. Moreau. After a long course of therapeutical means had failed, a vaginal examination revealed a retroverted condition of the womb. When liberated and allowed to rise out of the pubis, the vomitings ceased, and pregnancy went on favourably.

III. Labour.

1. *Carbonic Acid as a means of inducing Premature Labour.* By Scanzoni. (Wiener Med. Wochenschr., Nr. 11, 1856.)
2. *Case of Abnormal Labour: Presentation of the Abdomen; Escape of the Intestines of the Fœtus through its Umbilicus.* By M. Penjon. (L'Union Médicale, July, 1856.)
3. *Central Laceration and Transit of the Fœtus through the Perineum.* By John F. Lamb, M.D., Frankfort, Pa. (Amer. Jour. of Med. Science, April, 1856.)
4. *On Stricture of the Uterus.* By Dr. Lehmann, of Amsterdam. (Nederl. Tijdschr. v. Geneesk. 1855.)
5. *Two Cases of Rupture of the Uterus.* By Dr. Mangold, of Cassel. (Monatsschr. für Geb., Juli, 1856.)
6. *Death of the Fœtus caused by Torsion of the Umbilical Cord.* By Dr. Hafner. (Monatsschr. für Geb., Juli, 1856.)

1. Scanzoni, moved by the observation of Brown-Séquard, that carbonic acid irritates the smooth muscular fibre to contraction, and convinced of the insufficiency of his method of exciting labour by suction of the breast, has sought in carbonic acid a new means of exciting labour-pains. In a very small primipara, aged twenty-six, premature labour was indicated by contraction of the pelvis. She was in the 32nd-34th week of gestation. The portio vaginalis was five to six inches long, tolerably resistant; outer os uteri fast closed; the head floated; the fœtal pulse faintly heard. On the 2nd February, the first application of twenty minutes without subjective or objective alteration.

3rd February, eight A.M., for twenty-five minutes, and in the evening, thirty minutes. During the injection, prickling in the vagina; during the day often stinging in the region of the umbilicus; in the evening the portio vaginalis was loosened. The stingings were renewed in the night.

4th Feb. Morning and evening, each time half an hour. Prickling in the vagina. In the course of the day, the os uteri admitted the finger through, and the presenting head could be reached. In the night labour-like pains, and towards morning lively contractions of the uterus, which, later, ceased.

5th Feb. Prickling during the thirty minutes of the injection. The os was opened, yielding, dilatable. Increased vaginal secretion. About noon, painful persisting contractions; about half-past six P.M., rupture of membranes; seven P.M., birth of a living child over three pounds weight. Slight metrorrhagia, which ceased after the removal of the placenta. Recovery good.

Apparatus.—A glass vessel, holding a quart, is fitted with an air-tight cork-stopper, in which are two openings. Through one opening runs a glass tube provided with a funnel; through the other runs a horn tube fitted with an elastic tube three feet long, which ends in a bent uterine tube. Two tablespoonfuls of bicarbonate of soda, dissolved in twelve ounces of water, and a little vinegar, serve to supply the carbonic acid. A conical glass speculum and the uterine tube are introduced into the vagina, the tube being surrounded by a cork filling up the speculum, so as to retain the carbonic acid in the vagina.

2. The case of M. Penjon is rare, if not unique. A woman, aged forty, primipara, came under his care in labour, on the 4th March, 1855. The child was ascertained to be alive by the cardiac sounds. A transverse presentation was ascertained. After some hours, the patient sent for M. Penjon, saying something

soft and extraordinary had escaped from the vagina. A loop of intestines was recognised. A colleague concluded, after examination, that the intestines proceeded from a rent in the vagina of the mother. Turning being decided upon, and the hand introduced for the purpose, the umbilical presentation was detected; the feet were directed to the left near the fundus, the head to the right, and more elevated than the feet; the maternal organs were intact. The placenta came away spontaneously. With the exception of the umbilical tumour, the fœtus, which was of the female sex, offered nothing unusual; it was well developed, and about fifty centimètres long. The tumour, which M. Penjon was not allowed to dissect, was formed by the intestines, liver, spleen, and no doubt the stomach, seated in the umbilical cord.

3. Dr. Lamb records an example of that simple accident, central laceration, or perforation of the perineum. It occurred in June, 1821, in a primipara, aged thirty. The occiput presented to the right sacro-iliac synchondrosis. For some hours the parts seemed unyielding. The infant seemed to have "jumped through the perineum!" A single pain had expelled the child. The fourchette and sphincter ani were uninjured. The patient was kept on her back, thighs in apposition, for some days. The wound healed by first intention. She has since borne several children.

4. Dr. Lehmann gives a minute analysis of the forms of irregular uterine contraction, under the name of stricture of the uterus. Stricture of the uterus, he says, a partial tonic spasm, happens almost exclusively in the direction of the transverse fibres, especially in those places where the circular fibres predominate, at the lower part of the body of the womb, at the os internum and os externum uteri, and in the neighbourhood of the Fallopian tubes.

Tonic spasm of the lower part of the body causes a ring or band-like contraction more or less broad, which, when well developed, can be felt through the abdominal walls. The uterus seems to be irregularly shaped lengthwise, reaches to the pit of the stomach, and is often divided into two unequal halves, so that the lower half is separated like a strongly distended urinary bladder. The introduction of the hand makes the diagnosis clearer; mostly, a small, smooth, defined line, stronger in front, can be felt. The spasm-affected part is very painful to the touch, and the pain, as well as the contraction, remains beyond the labour-pain. The conclusion is erroneous, that these strictures happen only in the fifth stage; they are observed nearly as often in the third, and frequently they arise first when the head and shoulders are behind, or when, in a breech presentation, the breech is partly born. This condition is known by the following marks:—in spite of strong pains the presenting part recedes, and although no obvious obstruction exists in the pelvis. When head presents, it is found that the pains do not drive it down on the os uteri, showing that the obstruction does not lie here, but higher up. If, in such a case, through wrong diagnosis, forceps be used, the blades pass readily through the os uteri, but in pressing deeper strike upon an obstacle which cannot be overcome without great pain. If the application of the forceps be accomplished, it is found on each extractive effort, that the uterus is dragged down too. If turning is tried, the stricture is much more easily recognised. The hand easily penetrates the cervix uteri, but the fingers are with difficulty squeezed between fœtus and uterus, and if a smarting hand be passed through it, it is quickly paralysed by the compression. This stricture is more easily known in the fifth stage, when the fingers may be passed to the contracted part, and the hour-glass form recognised.

The *spasmodic stricture of the inner mouth of the womb* is most frequent in the fifth stage, but rare during the extrusion of the child. It is recognised by the same signs, only nothing can be felt through the abdominal walls; on the other hand, it is easily detected by vaginal examination. The os uteri externum may be flaccid and widely open, without trace of contraction.

The *spasmodic stricture of the os uteri externum* is the least dangerous form. Early in labour, it is clear to the observer that the pains are irregular and painful, the patient is restless, and uncomfortable. The strong boring pains are felt chiefly deep in the hypogastrium, and spread towards the sacrum and thighs in the course of the ischiatic nerve. The vagina is mostly hot, dry, and sensitive; the os uteri remains deep in the pelvis, and has very thin sharp edges, which, even in the interval of contractions, are stretched like a cord. The presenting part lies firmly on the lower segment of the womb, and even if a little engaged in the os uteri, a large swelling is caused by the pressure of the stretched edge. The sphincters in the neighbourhood often share in the spasm, and dysuria and tenesmus follow; or there arises, through the extension to the other nerve-spheres, hiccough, vomiting, cough, anxiety, and oppression; in the highest degree, the central nervous system is even affected; syncope, headache, delirium, sopor, convulsions, whilst, although the pains may continue, what is called metastasis takes place.

The *spasm of the mouths of the Fallopian tubes* only occurs in the placental stage. The womb assumes a remarkably oblique shape, as if the affected part had been lengthened out like a horn, as can be felt as well as seen.

Uterine strictures, when they occur, must be regarded as consequences of irregular contraction; but they may exist, according to Lehmann, as a real alteration of tissue at the affected part; this may have arisen before the beginning of the stricture, or may follow, upon a long-continued contraction, through which stasis in the uterine vessels, hyperæmia and inflammation of the whole organ may be easily developed. Not seldom it is observed that an ordinary clonic spasm passes gradually into a stricture, especially in the lowest part of the womb. Still more frequently it happens that, in a clonic spasm, at the time when the child is extruded, an uterine stricture follows in the fifth stage. But the predisposition to stricture often rests in the uterus, as in hyperæsthesia, through a rheumatic or inflammatory action, when an alteration of tissue may have existed during pregnancy. Besides this, strictures may arise, or become worse, through bad presentations of the child, after premature escape of the liquor amnii, through irritation of the os uteri from the frequent exploration, or instrumental interference.

Stricture of the uteri always perverts the course of labour, but naturally according to the degree, duration, seat, and stage of labour. The effect of the compression is also severely felt by the child. Bruises are sometimes seen, and asphyxia; the liver has been known to have burst. The uterine walls may be rent.

Lehmann advises to bring about retraction of stricture by venesection, opium, warm baths, belladonna injections. He has found ether and chloroform without influence.

5. Two cases of laceration of the uterus, related by Dr. Mangold, serve to illustrate the etiology of this accident:

Case I.—A woman in her eighth pregnancy. Had been delivered on a previous occasion by turning. For four weeks she had, without apparent cause, suffered a loss of coagulated blood; and for the same time, almost every night, labour-like pains. When seen, regular pains had set in; the membranes had burst half an hour, but very little water escaped. The midwife could feel no presenting part, nor even the os uteri. Outward examination revealed an extraordinarily stretched abdomen, enlarged transversely; fundus uteri somewhat lower than usual, and no part of the child. Inward examination: a varicose vagina; uterus easily reached, but no os, and no scar to indicate an obliteration of this part. Powerful pains went on. Case left to nature. Pains became even more violent; and when, after three hours from first visit, Dr. Mangold saw her again, he found the following condition:—The patient lay still, and without pain; face pale, cold, and collapsed; pulse almost imperceptible; extremities cold. Outward and inward examination

showed no change. The pains had suddenly ceased, and this condition followed. Death ensued shortly, caused by loss of blood.

Autopsy.—The muscular wall of abdomen remarkably thin. A rent in the uterus, six inches long, in direction of the linea alba. This rent had penetrated the substance of the uterus, but not the investing peritoneum, so that there was no escape of the child or liquor amnii. The os uteri was a cross-fissure, open to one inch, and squeezed tightly against the abdominal wall, above the pubic symphysis. The child lay crosswise, was well developed, weighed eight pounds, and was twenty-four inches long. There was great effusion of blood in the uterus.

CASE II.—A woman, aged twenty-four, of middle size, had menstruated with great pain at sixteen, a condition which lasted until her pregnancy. During the first months of gestation, her sufferings were increased, and vomiting superadded. These subsided at about fourth month. About the twentieth week, she awoke with acute pain in lower belly. When seen, she lay drawn up on her right side; face pale, sunken; look wild, hands cold, no pulse. She complained of pain in the whole right side; numbness and formication in the right thigh. Examination external and internal revealed nothing remarkable; os uteri quite closed, as at this period of gestation. After half an hour, pains came on in the hypogastrium, grew gradually stronger and stronger, and reached a frightful intensity. Slight convulsions affected the upper part of the body, and death followed.

Autopsy.—On opening the abdomen, an extravasation of coagulated blood was found. In the right side of the hypogastrium, near the pubic arch, was found the fœtus, partly covered by blood-extravasation and folds of intestine: well-formed, male, of the size natural to its age, fresh and red, showing no sign of anæmia. The uterus rose a few inches above the pubis. In its fundus was a rent, running from before backwards, and somewhat to right, of two inches and a half long, partly filled by the placenta. The structure was remarkable. At the neck and lower part of the body, the consistence was natural; upwards it was thinner; and at the upper part and fundus, the body of the uterus was quite membranous. This whitish membrane, which was covered internally by the remains of the decidua, had scarcely the thickness of stout paper. The placenta, which was seated upon the lower and more fleshy part of the organ, had not been detached: it was very vascular.

6. Dr. Hafner has twice observed death of the fœtus and abortion caused by tortion of the umbilical cord obstructing the placentar-fœtal circulation. The operation of these structures is, he says, clear: on the one side the fœtus gets a constantly-diminishing supply of blood in relation to the wants of growth; on the other side, the placental circulation is obstructed backwards, and congestions are occasioned, which are relieved by hæmorrhages into the uterus, and thus lead to abortion.

IV. PUERPERAL STATE.

1. *Report of Twenty-seven Cases of Puerperal Peritonitis.* By ROBERT K. SMITH, M.D. (Phil. Med. Examiner, April, 1856.)
2. *Puerperal Tetanus, in 'Notes on some of the Diseases of India.'* By EDWARD J. WARING. (Indian Annals, April, 1856.)

1. Dr. R. K. Smith relates the histories of twenty-seven cases of puerperal peritonitis, constituting an epidemic that raged in the obstetric department of the Philadelphia Hospital from December, 1855, to February, 1856.

The following is a concise summary of the facts:—Previous to the appearance of puerperal fever there were "a number of cases of erysipelas in the hospital;" there were still other cases after the fever had ceased. There were also seventeen other obstetrical patients who occupied the same wards, many of them adjoining beds to the fever patients, that escaped an attack.

After several of the first cases occurred the remaining pregnant women were removed to wards not previously occupied by obstetrical patients; and some of them were transferred to rooms in the medical department immediately upon their admission. This department is in a separate building. These removals had no effect in arresting the progress of the disease; it followed the patients wherever they went, until no more pregnant women were admitted.

Nearly every child of women having puerperal fever died in convulsions.

Of 29 cases (including 2 not reported), 12 recovered. *Symptoms.*—The symptoms, as derived from the details of the cases, generally were:—Chills within about forty-eight hours after delivery; pains in lower belly, pain on pressure, face flushed, skin hot, headache, thirst, pulse small, quick, 110 to 130 or 140. Milk and lochia not generally arrested. Diarrhœa, vomiting, tympanites, hectic, followed. Dorsal decubitus, legs drawn up. In some cases, inflammation of distant organs, fine crepitation of lungs, pleurisy. Sometimes delirium (in one case maniacal excitement), stupor. Several of the patients were quite anæmic, but this was the result of the rigorous antiphlogistic treatment pursued.

Treatment.—The most resolutely antiphlogistic. Bleeding at onset to twenty, thirty, or forty ounces; sometimes six dozen or two hundred leeches to abdomen; calomel and opium, in some cases to salivation; hot poultices to abdomen; enemata of castor oil and turpentine when tympanitic.

Post-mortem Appearances.—In one case the marks of inflammation were so slight as to prove that inflammation was not the essential primordial condition of the disease. In others, the results of inflammatory action were obvious. Inflammation of the uterus, and a condition of the cavity which the author calls "gangrenous," peritonitis sometimes slight, in some cases extensive and attended by effusion, mostly purulent; plastic exudation is not specified.

(The cases prove that the epidemic was of a septic character, not freely inflammatory.)

2. Mr. Waring's notes on puerperal tetanus are of especial interest. He says, "Of this very obscure form of the disease little is apparently known; and this is not a little remarkable, as from its extraordinary frequency at Bombay it cannot but repeatedly have come under the observation of medical officers at that Presidency. In the three years ending December, 1853, no less than 232 women are recorded as having perished from this malady; and it would appear as if it had been on the increase, as from 38 deaths from this disease in 1851, we rise to 87 in 1852, and 108 in 1853." In a table, Mr. Waring shows the cause of this increase more in detail, month by month in the three years. "A considerable increase," he observes, "of mortality took place towards the close of the rains, 30 per cent. of the number dying in the three last months of the monsoon (August, September, and October); whilst in the three months immediately preceding the setting in of the rains (February, March, and April), only 15 per cent. of the deaths occurred. Dividing the year into two seasons, the wet and the dry, we find the proportions to stand thus:

Wet season (May to October inclusive), 127 deaths, or 54·4 per cent. Dry season (November to April inclusive), 106 deaths, or 45·5 per cent. Great humidity of the air (such as exists in Bombay during the rains) seems, therefore, to increase the mortality from this disease, though not in any very marked degree.

The frequency of this disease in Bombay is rendered the more remarkable by the comparative rarity of other fatal puerperal affections; during the same period the whole number of deaths by puerperal fever being 21, by puerperal convulsions 2, by phlegmasia dolens 1.

The period of accession of the tetanic affection in the 233 fatal cases—a most nteresting feature—is shown in the following table:

First day .	7, or 3·00 per cent.	Eleventh . .	2, or 0·85 per cent.
Second .	32, or 13·73 „	Twelfth . .	9, or 3·86 „
Third . .	29, or 12·44 „	Thirteenth .	4, or 1·71 „
Fourth .	23, or 9·87 „	Fourteenth .	1, or 0·42 „
Fifth . .	22, or 9·44 „	Seventeenth .	1, or 0·42 „
Sixth . .	32, or 13·73 „	Eighteenth .	1, or 0·42 „
Seventh .	15, or 6·43 „	"Soon" .	10 } or 5·15 „
Eighth . .	14, or 6·00 „	Not stated .	2 }
Ninth . .	15, or 6·43 „		
Tenth . .	14, or 6·00 „	Total . .	233

It thus appears that up to the eighteenth day after delivery the patient is liable to the supervention of tetanus, although the liability greatly diminishes after the sixth day; the number dying during the first six days amounting to 145, leaving only 88 to be distributed over the remaining twelve days.

Trismus nascentium does not appear to be a frequent, or at any rate a fatal, disease in Bombay; during the above three years only six deaths being recorded under this heading.

Mr. Waring shows, in another part of his most valuable paper, that during the same three years there died out of 42,651 (the total deaths in the town of Bombay), 912 from tetanus: thus there was 1 death from tetanus to 46 from all causes. This enormous mortality from tetanus is the more remarkable when it is borne in mind that the puerperal form is excluded in this enumeration.

MEDICAL INTELLIGENCE.

The Eastern Hospitals.

The magnificent establishments for the sick and wounded which the English army formed at Balaklava, Therapeia, Scutari, and Renkioi, are now among the things that were. The Crimea was first emptied of its sick, and the Castle and Monastery hospitals were dismantled and broken up. Renkioi was cleared out in June, and Scutari in July. The amount of stores collected at Scutari to supply the several hospitals was enormous: the deficiency of 1854 had led the Government to form a store *dépôt* which could not be exhausted by any emergency. Great quantities of these stores were sold, and only those articles which did not find a ready sale were sent home. The loss on the forced sale must be very considerable, but such losses are among the necessary consequences of war.

The wooden houses at Scutari have been sold by auction; and those at Renkioi have, we understand, been purchased, with the land on which the hospital stood, by a Greek firm.

Whatever may have been the failures in the hospital department at the commencement of the war, failures consequent upon the unprecedented nature of the campaign and the want of early information given to the medical department, there can be no doubt that, at the close of the war, no army ever had such field hospitals in the front, or such perfectly-organized secondary establishments in the rear. Scutari had been made as perfect as the nature of its buildings would permit; and Renkioi, devised by Mr. Brunel, was a model of comfort and convenience.

The perfection of the medical arrangements is shown most strikingly by the almost incredible healthiness of the English army during the winter of 1855-56. The public have already learnt that the losses of the French paralleled, or more than paralleled, those of the English army in the previous winter. It has been supposed that at least 20,000 French died in the five months from November to March. Eleven per cent. of the Sisters of Charity in the French hospitals at Constantinople died of typhus alone. We have no means of knowing what was the

loss of this class in the Crimean ambulances. The loss of medical men was also so great that aid was obliged to be given from the English hospitals at Scutari.

It is not so well known that the losses of the Russians, after the taking of Sebastopol, and when there was a cessation from actual warfare, were still greater than those of the French. Not only among the garrison of Sebastopol, but in the distant camps of Bakshiserai and Simpheropol, where no hostile shot was ever fired, and at Odessa, typhus fever prevailed with extraordinary intensity. Scurvy, also, was most widely diffused; and we have been informed that no scorbutic patient attacked with typhus was known to recover. It is impossible to know the numerical loss of the Russians, but it may reasonably be conjectured to have been proportionably much greater than that of the French. In fact, there can be little doubt that the Russian army was most seriously lessened in number, and that the first movements of the Allies would have shown its weakness.

Now by the side of these facts, first that the French army was so reduced that even its numerical superiority over the English would not have been great in the campaign which the peace has stopped; secondly, that the Russian army was so decimated by disease that it must have fallen back before a vigorous advance—put this other fact, that the English army, placed under similar circumstances and acted on by the same causes of disease, preserved its numerical strength, and would have continued the war with men in the highest health and vigour. So struck were the Russians with this difference, that, at the proclamation of peace, their medical chief applied, we have been told, to the English authorities, for a copy of the regulations and customs which had preserved the English from the visitation which had fallen so heavily on themselves. In fact, the effect of these regulations would have influenced the campaign of 1856; and it is not too much to say, that the freedom of the English from disease during the last winter would have had a vital effect on the progress of the offensive operations.

If, then, there was a momentary failure in the medical department, they have nobly redeemed it. We do not know to whom the wise arrangements to which we have referred were owing, whether to Sir John Hall or to Dr. Sutherland, the excellent sanitary commissioner. Probably it was a conjoint operation, but we trust that the Government will take care to reward the men, to whom would have been owing, far more than to any general, the success which, according to all human foresight, would have signalized the campaign which the peace cut short.

Military Sanatoria.

A PLAN has recently been promulgated by Dr. Pincoffs, late of the Civil Staff in the East, and is now under the consideration of the authorities, of establishing sanatoria for invalided soldiers. The sanatoria are intended to be in connexion with thermal springs, at the sea-side, or at some watering-place likely to be conducive to the benefit of the wounded or diseased. The proposition is only novel as regards England. Abroad, in Germany, France, and Italy, for instance, such institutions have long since existed, and have been shown to bear excellent fruit. If we express ourselves in favour of the scheme, we do it not because it is theoretically plausible, but because similar systems have been extensively tried, and have long been a part of the military organization of other countries. We cannot now go into the numerous questions which suggest themselves in connexion with this topic; but as the general interest in all matters connected with the great struggle in which we have lately been engaged will soon subside, and this thing, if done at all, must be done soon, it is right that the profession should be aware of the proposal, and know something of its merits. Nothing will prove more conclusively the value which our French friends place upon their watering-places than the simple fact, that at six different places of the kind, they possess military sanatoria or hospitals. We give their names, with the main constituents and the respective temperatures of the waters; Amélie-les-Bains, sulphureous, 142° Fahr.; Barèges, sulphureous, 97° Fahr.; Bourbonne-les-Bains, saline, 138° Fahr.; Bour-

bonne-l'Archambault, chloride of sodium, 140° Fahr.; Guagno, in Corsica, sulphureous, 106° Fahr.; Vichy, alkaline, 118° Fahr.

In Piedmont we find a military hospital at the thermal waters of Acqui, which are sulphureous, and possess a temperature of 113° Fahr. Similar institutions are to be found in the well-known watering-places of Carlsbad and Töplitz in Germany.

The beneficial effect of thermal and other mineral waters in promoting the elimination of morbid poisons, by aiding in the metamorphosis of the tissues, or in giving tone to the system, can scarcely be denied. The form with which we are most familiar is the sea-bath. But those who possess an extensive acquaintance with the watering-places of this and other countries are familiar with results of as decisive a character as the seaside produces. The military sanatoria of the class alluded to are found to assist in the recovery from disease and injuries to which soldiers are peculiarly liable. Inveterate rheumatism, diseases of bones, skin diseases, syphilitic affections, are peculiarly amenable to treatment by the thermal sulphureous springs and other thermal waters; whilst wounds and ulcers, with the secondary effects of the various injuries to which the soldier is liable, are often found to undergo a change and amelioration under their influence when ordinary medication was failing to work a cure. We gather from a work by M. Herpin,* containing very elaborate calculations, that the following is the per-centage of cases benefited by this mode of treatment:

Cured	26 per cent.
Relieved.	48 per cent.

Thus, 74 per cent. of cases which had passed through the ordinary hospital treatment were decidedly benefited; cases which, but for the sanatoria would probably have been allowed to linger out their lives as hopeless invalids. It is to be especially remarked, that the most favourable per-centage is found among the rheumatic cases, which nearly regards us as the most rheumatic nation in the world.

It is, then, a very proper matter for the consideration of our military authorities, whether cause can be shown for the establishment of sanatoria for our own troops, either at home or abroad. We are glad to find that the plan has met with the countenance of some parties whose influence can scarcely fail to secure its realization.

BOOKS RECEIVED FOR REVIEW.

Neue Untersuchungen über den feineren Bau des Centralen Nervensystems des Menschen. Von Joseph von Lenhossek. Wien, 1855. pp. 70.

Varicose Veins, their Nature, Consequences, and Treatment. By Henry T. Chapman. London, 1856. pp. 99.

The Microscope and its Revelations. By W. B. Carpenter, M.D., F.R.S., &c. London, 1856. pp. 776.

The Complete Handbook of Obstetric Surgery. By Charles Clay, M.D. London, 1856. pp. 290.

Traité Pratique des Propriétés Curatives des Eaux Thermales Sulfureuses d'Aix-la-Chapelle. Par L. Wetzlar, D.M. Bonn, 1856. pp. 82.

A Treatise on Cancer of the Breast and of the Mammary Region. By A. Velpeau. Translated by W. Marsden, M.D. London, 1856. pp. 293.

Ismeer; or, Smyrna and its British Hospital in 1855. By a Lady. London, 1856. pp. 350.

The Treatment of Aneurism of the Arteria Innominata. By William Wright, M.D. pp. 39. (Reprint.)

A Practical Treatise on Stammering. By J. Ayres Poett, M.D. London, 1856. pp. 50.

Letters to a Young Physician just entering upon Practice. By James Jackson, M.D. Fourth Edition. London, 1856. pp. 344.

On the Fractures of Bones occurring in Gun-shot Injuries. By Dr. Stromeyer. Translated by S. F. Statham. London, 1856. pp. 120.

Report on the Pathology of the Diseases of the Army in the East. London, 1856. pp. 120.

New York Medical Times. Vol. V. No. 8.

Museum Anatomicum Holmiense Sectio Pathologica. Fasciculus I. Holmiæ, 1855.

* Etudes Médicales et Statistiques sur les Principales Sources d'Eaux Minérales de France, d'Angleterre, et d'Allemagne. Par M. le Dr. J. Ch. Herpin (de Metz), Lauréat de l'Institut, &c. Paris, 1856.

Notes of Three Lectures on the Physiological Action of Strychnia. Delivered by George Harley, M.D. (Reprint.)

Histology of the Cholera Evacuations in Man and the Lower Animals. By W. Lauder Lindsay. (Reprint.)

A Treatise on the Cure of Stammering. By James Hunt, M.R.S.L. Second Edition. 1856. pp. 104.

Lectures in Reply to the Croonian Lectures for 1854, of Charles West, of London. By H. Miller, M.D. 1855. (Reprint.)

A Review of the Present State of Uterine Pathology. By J. H. Bennet, M.D., &c. London, 1856. pp. 99.

The Half-yearly Abstract of Medical Sciences. Edited by W. H. Ranking, M.D., and C. B. Radcliffe, M.D. Vol. XXIII. 1856.

The Retrospect of Medicine. Edited by W. Braithwaite. January to June, 1856.

The Epidemic of Yellow Fever in Norfolk, Portsmouth, and Virginia, in 1855. By J. D. Bryant, M.D. Philadelphia, 1856. pp. 89.

Assurance Magazine, July, 1856.

The Indian Annals of Medical Science. No 17, April, 1856.

The Philosophy of the Stomach, or an exclusively Animal Diet is the most wholesome and fit for Man. By Bernard Moncriff. London, 1856. pp. 92.

Tenth Report of the Commissioners in Lunacy to the Lord Chancellor.

Cases of Nerve Disorder. Recorded with reference to the probable operation of Malaria as a cause. By C. Handfield Jones, M.B. (Reprint.)

L'Hydrothérapie comme Moyen abortif des Fièvres Typhoides. Par R. T. L. Diemer, M.D. Paris, 1856. pp. 128.

The Surgeon's Vade-Mecum. By Robert Druitt, L.R.C.P. Seventh Edition. London, 1856. pp. 760.

Remarks on the Lunacy Laws, as also Asylums of Scotland and France. By I. Webster, M.D. (Reprint.)

Statistical Account of Obstetric Cases in private Country Practice. By R. U. West. (Reprint.)

On a Peculiar Power possessed by Porous Media of removing Matter from Solution in Water. By Henry M. Witt, F.C.S. pp. 12. (Reprint.)

The Progress of Preventive Medicine and Sanitary Measures. By A. W. Barclay, M.D. 1856. pp. 85.

A Complete Guide to Government Appointments and to the Civil Service Examinations. By J. C. Hurst. London, 1856. pp. 56.

Norsk Magazin for Lægevidenskaben. x. Bind, 4, 5, 6 Hefte.

Forschungen über Wichtige Naturerscheinungen in einer Reihe von Abhandlungen dargestellt von Hermann Horn, M.D. 2te Aufl. München, 1856.

An Exposition of the Signs of Pregnancy. By W. T. Montgomery, M.D. Second Edition. London, 1856. pp. 706.

A Comparative Inquiry as to the Preventive and Curative Influence of the Climate of Pau, &c. By Alex. Taylor, M.D. London, 1856. pp. 355. A New Edition.

An Account of some Experiments on certain Seaweeds of an edible kind. By John Davy, M.D. (Reprint.)

On the direct Action of Strychnine upon the Spinal Cord. By Geo. Harley, M.D. London, 1856. (Reprint.)

A Practical Treatise on Disorders of the Stomach. By James Turnbull, M.D. London, 1856. pp. 160.

Archiv für Ophthalmologie. Bd. ii. Abth. 1 and 2. 1856.

Army Meteorological Register for twelve years, from 1843 to 1854 inclusive. Prepared under the direction of Brevet Brigadier-General Thomas Lawson, Surgeon-General U.S army. Washington, 1855. pp. 763.

The Medical Profession in Ancient Times. An Anniversary Discourse. By John Watson, M.D. New York, 1856. pp. 222.

The Treatment of Cancerous Disease by Caustics. By Langston Parker. London, 1856. pp. 40.

Annalen des Charité Krankenhauses. Band vii. Heft 1.

The Treatment of the Insane without Mechanical Restraints. By John Conolly, M.D. London, 1856. pp. 380.

Annual Report of the Grant Medical College. Bombay, 1856.

Criminal Lunatics, are they Responsible? A Letter, &c. By J. Russell Reynolds, M.D. London, 1856. pp. 39.

Quinine and Antiperiodics in their Therapeutic Relations. By J. Macpherson, M.D. Calcutta, 1856. pp. 107.

The Causes and Curative Treatment of Sterility. By Augustus K. Gardner, M.D. New York, 1856. pp. 170.

An Introduction to Practical Pharmacy. By Edward Parrish. Philadelphia, 1856. pp. 544.

Bericht über den ärztlichen Verein in Frankfort am Main im Jahre, 1855. pp. 8.

On the Nature and Treatment of Clubfoot. By B. E. Brodhurst. London, 1856. pp. 134.

Addresses to Medical Students, delivered at the instance of the Edinburgh Medical Missionary Society. Edinburgh, 1856. pp. 266.

Lehrbuch der Physiologie fur Akademische Vorlesungen. Von Otto Funke, . M.D Dritte Lieferung, 1856.

Disorders of the Blood. By Julius Vogel, M.D. Translated and edited by Chundar Coomar Dey. Calcutta, 1856.

Fifth Annual Report of the Wilts County Asylum, Devizes. 1855.

Report on the Past and Present Sanitary Condition of Hackney District. By J. W. Tripe, M.D. London, 1856. pp. 28.

Das Normalverhältniss der Chemischen und Morphologischen Proportionen. Von Adolf Zeising. Leipzig, 1856. pp. 1856.

INDEX TO VOL. XVIII.

OF THE

BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW.

END OF VOL. XVIII.